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MEDICAL REVIEW,

FOR JANUARY, 1847.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *An Experimental and Critical Inquiry into the Nature and Treatment of Wounds of the Intestines; illustrated by Engravings.* By Samuel D. Gross, m.d., Professor of Surgery in the Louisville Medical Institute, Surgeon to the Louisville Marine Hospital, &c.—*Louisville*, 1843. 8vo, pp. 219.
2. *Recherches sur l'Emploi d'un Nouveau Procédé de Suture contre les Divisions de l'Intestin et sur la possibilité de l'Adossement de cet Organe avec lui-même dans certaines Blessures.* Par J. A. Gely (de Nantes), m.d. Mémoire addressé à l'Académie Royale de Médecine.

Researches on the Application of a New Method of Suture in Wounds of the Intestines, and on the possibility of inflecting the Intestine on itself in certain Wounds of that Organ. By J. A. Gely, m.d., of Nantes. Memoir presented to the Royal Academy of Medicine of Paris.—8vo, pp. 84.

3. *Die Operative Chirurgie.* Von Johann F. Dieffenbach. Erster Band. Capitel LXII. *Operation des widernaturlichen Afters.—Leipzig*, 1845.

Dr. Gross thinks it necessary to make an apology for the publication of a work on Wounds of the Intestines, and pleads "that our systematic treatises on surgery unfortunately contain little, if anything, that is worthy of reliance; they enter into no details, and some of them do not even allude to the subject." This is unquestionably to a great extent true; and it might have been added that there are few subjects in surgery respecting which both systematic and other writers differ more widely. Dr. Gross needs to make no apology for the present publication; his attention has been directed towards a most proper and legitimate field of research, and his work, therefore, claims an examination which we regret circumstances have prevented us sooner giving to it.

Dr. Gross, with reason, dates the commencement of scientific knowledge respecting wounds of the intestines from the publication of Mr. Travers's excellent work in 1812, stating that he "did not, like his predecessors, limit his inquiries to the human subject, but extended them to the inferior animals;" at the same time Dr. Gross claims priority in this respect to a certain extent for Dr. Thomas Smith. We may just observe that Mr. Travers refers to Dr. Smith's experiments, and that it is not quite accurate to imply that either Mr. Travers or Dr. Smith had no predecessors in this branch of experimental inquiry. Mr. Travers very fully notices the antecedent experiments of Wm. Cowper, Mr. Skipton, Dr. Thomson, and Sir A. Cooper, and alludes to some by Mœbius and Louis. De Blegny, also, in 1682, Dr. Wallis in 1695, Vogel in 1704, Brunner in 1722, Schlichting in 1742, Mr. Watson in 1790, and doubtless others of whom we are ignorant, published experiments and facts respecting wounds of the intestines of animals. The value of those experiments, however, taken even in the aggregate, was but trifling. De Blegny, Brunner, Cowper, and Schlichting did not even dissect the animals they experimented on; and to Mr. Travers unquestionably belongs the merit of having first systematically investigated the subject of wounds of the intestines, and of having put us in possession of any precise and definite knowledge respecting the process whereby such wounds are repaired, whether spontaneously, or with the intervention of art.

Dr. Gross's first chapter, on the "Nature of wounds of the intestines," is divided into four sections, in which he considers the "structure of the alimentary canal," the "nature and extent of the peritoneal cavity," the "symptoms, diagnosis, and prognosis" of wounds of the intestines, and, finally, "their mode of reparation." As regards the three former sections, a few points only require to be adverted to; but the last section demands a more careful examination, which, however, we can most conveniently give to it when considering the chapter on treatment.

Wiseman, La Motte, Garengeot, and, we believe, the older surgeons generally, held that an instrument might traverse the abdomen without injuring its contents, and many modern surgeons are of the same opinion. Dupuytren, for example, states that such an event occurs "very frequently." (Traité des Bless. par Armes de Guerre, t. ii, p. 428.) With this opinion Dr. Gross agrees, while Mr. Travers, as our readers doubtless are aware, strongly maintains the opposite doctrine. Dr. Gross supports his opinion by appealing to the well-known cases recorded by Wiseman, La Motte, Garengeot, Hennen, and others, in which patients recovered rapidly, and without a bad symptom, after the abdomen had been transfixed, and also by adducing, "as strikingly illustrative of the manner in which the intestines glide away from the edge of the knife," certain cases of very extensive wounds of the abdominal parietes, in which the intestines protruded without being injured. As to this latter class of cases, they are quite beside the question, and prove nothing in favour of Dr. Gross's position, or against that of Mr. Travers. The former class of cases, however, are more to the purpose, but they are not conclusive. With respect to them, Dr. Gross, in common with Wiseman, &c., infers the absence of wound of the bowel from the absence of symptoms indicating its occurrence; but that such an inference is unwarranted sufficiently appears from

experiments and cases in the human subject, so well known that we shall not occupy space by referring to them. There are, however, some cases on record, which prove that an instrument can pass among the mass of hollow viscera without wounding them. Dr. Gross refers to one inconclusive case recorded by La Motte, in which the patient recovered; but he does not refer to two other cases of fatal sword wounds of the abdomen, also recorded by La Motte: in one of which, on dissection, the vena cava, in the other, the vena cava and the aorta, were wounded, but in both the intestines were uninjured. It may be remarked that Mr. Travers refers to those two cases when treating of hemorrhage into the abdomen, and says, "in neither instance had the sword wounded the viscera" (An Inquiry, &c., p. 72); yet he elsewhere denies the fact which he admits in the words just quoted. Those two are, so far as we know, the only conclusive cases in point recorded when Mr. Travers, or even when Dr. Gross, wrote; but some which go to establish the same fact have since been published. Mr. South, in his recent translation of Chelius's Surgery (Part v, p. 458), gives an analogous case, in which an iron spike traversed the abdomen, tore the right common iliac vein and deeply indented the third lumbar vertebra without injuring any of the viscera. In the 'Experience' (No. 373, Aug. 1844) we find the following remarkable case: a woman, five months pregnant, fell from a tree on a wooden stake, which entered the inner and back part of the left thigh, and could be felt through the soft parts at the external border of the left quadratus lumborum muscle passing up beneath the false ribs. As the stake had broken off deep in the wound, and could not, therefore, be withdrawn from below, M. Scaruffi cut down on it in the lumbar region, opened the peritoneum sufficiently to admit the hand, and extracted a piece of wood 8½ inches long; his hand was in contact with the intestines, which certainly do not appear to have been wounded; the woman, we may add, perfectly recovered. The case recorded by Mr. Hennen, in which a ramrod penetrated the abdomen from before backwards, and became firmly impacted in the spine, has been very frequently quoted by writers. We suspect, but cannot be certain, that the following supplies the sequel of that case; but be that as it may, it bears on the point under consideration. Dr. Gilkrest mentioned at the Westminster Medical Society (Lancet, 1832, p. 147) a case, if not the same, precisely similar to that mentioned by Hennen, in which the patient, shortly after his recovery, was drowned, and no injury of any of the viscera could be detected on the most minute examination. If this latter case is excepted to on the grounds that a cicatrix might have been overlooked, which, considering the nature of the projectile (a ramrod), we think quite improbable, the preceding cases sufficiently establish that an instrument can penetrate between the intestinal convolutions without wounding them; and, indeed, we think the fact is proved by one of Mr. Travers's own experiments (Exp. D, p. 54), in which, a month after having pushed a catlin to the shoulder in the abdomen of a dog, he could not detect any sign of the intestine having been wounded; whence, instead of the more obvious inference that the bowel had escaped injury, Mr. Travers concludes that a simple incision in the intestine may unite *per se* and escape detection.

We have already admitted that the mere fact of absence of symptoms after deeply penetrating wounds of the abdomen affords no evidence that

the viscera have escaped intact; but as the fact is certainly sometimes so, it seems not out of place to here mention some of the more recently recorded cases of this very remarkable accident. Mr. Ellis (Lancet, 1834-5, vol. ii, p. 756) mentions a case of attempted suicide, in which a sword entered near the navel, and protruded through the integuments very near the spinal column; in a few days the patient recovered, having manifested scarcely a symptom of injury. M. Roy records a case in which an iron spit entered two inches to the right side of the navel, and was so firmly impacted in the bones of the pelvis near the sacro-iliac symphysis, that on endeavouring to extract it while the patient (a boy) lay on his back, he was lifted from the ground; by a little management the instrument was extracted unbroken, and recovery was uninterrupted by a bad symptom. As the first stool, which was passed on the ninth day, was flattened or riband-like, M. Roy thinks this circumstance may probably indicate temporary contraction of the intestine at some point where it had been perforated. (Gaz. Méd. de Paris, 1843, p. 708.) In the same Journal (1845, p. 313) we find the case of a child, aged 14, who fell on an iron rod used by silk-winders, which passed in at the fold of the left buttock, about 2½ fingers' breadth from the anus, and came out a little below, and to the right side of the umbilicus. The boy was walking down stairs when he was met by his mother, who withdrew the instrument; but the circumstances of the wound were fully ascertained by Dr. Bessem, who discharged him from hospital on the twentieth day, without any symptom indicating injury of the intestine having ensued.

Fæcal effusion is, in the opinion of Mr. Travers, and of almost all surgeons since he wrote, an unusual result of penetrating wounds of the abdomen, which can scarcely indeed occur unless "the gut be full and the wound extensive," or unless the escape of fæces is favoured by the extravasation of a considerable quantity of blood, or of air within the abdomen. Dr. Gross regrets that Mr. Travers, "in the experiments which he instituted to illustrate this branch of the subject, as well as in the cases which he has adduced from his own and the practice of others, has not specified the size of the lesion—a matter of such paramount importance that it is only surprising how it could have been overlooked" (p. 11); and he maintains, from the result of eight experiments performed by himself, that extravasation occurs "very frequently, and with great readiness," that it "almost always, if not invariably," follows "wounds of the bowel to the extent of six lines, whether transverse, oblique, or longitudinal," but that it does not in the majority of cases result from wounds not exceeding four lines in length (pp. 9-10). Dr. Gross's experiments on this point are, however, open to the serious, indeed insuperable objection, that the intestine was displaced from the abdomen, and returned after having been wounded; a circumstance which, as Mr. Travers specially observes, is much more favorable to fæcal effusion than when the gut has been wounded in situ. It is indeed impossible, by experiments so conducted, to determine within any tolerable degree of approximation, the extent of wound that is likely to be followed by fæcal effusion, when the bowel has been opened without prolapsing; and so far as our knowledge at present goes, the occurrence of such effusion seems to be governed much more by accidental concomitant circumstances, such as fulness of the gut, &c., mentioned by all writers on the subject, than by the

mere extent of the wound. Dr. Gross says that Mr. Travers's object "appears to have been, not so much to deduce from his experiments on this point any practical precept in reference to the management of" wounds of the intestines, "as to show that the apprehension of intestinal effusion in penetrating wounds of the abdomen is, in the majority of cases, without" foundation (pp. 11-12); but when Mr. Travers wrote he states it to have been the "prevailing idea that it (effusion) is the uniform consequence of a wound of the intestines," whence many cases were abandoned as hopeless which might have recovered under suitable treatment, and Mr. Travers, therefore, held it to be very important in reference to the management of such accidents to establish that fæcal effusion was a comparatively rare event—a proposition which we think Dr. Gross has not shaken. Cases of, and experiments on, wounded intestine with prolapse returned without suture, have, we repeat, no bearing on the question at issue; and when we look to the numerous cases in which wounds of the intestines have been inflicted without fæcal effusion, together with the various experiments which have been performed respecting this point, we think there are good grounds for concluding that Mr. Travers's position comes much nearer to the truth than does that of Dr. Gross. Mr. Travers, we may here observe, terms Petit's statement, that fæces when effused may form a circumscribed depôt, a mere "conceit;" and avers that "in all cases which give the slightest countenance to this opinion a rupture had pre-existed, by which the gut had contracted an adhesion to the peritoneum." This is a point to which Dr. Gross does not refer; but the two following cases prove that effused fæces may be perfectly circumscribed. The first occurred in the practice of M. Jobert. A man was stabbed in the abdomen, and a portion of wounded intestine which presented at the wound of the parietes was sewed and returned; the patient died in thirty-eight hours, and on dissection another wound of the small intestine was discovered six feet from the stomach, whence had resulted an effusion of fæces, which was perfectly circumscribed on every side by the intestines, the mesentery, and false membranes. (Archiv. Gén. de Méd. 1837, p. 306.) M. Baudens records a case of gun-shot wound of the abdomen, in which the parietal wound was dilated, eight inches of small intestine included between two wounds of the bowel removed, and the extremities of the gut united by Lembert's suture: the patient died on the third day, and on dissection there was discovered a wound of the cæcum, with effusion of fæces, which was perfectly circumscribed and isolated by adhesions. The adjacent peritoneum was inflamed, which M. Baudens thinks was the cause of death, as adhesion of the reunited intestine had commenced. (Clinique de Plaies d'Armes à Feu, p. 335.) Both Mr. Travers and Dr. Gross agree that fæcal effusion into the abdomen is of necessity fatal. Were the fæces diffused, such must, doubtless, be the result; but Mr. Nourse's well-known case (Phil. Trans. 1776)—and others of the same kind might be quoted—is an example of confined fæces ultimately escaping by the external wound and sloughing of the abdominal parietes, and the case terminating favorably. This, it is true, has only occurred, so far, at least, as we are aware, when the fæces were circumscribed in the vicinity of the wound of the abdominal parietes; but it yet seems to be possible, in cases like those just cited from Jobert and Baudens, where effused fæces are circumscribed at a distance from the external wound, that they might, as has happened with effusions of blood

similarly circumscribed, be discharged by suppuration making its way externally.

In the second chapter, on "Treatment of wounds of the intestine," Dr. Gross first considers "penetrating wounds of the abdomen unattended with protrusion of the intestines;" a section which is very judiciously written, with the exception, as we think, of one or two points on which we cannot coincide with the author.

When there is reason to suppose that fæces are effused within the abdomen, Dr. Gross recommends cutting down on the wounded bowel, removing the effused matter, and securing the wound of the bowel by suture. We certainly would approve of this practice if the occurrence of fæcal effusion could be diagnosed; but it is there the difficulty lies. Mr. Travers, Dr. Gross observes, with most surgeons, opposes searching for a wounded bowel, "on the grounds that the intestinal aperture retains its apposition with the parietal orifice; but," adds Dr. Gross, "he has adduced no experiments or facts of any sort in support of this conclusion, which is, besides, at variance with the existing state of our knowledge in relation to the subject." (p. 36.) We are here completely at issue with Dr. Gross. So far from failing to adduce facts in support of his position, Mr. Travers quotes numerous "instances of complicated intestinal wounds unattended by prolapse;" which, he says, "appear" to him "to authorize" his "conclusions." (p. 152.) Those cases surely are "facts," and important ones too, and we think, along with a host of others, abundantly establish that the wounded gut usually, to adopt Mr. Travers's words, "retains its apposition to the peritoneal wound;" a proposition which, though we think Mr. Travers states it too generally, as he does not admit, at least does not hint at, any exception to it, so far from being at variance with, is, on the contrary, consonant with the existing state of our knowledge. We do not, however, deny that cases may occur in which it would be proper to search for and secure the wounded intestine, though, indeed, we find it difficult to specify the circumstances which would justify such a proceeding. Dr. Gross, "for the sake of being more fully understood, supposes a case," that of a man, after a hearty meal, stabbed in the abdomen, "the bowel is pierced, or it may be nearly divided, and there is a copious discharge of fæcal matter, both externally and into the peritoneal cavity, as is evinced in the latter event by the excruciating pain, the gastric oppression, and the collapsed condition of the sufferer." (p. 34.) We need not stop to show that the symptoms here laid down as diagnostic of fæcal effusion are utterly insufficient; we have no doubt Dr. Gross, on reconsideration, would admit them to be so himself, but the question is, can better be supplied? Dr. Gross, though he subsequently quotes some of the cases contained in the work of M. Baudens, which we have already referred to, does not notice the statements and practice of that surgeon respecting this important subject. M. Baudens maintains, from the experience of, as he states, upwards of one hundred cases, that in penetrating gunshot wounds traversing the portion of the abdomen occupied by the intestinal canal, the latter is wounded in at least nine out of ten cases; and although no very alarming symptoms occur at first, fæcal effusion almost constantly exists, which will terminate fatally, unless the surgeon dilate the external wound, removes the effused matter, and sews the intestine. Larrey, it will be recollected, also considers fæcal effusion a very frequent consequence of

gunshot wounds. M. Baudens, moreover, lays down certain signs whereby, he says, the fact of the intestine being wounded may be determined. According to him, the parietes of the intestine when it is torn by a gunshot wound lose their softness and pliancy, contract spasmodically, and acquire an almost cartilaginous hardness; the finger then should be passed through the external wound into the cavity of the abdomen, and, as the wounded bowel almost uniformly lies immediately behind the external wound, the foregoing condition of the intestine will generally be felt, and if so, M. Baudens says, we may certainly conclude that the intestine has been opened. If fæces escape externally, or soil the finger, of course no doubt can exist. And, furthermore, we may also conclude that the bowel is wounded, if gentle pressure on the abdomen expels bubbles of air through the wound of the abdominal parietes. If none of those symptoms exist we may infer that the intestine has escaped; but if any of them are present we should, he says, cut down on and sew the intestine. M. Baudens gives two cases in which he adopted this practice, in consequence of feeling the indurated condition of intestine above described, when he passed his finger into the wound. One of those cases we have already mentioned; in the other, after the external wound was dilated, the patient was desired to cough, on which a quantity of air, which had been effused into the abdomen, escaped, and the intestine protruded: a wound was discovered in the arch of the colon, which was secured by three points of Lembert's suture, and the patient recovered. M. Baudens, it is to be observed, confines his observations to gunshot wounds exclusively, and whatever may be the case after that description of injury, as the indurated condition of intestine he describes certainly does not exist after incised or punctured wounds, one of the diagnostic marks he mentions would in such cases be absent; nor, indeed, does it at all appear that M. Baudens considers fæcal effusion to be of frequent occurrence, except after gunshot wounds. However, one sign which he mentions, namely, tympanitis (which M. Jobert considers so important in rupture of the intestine without any external wound), would certainly seem to be of some value in any case; and if it be admissible to speak conjecturally on such a point, we would be inclined to say that tympanitis rapidly following a wound of the abdomen, especially if air could be expelled through the wound, would entitle us to rationally conclude that the intestine was open, and that fæcal effusion had either taken place or was to be apprehended, and certainly, if external issue of fæces were conjoined with or had preceded tympanitis, we would deem a surgeon justified in searching for the wounded bowel. The two cases, however, already quoted from MM. Jobert and Baudens show the uncertainty that attends diagnosis of wounds of the abdomen, but the complication that existed in those cases scarcely affects the point we are now considering.

The next section is on "Penetrating wounds of the abdomen, with simple protrusion of intestine, or omentum." There is, perhaps, no point on which surgeons are so unanimous as that a protruded and unwounded intestine should be replaced in the abdomen. The only writer we recollect who hints at any limitation to this rule is Delpech, who recommends us not to interfere if the gut adheres to the lips of the parietal wound, because adhesions limit inflammation, which might spread were they broken up; and experience has shown that a considerable portion of protruded

intestine may heal over and continue to perform its functions. Dr. Gross says that to leave an intestine unreduced is a practice which must be "speedily followed by the death of the patient, or, what is scarcely less pitiable, an artificial anus." (p. 37.) It is, however, perfectly certain, as Delpech above incidentally states, that neither of those results must necessarily follow, as appears from several remarkable cases. Dr. Cochrane informs us that at St. Christopher's, in 1778, a negro stabbed himself above the navel; "an expert surgeon" found a considerable portion of bowel protruding, which he attempted to reduce after dilating the external wound; but, being foiled by the obstinacy of the patient, the "expert surgeon," at last, "got out of temper," when "the manager, knowing the worthlessness of the negro, locked him up, and, in a great measure, neglected him, thinking his recovery impossible." Some days subsequently Dr. Cochrane met the man walking to town, "supporting, in a coarse woollen blanket, the protruded intestines," which formed a mass about the size of a child's head; and, to the increase of the doctor's amazement, he found that the negro had been bathing in the sea. Ultimately, "the guts" became covered with a cicatrix, and the negro "could undergo any labour, and had no other inconvenience than supporting the tumour, which resembled the mamma of a woman." (Medical Commentaries, by A. Duncan, vol. x, pp. 276-8.) M. Lessiere records a case of protrusion of the omentum and stomach, complicated with a wound of the latter organ. The omentum was tied and cut away; and, as the patient was threatened with suffocation at every attempt to return the stomach, it was left unreduced. On the fourth day, a surgeon attempted to sew the wound of the stomach, to prevent the escape of the food; but the thread cut through the coats of the organ. In about two months the stomach had gradually and completely retracted within the abdomen, and the visceral and parietal wounds healed perfectly. (Mém. de l'Acad. de Chirurg., t. i, pp. 59-24.)

Though no doubt exists as to the propriety of reducing a simply protruded portion of bowel, there is some difference of opinion as to how protruded omentum should be dealt with. Dr. Gross very properly, as a general rule, recommends it to be returned, and deprecates the practice of Larrey, who, he says, "has advised us to let it alone." (p. 39.) But this is an inaccurate statement of Larrey's advice, who distinctly recommends the omentum to be returned, if seen before it has become considerably swollen, and, in that event, to dilate the external wound, if the omentum is thereby strangulated. We certainly consider those rules of practice more judicious than our author's recommendation, that protruded omentum "should *always* be carefully returned;" for our practice should vary according to the particular circumstances of the case.

In reference to the reduction of protruded intestine, Dr. Gross correctly observes, "it is all-important to know that it has actually slipped into its natural situation." (p. 38.) Arnaud and Garengeot long since warned surgeons not to return the bowel into the sheath of the rectus when the parietal wound involved that muscle; to this Dr. Gross does not allude, but he mentions another possible mishap which has been little noticed, viz. passing the intestine between the peritoneum and the abdominal muscles. As this is an event which may appear very little liable to happen, and as Dr. Gross refers to no case in which it occurred, it may be well to notice one recorded

by Mr. Ellis. A man received a wound about three fourths of an inch long in the abdomen, from which nine inches of intestine protruded; with much difficulty, and after about forty minutes' perseverance, the gut was put out of sight, and in forty-eight hours the man died. On dissection, Mr. Ellis found a considerable portion of the bowel placed between the peritoneum and the abdominal muscles. (Lancet, 1834-5, vol. ii, p. 755.) A similar but not identical misadventure, not noticed by Dr. Gross, consists in pushing the intestine between the abdominal muscles, an example of which occurred in the practice of M. A. Berard. A portion of intestine prolapsed from a wound a little above Poupart's ligament, and was returned with apparent facility, but uniformly presented again externally after having been seemingly reduced. At length the gut remained up, and M. Berard, aware of the possibility of what had actually happened, passed his index-finger into the abdomen in order to assure himself that the intestine had been regularly replaced. On the fourth day after the receipt of the wound the patient died of peritonitis, which evidently commenced at and spread from the site of the wound, and on dissection a portion of intestine was found in a cavity between the external and internal oblique muscles. (Gazette des Hôpitaux de Paris, June 1842.) Those two cases show that such accidents may occur in the practice of surgeons of unquestionable skill and information. Mr. Ellis and M. Berard both ask whether the separation of the tissues into which the bowel was insinuated in their respective cases, was made during the attempts at reduction, or whether it had been in the first instance replaced within the abdomen, and had subsequently gotten into the situation in which it was found, and they both answer that the bowel was placed during the attempts at reduction in the positions above indicated, in which we think they are perfectly right. In both those cases reduction was extremly difficult, and perhaps, therefore, when such difficulty occurs there may be additional reason to suspect the possibility of the existence of a parietal pouch, and to take every possible precaution against passing the intestine into it.

"Penetrating wounds of the abdomen, with protrusion and injury of the intestines." After a judicious section on the therapeutic means to be adopted in those cases, Dr. Gross proceeds to discuss their treatment by different kinds of suture, but it seems more methodical to first consider the methods of treatment without any suture of the wound of the intestine itself.

When a wounded intestine protrudes, surgeons are by no means agreed as to what practice should be adopted. Among the older surgeons, Heister, Garengeot, De la Faye, Dionis, Sharp, Palfin and others, inculcated returning the intestine without suture when the wound is small; Le Dran and B. Bell, on the contrary, recommend the smallest orifice through which the contents of the gut could escape to be sewed up. Among modern surgeons, Mr. Travers, though he concludes from experiments that very small wounds may be safely returned, yet recommends the wound, however small, to be secured by suture, though M. Jobert represents him as attributing with Scarpa the most mischievous consequences to sutures, and utterly rejecting them. (Traité des Malad. Chirurg. du Canal Intest., t. i, p. 73.) Mr. Lawrence dispenses with the suture in a mere puncture, but recommends its employment if fæces could possibly escape through the aperture. Boyer considers the suture indispensable in wounds of the intestines exceeding four lines in length; Richerand rejects

it in wounds not more than two or three lines long; Vidal de Cassis recommends its application when the wound is two lines long; and Jobert says we may safely return a wound three lines long, and, *à fortiori*, a puncture, even though a little fæces exude from it. Callisen, Richter, Marjolin, Begin, and Gibson say that the suture should not be applied in small wounds of the intestines, but none of them specify what extent of wound may be safely left to nature; and Mr. Gibson, as if mistrusting his own precept, during an operation for strangulated hernia, successfully imitated Cooper and Lawrence, by tying a ligature circularly round a small aperture in the intestine. Mr. Syme speaks doubtfully on this matter, but thinks it prudent to make a point of suture when the wound exceeds a mere puncture. Finally, the suture is altogether rejected by some, as, for example, Scarpa, who, however, admits that "a timorous surgeon," afraid "to commit the whole to nature," might "with impunity pass a ligature through the mesentery opposite the seat of the wound of the intestine," a proceeding which others, with J. Bell, more timorous still, replace by stitching the wound of the gut to that of the parietes to the abdomen.

Dr. Gross states that the method of simply returning a wounded intestine without suture was proposed by Scarpa. He considers it to be a plan even "more extraordinary and unaccountable" than that of J. Bell, and states that it, in common with J. Bell's method, is now universally condemned. The fact, however, is that this method is a very old one, was practised long previously to the time of Scarpa, and has frequently succeeded: that it is very easy, moreover, to account for its having been proposed, as the practice is a very legitimate inference from the ideas once generally held respecting the reparation of wounds of the intestine, and that so far from its being universally condemned, many modern surgeons recommend it, and we have some recorded examples of its having been adopted very recently. As to the method having originated with Scarpa, Tulpius (Obs. Med. lib. iii, cap. 20) and Hollerius (Obs. et Cons Curandi, p. 17), who are referred to by Mr. Travers, each record a case in which this mode of treatment succeeded perfectly, the wounds having healed thoroughly after artificial anus had existed for some time: Tulpius's case is the more interesting, as it is, we believe, the first example of a wounded intestine having been dissected long after union; the wound, we are told, was closed by a dense firm cicatrix. Ravaton's practice also was to return a wounded intestine without suture; he condemned the suture as not merely mischievous but murderous, and deemed it to be the essential point not to heal the outer wound till the fæces ceased to pass by it; with which view he sewed the upper portion only of the external wound, and kept a tent in its lower part; and he gives two cases which were thus treated and recovered perfectly. Several modern authors also favour, more or less, the method improperly termed Scarpa's. Delpech, in every case, Richerand, when the wound exceeds three lines in length, and Marjolin (Dict. en vol. 21, t. xvii, pp. 107 et seq.), in extensive transverse wounds and those with loss of substance, recommend that method, with the precaution of passing a thread through the mesentery. We find too, that in addition to the older cases, there are a few of very modern date, in which Scarpa's plan, so called, was adopted. One such case is mentioned in Scarpa's work on Hernia; and we may also refer to the following cases: A female lunatic wounded herself in the abdomen, and cut

away seventeen inches of the small intestine, with a portion of the omentum, weighing one ounce and a quarter. One end of the gut had retracted within the abdomen, the other protruded, presenting an oblique ragged edge. Dr. Buttolph, deeming death inevitable, replaced the protruding portion of gut within the abdomen, and carefully stitched the external wound. No peritonitis supervened. Thirty-three days after the injury there was a scanty discharge of hard fæces, on the following day a copious alvine evacuation occurred, and all then went well. Two years subsequently the woman was in good health. (American Journ. of Med. Sc., by Hayes, 1835.) A man was gored in the abdomen by a bull, a portion of protruding small intestine was perforated by the animal's horn, and its contents issued freely from the wound; Dr. Mayberry returned the intestine into the abdomen without sewing it, and a fæcal fistula resulted, which, four months after the accident, occasionally gave exit to a very little liquid fæces. (Dub. Med. Press, 1842, p. 376.) Reybard also, in a case of wound involving about two thirds of the circumference of the colon, replaced the intestine unsewed, and retained it in the vicinity of the parietal wound by means of a thread in the mesentery; the patient recovered with an artificial anus. (Mém. sur le Trait. des Anus Artif. &c. p. 43.) Velpeau also having in an operation for strangulated hernia returned with impunity a portion of bowel in which three small orifices existed, ventured in another case to replace a wound of the intestine *two thirds* of an inch long, and the patient recovered. The last case of this kind which we shall mention is that of a man who having been stabbed in the abdomen, a wound six lines long was seen in a protruded portion of small intestine; the surgeons who first saw the patient retained the bowel between the lips of the external wound by a thread in the mesentery; the patient was then carried to La Charité, where M. Roux withdrew the thread, and returned the bowel into the abdomen without sewing it; death occurred on the second day, when fæces were found effused into the abdomen. (Gazette des Hôp. de Paris, 1835, p. 1.) Those cases show that Scarpa's practice is not universally repudiated, and in the two last cited we find it was adopted by surgeons of the highest reputation. We certainly, however, agree with Dr. Gross that it deserves to be rejected, and the last case well illustrates its danger, which might be further shown by several others: for example, in Cooper's folio work on 'Hernia' (pp. 32-34), two cases are given in which, during operations for crural hernia, the gut was either torn or cut in one, and perforated in the other, and in both inadvertently returned, the breach not being perceived at the moment; the patients both died with fæcal effusion into the abdomen.

Among the foregoing cases we have seen one in which a single extremity of a completely divided intestine protruded from the abdomen. The possibility, however, of this event has been doubted, and we do not find it noticed by Dr. Gross. John Bell sneers at B. Bell's recommendation, that in such a case we should search for the other end, and stitch the two together; saying, "as for intestines cut fairly across in all their circle, I believe the thing cannot happen, and that this, like all the rest, is a piece of mere guesswork: for, if I know anything about the way in which the viscera are disposed within the belly, it must happen that a sabre which cuts one piece of intestine fairly across, must have many others torn half through," and then he adds, we may "just as well let the poor man alone."

Roux also says (Nouv. Elémens de Méd. Opérat.) he does not know "that a case of an intestine completely divided by a wound has ever been observed." Such cases are, unquestionably, of very unfrequent occurrence, and are very difficult of explanation, except where, as in Dr. Buttolph's case, a maniac severs the protruded bowel; there are, however, some examples of the intestine having been completely divided by a simple stab. Garengeot gives the case of a soldier who was wounded in the lower part of the right epigastrium; the ileum was completely cut across, and one end of the gut protruded from the wound. The wound was judged mortal, "and the poor man was let alone," but he recovered with an artificial anus. (Traité des Opérations, t. 1.) The following case occurred in the practice of M. Berard. An assassin was carried to the hospital of St. Antoine, after having received in the left inguinal region a sabre wound 3½ inches long, from which about two feet of small intestine protruded, the bowel was completely divided, and one of its extremities only depended externally; M. Berard attempted in vain, by methodical traction on the mesentery, to discover the concealed end of the bowel. Matters were then left to themselves, and the man died in a few hours. On dissection the depending end of the gut was found to be the lower end, while the upper extremity lay close to the internal orifice of the wound of the abdominal parietes, and could have been easily reached by dilating the wound a little upwards and following the mesentery. (Gaz. des Hôpit. de Paris, 1832, p. 269.) Despite J. Bell's sarcasms, we cordially concur in the practice recommended by B. Bell in such cases. It may be right to add, that in M. Berard's case just mentioned, J. Bell's speculation was partly borne out, as several coils of the intestine were slightly notched—very slightly so, however; and so far from the bowel being "torn half through" in several situations, its cavity was scarcely laid open, except at the point where it was completely severed.

What is commonly termed John Bell's method, viz. stitching the gut to the external wound, should, Dr. Gross states, rather be termed the method of Palfin, as having originated with that surgeon. This is, however, a mistake. The method in question was, we believe, first proposed by De Blegny, who, having witnessed a wound of the ileum in which the gut adhered to the external wound, and the patient recovered with an artificial anus, instituted experiments on dogs, and finding that they recovered, though with artificial anus, after the intestinal canal had been opened and stitched to the external wound, proposed this mode of treating wounds of the intestine in the human subject. (Acta Eruditorum, 1682, t. i, p. 22.) The date of Palfin's Anat. Chirurg. is 1734 (not 1743, as Dr. Gross states), so that supposing there was any parity between De Blegny's and Palfin's methods, the former anticipated the latter by upwards of sixty years. But dates are here of no importance, for Palfin's method is entirely different from that proposed by De Blegny and adopted by J. Bell, as we shall presently see. Nor was this method forgotten between the days of De Blegny and J. Bell: Amyand, for example, surgeon to St. George's hospital, writing in 1736, says, "the readiest way to obtain a cure of a wounded or bursted gut, is to keep it in contact with the outer wound and the patient on a very low diet." (Phil. Trans. 1736.)

Respecting J. Bell's method (for so we shall term it for convenience sake), Dr. Gross says, "that it might occasionally be attended with suc-

cess is not improbable" (p. 106), but that the verdict of the profession is entirely against it. Now the truth is that the method has frequently succeeded; B. Bell, for example, saw two cases of completely divided intestine, in which the extremities of the gut were stitched exactly opposite each other to the lips of the external wound, and recovery ensued without the formation of an artificial anus; and Hennen mentions two cases of oblique wound of the colon, upwards of an inch long, in each of which he secured the gut by a single stitch to the parietes of the abdomen, and both patients recovered in a few days. (Military Surgery, p. 416.) As to the verdict of the profession having been pronounced against this mode of treatment, we really are not quite certain but that the authorities might nearly, if not quite, preponderate in its favour; Dr. Gross himself cites Professors S. Cooper, Gibson, and Syme as giving it their sanction, and Hennen, we have seen, adopted it,—but to this point we shall return.

Dr. Gross, while condemning J. Bell's method, resolved to test its value by experiment, though Dr. Smith and Mr. Travers had, he says, already exposed its insufficiency. Mr. Travers, however, has not published any experiment conducted according to J. Bell's method. Dr. Gross refers to his Experiment R (p. 116) as being such, but it is, on the contrary, the very reverse, for in it the lips of the wound of the bowel were brought into contact by a single point of suture inserted opposite the mesentery, the gut was returned, and the external wound sewed. It is right to add that Mr. Travers by no means gives this experiment as a trial of J. Bell's plan; he adduces it to show that the interrupted suture favours fæcal effusion more than no suture at all, as each stitch, he maintains, becomes the extremity of an aperture the area of which is determined by the distance of the stitches. Dr. Smith's experiments, indeed, purport to be trials of J. Bell's method, but they are not; for he distinctly says that the intestinal wound "was *secured by one stitch, and* fastened to the wound of the parietes." Dr. Gross's three experiments, intended to test the value of J. Bell's method, not only have no resemblance to it, but are the very reverse of it; they are simply repetitions of Mr. Travers's experiment just noticed, one stitch having been taken *in the wound of the gut* and the external wound *closed.* Consequently, neither Dr. Gross's experiments nor those of Dr. Smith were trials of J. Bell's plan, and Mr. Travers's neither was, nor was intended to be so; yet Dr. Gross says, "thus, in seven experiments, all conducted, there is reason to believe, with the requisite care and skill, not a single one had a favorable termination." (p. 108.) The truth is, J. Bell utterly deprecated any suture of the wound of the intestine; his recommendation was to secure the bowel by a single stitch to the outer wound, which was to remain open to give free exit to the fæces. Dr. Smith's experiments, above noticed, were close imitations of Palfin's plan, the only difference being that Palfin, after making a single stitch in the wound of the gut, simply brought the extremities of the thread through the external wound, instead of fastening the bowel to the external wound. We are not aware of any case in which Palfin's method has been adopted in the human subject.

Dr. Gross, we have seen, considers Palfin's, J. Bell's, and Scarpa's methods as "extraordinary and unaccountable." Palfin, he states, "entertained the singular notion that the divided ends" (of the intestine) "never united with each other, but that the cure was effected solely by

the adhesions which they formed to the surrounding parts." (p. 105.) Now this gives the explanation of the puzzle, and fully accounts for the proposal, and, till very recently, the nearly general adoption of the modes of treatment we have been considering. Palfin's notion, respecting the mode of reparation of a wounded intestine, instead of being singular, is as old as the days of Hippocrates:—"Si quod intestinorum graciliorum discinditur non coalescit" is a dictum which still passes current. Surgeons have always been guided in their practice by their views respecting the process whereby a wounded intestine was repaired, and, as they almost universally assumed that the intestine could only heal by adhesion to the adjacent peritoneal surfaces, they not illogically rejected sutures as tending to excite inflammation; and, accordingly, Morand, Ravaton, Sharp, Scarpa, Sabatier, Delpech, Leveillé, J. Bell, Richerand, Begin, Reybard, Roux, Patissier, more or less completely reject the suture, while others, with Boyer, merely adopt it as a temporary resource against fæcal effusion. The opponents of the suture also deprecated it on grounds which we find admitted to be valid by some of its warmest advocates. Thus Mr. Travers admits that the issue of fæces externally in wounds of the intestines "relieves the patient from local and constitutional disturbance" (pp. 153-55); that, in such cases, "the symptoms are generally less imminent than of those in which the external communication does not exist" (p. 138); and, finally, that "the passage of the matters by the wound does not in any degree impede the subsequent efforts of nature to restore the canal." (p. 157.) These are, in fact, the very arguments of those who deprecated the suture, they held that dispensing with it was the safer practice, and not likely to be followed by artificial anus. Within the last few years, indeed, sutures, so applied as to bring the serous surfaces of the intestine into contact, have come into favour; but most authorities hold with A. Berard (Dict. de Méd., 2d ed., vol. xvii, pp. 57 et seq.) that, in wounds of the intestine above a few lines long, Scarpa's or J. Bell's method would be preferable to any of the old sutures. Larrey, indeed, is the only continental writer we recollect, who, until very recently, held that the structures of the intestinal canal could adhere mutually by the action of their own vessels, and who accordingly adopted the suture with the intention of producing such union.

Before considering Dr. Gross's experiments respecting the process of reparation, we shall advert to his experiments respecting the phenomena presented by a wounded bowel, and the action of a ligature on the intestine. Dr. Gross's results, respecting the former point, are generally similar to those of Mr. Travers, and MM. Jobert and Reybard, but they are more precise, being estimated by measurement, and differ, in one respect, as regards the behaviour of longitudinal wounds. Reybard states that a longitudinal wound of the bowel becomes elongated; a wound two inches long, for example, becoming two and a half inches long; Travers's statement is to the same general effect. Dr. Gross found—1. That a longitudinal incision $2\frac{1}{2}$ lines long contracted to $1\frac{3}{4}$ line, and was completely closed by the everted mucous membrane. 2. A similar wound, 4 lines long, contracted, in a few seconds, to 3 lines by $1\frac{1}{2}$ in width; and the protrusion of the mucous membrane left no perceptible opening. 3. An oblique cut, 7 lines long, contracted to 5 by $2\frac{1}{2}$ in width. 4. A transverse wound, $2\frac{1}{2}$ lines long, was immediately reduced to a rounded opening 2 lines in

diameter. 5. A transverse incision half an inch long became an oval opening 4 lines long by $2\frac{1}{2}$ wide.

Dr. Gross repeated several times Mr. Travers's celebrated experiment, of firmly tying a small ligature round the intestine of a dog, and with the same general result as Mr. Travers; a few points, however, deserve notice. Mr. Travers and M. Jobert both state that a ligature, firmly applied, divides all the coats of the intestine, save the serous; and, according to M. Jobert, the latter is also ruptured at several points by an unwaxed thread, which should not therefore be employed in sewing an intestine. Dr. Gross found that, even when a small ligature is very tightly constricted, the cellular and muscular coats are but partially divided. Neither Travers nor Jobert say anything as to the time the ligature takes to cut into the cavity of the intestine. Dr. Gross, in one case, found it through more than half the circumference of the bowel on the third day; in another experiment it passed completely into the cavity of the gut in $3\frac{1}{2}$ days: in both those experiments the intestine was found constricted, the opposed mucous surfaces, lying in contact in one, the canal being partially pervious in the other. The escape of a ligature, however, is extremely variable; in another experiment it was found imbedded in coagulable lymph at the point where it had been applied on the eleventh day. We observe, Dr. Marshall Hall states (Lancet, 1837-8, p. 73), that, in performing this experiment, it is necessary to the restoration of the continuity of the canal that the animal shall have fasted twenty-four hours, and that a portion of intestine near the stomach shall be operated on. It does not appear that either Mr. Travers or Dr. Gross adopted the former precaution. Mr. Travers, indeed, operated on the duodenum, but Dr. Gross incidentally says that the experiment succeeds perfectly on the large intestine. (p. 26.)

Though Dr. Gross's results respecting the process of reparation of a wounded intestine by suture have developed nothing absolutely new, they elucidate, indeed we think decide, a much disputed point. Mr. Travers states that the contiguous mucous surfaces when approximated by suture agglutinate, but that this union is never permanent, being destroyed by the action of the longitudinal fibres of the bowel when the ligatures loosen; and this, he states, is the reason why the thread invariably passes into the intestinal canal. But, even if this retraction could be prevented, Mr. Travers thinks the mucous surfaces are incapable of uniting either directly or by granulation, and that, consequently, the breach is never obliterated internally. (Op. cit., pp. 130-2.) Jobert, on the contrary, affirms that the mucous membrane is reproduced at the line of division, and expressly states that Travers is wrong on this point. (Op. cit., p. 78.) M. Reybard, we believe, was the next to examine the action of the suture; he did not, like Travers, observe any separation of the lips of the wound after their primary agglutination by interposed lymph, on the contrary, he found them remain closely approximated; but he agreed with Travers that the edges of the divided mucous membrane never united with each other, the interspace between them remaining occupied by the originally effused lymph, which becomes an organized cicatrix depressed below the level of the mucous membrane at each border, but presenting a prominent ridge along the centre where it is thickest, and adhering to the serous and muscular coats of the intestine only, as is easily shown after a couple of days' maceration, when it can, he says, be detached in its entire extent; and this cicatrix, he thinks,

M. Jobert probably mistook for regenerated mucous membrane. (Journ. Complém. des Sc. Méd., 1830.) Dr. Gross, who does not notice M. Reybard's experiments, observed that the early cohesion between the lips of the wound very commonly, though not uniformly, persisted when the suture loosened; he also found that the adhesion of the mucous membrane, though achieved more slowly than that of the other tunics, is ultimately complete, and may occur in one of two ways. Usually the lymph effused between its divided edges is gradually absorbed, until the edges themselves come into contact, "and, after a period varying from a few weeks to as many months," coalesce directly; sometimes, however, but not frequently, the breach of the mucous surface is healed by granulation. The appearances observed in Dr. Gross's experiments are detailed with such precision as must, we think, satisfy every reader of their accuracy, and that they definitively settle in the affirmative the question of the regeneration of the mucous membrane of the intestine. It is satisfactory, however, that Dr. Gross's conclusions are strengthened by experiments performed, or at least published, almost concurrently with his own. M. Reybard's opinion has been completely altered by new researches, and he now admits that the mucous coat can cicatrize completely like the other tunics (Journ. de Méd. de Lyon, October 1843, and L'Expérience, December 1843); and M. Petrequin has also fully ascertained that the mucous tunic is regenerated, the only perceptible alteration being that the villous surface is thinner than natural, and that a slight depression exists at the seat of reparation. (Anatom. Méd. Chirurg.)

"Treatment of wounds of the intestine by different kinds of suture." Though it is fully established that a wounded intestine may be completely united by suture, it remains to be determined what is the probability of union occurring in wounds of various extent and direction, and what kind of suture is preferable. Dr. Gross first treats of the "continued" suture, which, he says, is synonymous with the glover's suture, and considers Bertrandi's suture, or the "*suture à points passés*" as different from the continued suture. (p. 101.) It is scarcely necessary to say that those two sutures are merely two varieties of the continued suture.

The experiments which have been performed with the glover's suture are very numerous. Shipton cut away two inches of the ileum of a dog, and united the extremities with the glover's suture; on killing the dog several weeks after, the ends of the bowel communicated through a kind of cyst formed by adhesions among the surrounding parts. Dr. Wallis mentions that a horse having been staked and the stomach wounded, a farrier emptied the stomach, sewed the wound, and "then thrust the maw back again into the body, and stitched up the wound in the rim of the belly;" in a few weeks the animal worked as usual. (Phil. Trans. 1695). Brunner sewed a wound one inch and a half long in the small intestine of a dog with the glover's suture; the dog soon recovered, but it is difficult to say what he would not have recovered from, as, subsequently, milk was injected into his thorax, his femoral artery was tied, his spleen was extirpated, his pancreas was cut away, and, finally, he was compelled to swallow a scruple of opium, and all without serious consequences, as he made his escape three months after the last attempt on his life. (Exper. Nova circa Pancreas Præf. pp. 6-7). According to Louis, Mœbius having failed in effecting Ramdohr's method of invagination in dogs, sewed

the ends of the bowel together, but the animals died with fæcal effusion into the abdomen. (Mém. de l'Acad. Roy. de Chirurg. t. iii.) Dr. Thomson in two dogs, Sir A., then Mr. Cooper, in one, applied the interrupted and glover's suture conjointly to wounds of the small intestine an inch and a half long; two of the dogs survived, a third died with fæcal effusion. (Cooper on Hernia, fol. pt. i.). Mr. Travers performed experiments also with the glover's suture, but deemed an account of them superfluous. M. Reybard, from his earlier experiments in 1830, already alluded to, concluded that the glover's suture was the best mode of treating a wounded intestine, an opinion in which he is confirmed by, and which M. Petrequin has adopted from, their more recent researches.

Dr. Gross performed 17 experiments to ascertain the effects of the glover's suture. In 2 the bowel was wounded transversely, and in 1 completely divided. In 12 the wound was longitudinal, varying from half an inch to 6 inches, and in 3 oblique. All the animals recovered, but 3 of them "were killed too soon after the operation to render it at all certain that they would have recovered from the effects of it." (p. 64.) We shall give the summary of the particulars of those experiments in Dr. Gross's words.

"In 8 the needle was carried through the whole thickness of the bowel, and in 5 the everted mucous membrane was pared off on a level with the surrounding surface; in 8 the suture was introduced through the fibrous lamella, or between the muscular and mucous coats, and in 1 through all the layers of the tube, except the peritoneal. It is worthy of remark that the caliber of the tube was not sensibly diminished by the operation in any of the experiments.

"Of those three methods, that of introducing the suture through the cellulo-fibrous lamella is the least objectionable, as it enables us to bring the serous surfaces into more accurate apposition. Where the needle is conveyed through all the tunics there must necessarily be some degree of puckering, whereby the mucous lining will be forced between the lip of the wound, if not beyond the level of the peritoneal coat. By such an arrangement the adhesive process would be retarded, and if the stitches were to lose their hold, or if the bowel should not become glued to the neighbouring parts, fæcal effusion might occur." (pp. 64-5.)

Dr. Gross, and MM. Reybard and Petrequin all dwell on the importance of taking the stitches of the glover's suture very close to each other, and drawing them firmly in order to ensure union. The French writers also recommend, as essential to ensure the passage of the suture into the bowel, to take the first stitch from within outwards, and to make a very large knot on, or even attach a small dossil of lint to, the extremity of the thread.

Dr. Gross considers that "the results of his experiments are eminently favorable to the use of the continued suture" (p. 64), and certainly, so far as they go, they are. In one dissection only was the breach found partially blocked by adhesion of an adjacent peritoneal surface; but here a wound six inches long had been inflicted, upwards of three inches of which was closed by the transparent mesentery. The earliest date at which the suture was found to have completely passed into the intestine was the 35th day; in other instances it was found partially adherent on the 22d, 28th, and 31st days, and its separation does not appear to have been at all influenced either by the everted mucous membrane having been pared off, or by the needle having been carried through the fibrous lamella of the

bowel, a point which we notice for the following reason. Since Dr. Gross's work was published, M. Moreau-Boutard has proposed to sew wounds of the intestines by excising the everted mucous membrane, and bringing the peritoneal coat and the exposed submucous tissue into contact, and he effects the latter object (as we infer, for in the abstract it is not very distinctly stated) by passing the sutures through the submucous cellular tissue. M. Jobert, in his Report to the Academy of Medicine, on M. Moreau-Boutard's Memoir, objects to this proposal that it would often be physically impossible in the human subject, that it incurs the risk of the intestine mortifying, from being deprived of nutrition, and, finally, that the suture cut short on the external surface of the bowel must almost infallibly be detached into the cavity of the peritoneum; for, according to M. Jobert, unless a suture traverses all the coats of the intestine, it is quite uncertain whether it will pass into the cavity of the bowel, and if it does not it either remains in the cicatrix, where it may excite inflammation and suppuration, or becomes detached into the peritoneum. It is, therefore, only when all the tunics are perforated that the suture can, he says, be safely cut short, and if the mucous coat is not pierced, the thread should, he maintains, depend from the external wound, to admit of its escaping when detached; M. Jobert adds, that a surgeon, who was ignorant of this mechanism of the elimination of sutures, had the mortification to see a patient on whom he had performed suture of the intestine die of peritonitis, excited by a suture falling into the peritoneal cavity. (Bulletin de l'Acad. Roy. de Méd. Sept. 1845, pp. 1036-40.) M. Jobert does not give any particulars of the case just referred to, nor does he cite any experiments of his own in evidence of this point, neither does he say that the sutures had not passed into the bowel in any of M. Moreau-Boutard's experiments, but simply alleges that his method is attended with great danger of their not doing so. M. Reybard, however, expressly says that if sutures do not embrace the entire thickness of the bowel they should not be cut close, as they will not then pass into the cavity of the intestine, but cause abscesses on its external surface, as he observed in experiments on dogs in which the sutures did not penetrate the mucous membrane. (Journ. Compl. 1830, t. xxxvii, p. 21.) But, on the other hand, there are experiments which lead to the directly contrary conclusion. Dr. Thomson found that the suture passed into the bowel when the peritoneal coat *alone* was included in the suture; we have seen that in Dr. Gross's experiments the separation of the suture was not influenced by the coats of the bowel it was passed through, and we may add, that in those experiments in which it was carried through the submucous tissue, and the animal was killed before its complete separation, it had made considerable progress towards being thrown off into the cavity of the bowel. Coupling, however, M. Jobert's statement with M. Reybard's positive experiments, we must conclude that this point requires re-examination, and that it would be scarcely prudent in practice to cut short a suture which included a portion only of the tissues of the bowel. M. Jobert's other objections to M. Moreau-Boutard's method are merely speculative, and seem to us to be unfounded, but the method itself, probably, does not possess any peculiar advantage.

Dr. Gross concludes the section on the glover's suture by detailing six cases: that of Mr. Travers, of Lisbon, two from Glandorpius, one from

the Dict. des Sc. Méd., and two from Larrey; of those cases two only were fatal. We are aware of but a few additional cases in which this suture has been employed. Guy de Chauliac mentions a successful case in which he sewed an intestine "vulnerata secundem longum et latum, cum sutura pelliparioruin." (Ars Chirurgica, 1546, p. 336.) Fallopius, we think, records a similar case, and Garengeot mentions a patient who died the third day after suture of the intestine was practised by Guerin. (Traité des Opérations, t. i. p. 191.) The most remarkable case, however, of the application of the glover's suture on record is one published by M. Reybard. A man, aged twenty-eight, laboured under a carcinomatous tumour of the sigmoid flexure of the colon, and M. Reybard having diagnosed the affection, cut into the abdomen, cut away three inches of the intestine affected with the disease, tied the arteries of the mesocolon, and united the extremities of the divided bowel with the glover's suture, the suture was cut short, the intestine replaced in the abdomen, and the wound of the parietes sewed with three stitches. On the 10th day a copious evacuation was passed per anum, and on the 38th day the patient was pronounced well. After the lapse of six months, symptoms of cancerous disease of the intestine again set in, and the patient died about twelve months after the operation. No examination of the body could be obtained. This case was communicated by M. Reybard to the Academy of Medicine of Paris, and M. Jobert, in a report thereon, stated that M. Reybard had performed experiments, before a committee of the Academy, on seven dogs, but in not a single instance was union of the bowel obtained; fæcal effusion occurred in all, in some diffused through the abdomen, in others circumscribed by adhesions between the intestines. The committee consequently concluded that the glover's suture cannot be relied on in the treatment of wounds of the intestine, and we certainly think that however favorable many experiments may seem to be to it, it is far inferior to suture with inversion of the serous surface of the bowel. (Ann. de la Chirurg. Fr. et Etr. Aug. 1844. p. 493.)

We may pass briefly over Dr. Gross's experiments on the interrupted suture. In fourteen experiments death occurred once only (and that after complete division of the bowel), though in some the stitches were four and five lines apart, and in no instance was the caliber of the intestine diminished. In ten experiments the sutures were cut short, and in all they either passed or were in progress of passing into the cavity of the bowel. We are surprised Dr. Gross fails to remark the great rapidity, as compared to the glover's suture, with which single stitches are detached. The latest period at which the sutures were found adherent was the 17th day, and in two experiments they were detached by the 7th and 11th days respectively. Dr. Gross concludes this, like the preceding section, by quoting a number of cases in which the interrupted suture was employed in the human subject, which, he observes, "taken in connexion with the experiments just detailed, exhibit an array of success highly favorable to this method of treatment." (p. 82.) Those cases are seven in number, five successful, and two fatal; the two latter are extracted from Cooper's folio work on Hernia; three of the former occurred in America, and two are taken from the 'Edinburgh Medical Surgical Journal' (vol. xii, p. 27), and the 'Medico-Chirurgical Review' (vol. xx, p. 182). We may just add that in the latter journal (vol. vi, p. 557) there is another

case mentioned, in which Dr. Washbourn united several small wounds of the intestine by the interrupted suture with success, and in the Journ. der Practisch. Heilkunde, Feb. 1825, is a remarkable case, in which Dr. Fuchsius, having diagnosed the existence of intussusception of the intestine, cut into the abdomen, drew out the invaginated bowel, and, being unable to free it, cut into the gut and disengaged the invaginated portion, which was two feet in length, united the wound by sutures, and the patient recovered in fourteen days.

Dr. Gross next (certainly very unmethodically) introduces a section on the "Method of Ramdohr," the history of which is very accurately given, and Dr. Gross adds to the cases of its application in the human subject that are generally known, one from the American Journal of Medical Science (vol. x, p. 42.) In this case the lower extremity of the intestine is supposed to have been passed into the upper extremity, as what seemed to be the lower end was separated from the mesentery by the knife which had inflicted the wound; the invagination was maintained by three points of suture, two other wounds of the small intestine were secured by the continued suture, and a single stitch was taken in a wound of the colon; the patient was well by the 19th day.

The short section on Le Dran's suture requires no comment, and but little need be said respecting that on the "Method of Bertrandi, or la suture à points passés." Dr. Gross says that, excepting Boyer, he knows of no modern advocates of this suture: it is, however, recommended by Sanson, Roux, Patissier, Lombard, Richerand, and others, but by the last writer in long wounds only. Dr. Gross says we have no cases of its application in the human subject, and it is remarkable that we have very few, considering how long this suture was favorably regarded by many continental surgeons. Lombard, however, mentions one case, (Clinique des Plaies Recentes, p. 197): in 1778 an English soldier was landed at Gravelines with a sabre wound in the hypogastric region, through which the ileum protruded, and was wounded to the extent of more than a finger's breadth. Lombard sewed the wound with Bertrandi's suture; all went well till the ninth day, when Lombard lost sight of the case, and did not learn its issue. The following is also an example of the use of Bertrandi's suture, as the mucous surfaces were directly confronted. M. Judrin, operating on a femoral hernia nine days strangulated, found a perforation two lines in diameter in the intestine; finding that two stitches à points passés suffered fæces still to exude, M. Judrin took two other stitches at right angles with the former, the gut was returned, and the patient recovered in a few weeks. (Gazette Méd. de Paris, April, 1840, p. 250.)

Dr. Gross, in his account of the "Method of the Four Masters," only alludes to a single experiment on this method performed by Sir A. Cooper. Louis informs us that Duverger tried the method on dogs previous to his well-known application of it in the human subject. Louis himself tried it on two dogs, and they recovered. (Mém. de l'Acad. Royale de Chirurg. t. iii.) Vogel tells us that Thomas Brayn, a veterinary surgeon, in 1704, opened the abdomen of an ox affected with obstinate constipation, cut away a mortified portion of intestine, and sewed the ends of the bowel over a tube of wood; the cylinder was soon voided per anum and the animal lived several years. (Sandifort, Thesaur. Disputation, etc., vol. ii,

p. 130.) Dr. Watson proposed this method as a new one so lately as 1790, and having excised a portion of the bowel of a large dog, sewed the divided extremities over a cylinder of isinglass. The animal, " recovering perfectly, seemed not to suffer any inconvenience from having had his gut shortened four or five inches." (Medical Communications, vol. ii, p. 111.) But, though successful in those experiments, Duverger's case is, we believe, the only authentic example of its having succeeded in the human subject, or, indeed, rather the solitary instance of its having been so applied, for it seems uncertain whether Jamerius, Roger, Theoderic, William of Salicetus, or the Four Masters, really ever practised this method, or merely recommended it theoretically.

The next section, entitled " Method of Palfin, Bell, and Scarpa," we have already disposed of. The following one is devoted to the " Method of Jobert."

Dr. Gross tried Jobert's method of invagination on two dogs, both died, one on the third, the other on the seventh day; on dissecting the latter animal, he found that the lower extremity of the intestine had been introduced into the upper, "into which it projected (firmly united to its inner surface) in the form of a mammillated protuberance, six inches in length, tapering at its free extremity, and perfectly closed" (p. 118); the intestine, consequently, was completely obstructed, and was distended with gas and fæces to at least five times more than its natural size above the obstruction. Indeed this method has proved very unfortunate in the hands of every experimenter whose account we happen to have met with, excepting always M. Jobert himself. M. Begin tried it on three dogs, they all died, one from fæcal effusion, two from peritonitis. (Recueil des Mém. de Méd. de Chir. et de Pharm. Milit., t. xxii, 1827, pp. 60 et seq.) Reybard, in fifteen experiments on dogs, passed the lower into the upper end of the gut five times, and fæcal effusion uniformly occurred: in the ten others, in which the upper extremity of the bowel was invaginated, fæcal effusion also occurred in two; three of the remaining dogs died within a month, and the five that survived that period remained emaciated, were subject to frequent vomiting, and when killed for examination, it was found that the bowel had contracted almost to obliteration at the seat of invagination, above which, it was greatly dilated, and correspondingly contracted below. (Jour. Complém. des Sc. Méd., t. xxxviii, pp. 337 et seq.) M. Petrequin's experiments with this method were also so unfortunate, that he unreservedly condemns it. Dr. Gross also rejects this plan because of its difficulty, of the violence done to the parts in executing it, and of the uncertainty of its result; and we must entirely concur in this condemnation, were it for no other reason than the impossibility of distinguishing the lower extremity of the intestine with certainty; but that distinction, according to Jobert himself, is so essential to success, that he proposes to adopt Louis' method of discriminating the upper extremity of the intestine, a proceeding which, we trust, needs no refutation. Dr. Gross, after adverting to J. Cloquet's case, in which this method promised to succeed when the case was reported, but the ultimate issue of which we do not know, mentions another, and a fatal case, in which Professor Berard, relying on the alleged sign that the lower orifice of a divided intestine is more contracted than the upper, invaginated the lower extremity of the bowel into the upper one. There is, however, a circumstance of some

interest in this case which Dr. Gross omits to notice; the intestine was furnished with valvulæ conniventes nearly to the cæcum, and it was this circumstance that induced M. Berard to attempt invagination instead of allowing an artificial anus to form; for as he supposed that the small intestine was divided high up, because of the existence of valvulæ conniventes at the site of the wound, he feared that an artificial anus would have been followed by death from defective nutrition, had the patient survived the immediate consequences of the wound. There are two other cases not mentioned by our author. One is mentioned by M. Jobert. (Archiv. Gén. de Méd. 1824, t. iv, p. 74.) A woman wounded herself in the throat and abdomen; the divided intestine was invaginated, and she died in fifteen hours from the effect of the wound of the throat, but soft filaments of lymph were found interposed between the portions of the bowel. The second occurred in M. Jobert's practice at the Hospital St. Louis. A man, aged 70, cut away a portion of his intestine during the night, and the injury was not discovered till next day, when, in addition to the wound with loss of intestine, it was found that the bowel was completely divided in a second situation; this latter wound was united by an invagination retained by five points of simple suture; the extremities of the portion of the canal which had suffered excision were kept in contact with the abdominal wound, in hopes of establishing an artificial anus. In thirteen hours the patient died, yet adhesion had made such progress that the invaginated serous surfaces remained glued together after the sutures were removed. (Annales de la Chirurg. Fr. et Etrang. Dec. 1842, p. 437.) M. Laborie, who records the foregoing case, adds, that M. Boulen has successfully treated *two* cases of complete division of the small intestine, by M. Jobert's method of invagination, and that M. Jobert himself had lately performed the same operation with perfect success on a patient of M. Beaumes, in whom the intestine had become gangrened in a crural hernia. No particulars are, however, given respecting the three last cases, and, indeed, the precise mode of invagination practised is not clearly stated as regards the two former either, so that the four last, at all events, of those five cases may be, and we suspect probably are, examples of Lembert's rather than of Jobert's method. Dr. Gross does not appear to have tried experimentally M. Jobert's suture for partial wounds of the intestine; M. Reybard objects to this method, that the kind of valve which results from it would be dangerous in transverse wounds, and that simply collecting and twisting the sutures together and bringing them through the parietal wound, without knotting them, is insufficient to keep the lips of the wound of the intestine inverted and in contact, as he experimentally ascertained in dogs. This objection we find to some extent confirmed by a case published in the 'Annales de la Chirurgie.' (loc. sup. cit.) M. Jobert united a wound in the small intestine three centimetres long, with three sutures inserted according to his method, and instead of knotting the threads, twisted them together, and allowed them to depend from the external wound; the patient died next day from peritonitis and a vast effusion of blood in the pelvis; the wound of the intestine had almost all united, but was patulous at one angle.

Dr. Gross performed twenty-three experiments with Lembert's suture, but he neither deduces any conclusions from them, nor gives any analysis of the results. In nine of the experiments the bowel was completely

divided, and four of those animals died, two with fæcal effusion. The results of the experiments on partial wounds of the bowel with this suture are very favorable, one dog only perished; those experiments also show the rapidity with which single points of suture pass into the bowel; in one experiment they had disappeared on the 12th day, and were never found at a much later date. The caliber of the intestine does not appear to have been in any instance diminished even when the wound was two and a half inches long, nor do the inverted lips of the wound seem to have permanently projected into the bowel; in one dissection, on the 17th day after a wound two and a half inches long had been united by eight points of suture, it is merely stated that "the villous margins of the wound were a good deal more elevated than common, but it was evident that they were everywhere continuous with each other." (p. 140.)*

We pass the "Method of Denans" and its modification by Baudens, nor shall we stop to notice a somewhat analogous, but even more objectionable proceeding proposed by Spillmans, an account of which may be found in the Memoir of M. Begin, which we have already referred to. As regards the "Method of Reybard," we shall only observe that M. Reybard has long since renounced his own method, and strongly pointed out the disadvantages and dangers attendant on it. Dr. Gross says he is not aware that this method has been employed on the human subject; but it has, and with success, by its inventor, who, notwithstanding, we repeat, unreservedly condemns it. The case in which M. Reybard applied his own method, is, we may observe, recorded in M. Reybard's 'Memoir on Artificial Anus,' &c., a work to which Dr. Gross himself refers.

Dr. Gross finally considers the methods of Amussat, Thomson, Choisy, and Beclard, which are all mere speculative proposals founded on Mr. Traver's celebrated experiment, in which the continuity of the intestine was restored after it had been circularly constricted with a ligature, and severally consist in so constricting the extremities of a divided intestine after they have been invaginated, either simply, as Beclard proposed, or over a tubular apparatus of more or less fanciful construction, as devised by others. On these speculations we need not dwell; but we may here notice the result of the only case we are aware of, in which a human intestine was completely included in a ligature; the case is mentioned by Reybard. (Mém. sur le Trait. des Anus Artif., etc. pp. 86 et seq.) A child, aged between six and seven, was wounded in the abdomen, the colon protruded, but was not wounded, and the surgeon, if such he is to be called, who saw the child, tied a silk ligature round the bowel and replaced it, the result of this strange proceeding was, that symptoms of strangulation came on, and continued until the ligature cut through the coats of the intestine, and an artificial anus resulted. It is well to know that such a proceeding is not necessarily fatal in the human subject.

Of the preceding methods of suture, Dr. Gross prefers the glover's

* Dr. Gross quotes several cases in which Lembert's suture was employed. We are not aware of any other cases besides those quoted by Dr. Gross, and we give the references to them, as so doing may prove convenient to some of our readers. 1. M. Jobert, Recovery (Lawrence, Treatise on Ruptures, p. 306; Velpeau, Medecine Opérat., t. iv, p. 143). 2. Dieffenbach, Complete division of the intestine, recovery from operation, death several weeks after from internal strangulation (Lawrence, ut supra, p. 362; British and Foreign Medical Review, vol. iii, p. 517). 3. Jobert, Recovery (Archiv. Gén. de Méd., March, 1837). 4. Baudens, Recovery (Op. sup. cit., p. 336). 5. Liegard (Velpeau, Méd. Opérat., t. iv, p. 143). 6 and 7. Jobert, Fatal (Lawrence, Op. sup. cit.), 8. Baudens, Fatal (Op. sup. cit.)

suture and that of Lembert, without awarding a preference to either, except, indeed, the whole cylinder of the intestine were divided, in which case he would prefer the glover's suture, "especially in young subjects, in whom the canal is very narrow, or in persons in whom the bowel is overloaded with fæcal matter at the moment of the injury," as "in a case of this kind the inverted edges might occasion serious obstruction, from the manner in which they project into the interior of the canal." (p. 157.) For our own part, we would certainly prefer Lembert's suture, or that of Gely, in every case where we deemed a suture applicable. We confess, however, that we do not think it is yet satisfactorily determined what is the most advisable course to pursue when an intestine is completely divided by a recently-inflicted wound, and we are by no means satisfied but that the safest course, in such an event, is to establish an artificial anus, taking care so to dispose the extremities of the intestine as to favour attempts to subsequently heal the wound; to discuss this point suitably would, however, at present carry us too far. Dr. Gross, we may here observe, states in the next chapter (for he is eminently unmethodical in his arrangement and discursive in his observations) that he would deem it advisable "in extensive longitudinal or oblique wounds, to excise the affected part, and treat the case like one in which the tube is completely divided in the first instance . . . . especially where the opening is more than two inches in length." (p. 171.) In this opinion we can by no means coincide; Dr. Gross's reasons for adopting it are—that extensive wounds take a long time to heal, that the canal may become permanently contracted, that adhesion in them is seldom complete, that numerous sutures are a source of irritation, and that mischief must result from the protracted manipulation necessary to apply them. We do not dispute the force of those reasons, but most assuredly they apply *a fortiori*, one and all, to complete division of the intestine, and, *if* valid, the necessary inference from them is, that extensive wounds of the intestine, and still more complete division of it, should be treated, not by suture, but by the establishment of an artificial anus.

It has been objected, and not without some reason, to Lembert's suture, that the wound is not securely closed unless the points of suture are tolerably close, and that the danger of inflammation increases with their number. M. Gely, to obviate this inconvenience, has proposed a modification of Lembert's suture, which purports to combine the advantages of so thoroughly closing the wound as to obviate all danger of either immediate or secondary effusion; of presenting such an arrangement of the suture that neither the knot nor any portion of it is visible on the peritoneal surface, and that it must certainly pass into the intestine; and finally, of being easily executed and applicable in every case. This suture may be practised with one or with two needles. When two needles are employed, which is the preferable mode, one of them is inserted a line or so behind, and external to one of the extremities of the wound, is carried parallel to the wound for two or three lines in the cavity of the intestine, and is then brought out on the peritoneal surface again: precisely the same is done with the opposite needle; the threads are then crossed, the left needle is now introduced into the puncture through which the right needle has just passed, and a stitch similar to that first made is taken with it, and a like stitch, in like manner, is made with the right needle. As many stitches

are thus made, parallel to each side of the wound, as its extent may require, and it then only remains to tie the threads and tighten the stitches sufficiently; this is accomplished by pulling the threads at each point where they cross the wound with a dissecting forceps, at the same time inverting the lips of the wound, which soon become so perfectly coapted that the thread is completely concealed between them; finally, the extremities of the thread are knotted and cut close, and the knot is as effectually concealed as the rest of the suture. The needles should be fine, but a little larger than the thread, to permit the latter to run with freedom; when the needles are crossed, it is not indispensable to pass them exactly into the orifice which the opposite one has traversed, and it both facilitates and hastens the operation to tighten and knot the threads each time they are crossed: in complete division of the intestine this is indeed indispensable. This mode of suture may, perhaps, seem difficult when thus described, but it is really easily performed, as we have ascertained by trial; the essential point is to make the corresponding stitches at the opposite sides of the wound of equal length, as otherwise the wound is puckered. It is unnecessary to describe the manipulation when a single needle is employed. M. Gely states, that when the completely divided intestine of a dog is united by his method the resulting valve produces nearly complete temporary obstruction; but the valve gradually diminishes, and perhaps it may finally disappear, as we find, that in one of his experiments, five and a half months after complete division of the intestine, the valve had disappeared round half the circumference of the bowel, and in the other half resembled one of the valvulæ conniventes somewhat thickened. The suture applied in this way is detached very slowly: in one experiment it was found adhering in the wound two months after having been applied. M. Gely's experiments on animals with this suture are scarcely sufficiently numerous, but so far as they go are favorable to the method. M. Gely has, also, twice applied his suture in the human subject. The first case was that of a sailor, who received a wound in the left lumbar region, through which about a yard of small intestine protruded; the prolapsed bowel presented two small wounds exactly opposite to each other, having been transfixed by the knife; each wound was secured according to the method already described; the intestine was reduced, and the patient recovered without having presented any very serious symptoms. In the second case, M. Gely inflicted a small wound on the intestine during an operation for strangulated hernia; he secured the wound by a stitch of his suture, returned the intestine, and the patient recovered.

We may here mention that M. Hip. Nunciati of Naples, has proposed a *spiral* suture, by which the serous surfaces of the lips of the wound of the intestine are inverted and brought into contact; it may be that the account we have seen of this method is imperfect, but it seems to us quite analogous to the modification of Lembert's suture already proposed by Dupuytren. M. Nunciati's suture, is performed with a single thread, which is carried along the wound alternately from left to right, and from right to left, and then, by pulling the extremities of the thread, one at each angle of the wound, the lips of the wound are inverted and brought into close contact. M. Nunciati is said to have treated three cases successfully in this way, but we are not acquainted with their particulars. (Bulletin de l'Acad. Royale de Méd., Sept. 1845, p. 1041.)

None of the old sutures are applicable in wounds with loss of substance, involving a portion only of the circumference of the intestine; but M. Gely maintains that his method is perfectly suited for such a case. When so applied, the intestine must of course be flexed on itself, and the more so, the greater the loss of substance. The resulting curvature of the bowel, M. Gely maintains, from the results of experiments on animals, does not cause any obstruction or other inconvenience, even when the inflected portions of the bowel are placed parallel to each other. Again, if two orifices, with loss of substance, should exist in the intestine, M. Gely proposes that they should be brought in contact by means of his suture, which is as easily applied to two orifices as on the two margins of one aperture. But the few observations we have to make on this head may conveniently find place in a brief notice of Dr. Gross's third chapter, entitled "Of the treatment of wounds of the intestine by ligature and excision."

This chapter relates entirely to the conduct which should be pursued when a portion of intestine is gangrened in a strangulated hernia. Dr. Gross first alludes to the case of a minute orifice in the intestine, and notices the practice of encircling it with a ligature, which we have already referred to; but we may here allude to a case which shows that a ligature thus applied does not uniformly make its way into the cavity of the bowel. A man received a sabre wound in the abdomen, two and a half yards of the bowel protruded, and at one point, near the mesentery, presented a wound about the size of that commonly inflicted in venesection; it was uncertain whether this wound penetrated all the coats of the intestine; but Dr. Kothe, to render matters secure, pinched up the wound in a forceps and included it in a small circular silk ligature, which he cut close to the knot. All went well till the sixth day, when peritonitis set in, and the patient died on the ninth day. On dissection, the ligature was found lying loose on the surface of the jejunum, and though the intestines were removed from the body, no trace of the wound could be found, though it was supposed to have pierced the bowel, as the patient had passed bloody stools. The failure of the ligature to cut into the bowel, in this case, may have arisen from its not having been applied sufficiently tight, but this, of course, is matter of conjecture. (Lond. Med. Gaz. 1827-8, vol. i, p. 807, and Rust's Magazine, 1828.) As to the practice to be adopted when a considerable portion of intestine is gangrened, Dr. Gross cites several cases to show that patients may survive the loss of even several feet of the intestinal canal. Among these, he alludes to Mr. Needham's case in the 'Philosophical Transactions,' as being that of a boy who had fifty-seven inches of his bowel "cut off by a cart;" but that is not exactly the fact; the case, indeed, is a curious one,—the pressure of a cart passing over the abdomen forced a large portion of the intestine, with part of the mesentery, out of the anus. Every attempt at reduction failed, as the bowel was constantly again protruded by retching, and on the third day, as the parts appeared to be mortified, Mr. Needham cut them away. The greatest loss of intestine we recollect, where the patient survived, occurred in an operation for hernia by Arnaud, who cut away "more than seven feet" of gangrened bowel (A Dissertation on Ruptures, pp. 341 et seq.); but all this has really nothing to do with the matter at issue; if an intestine is gangrened it is already lost, and the only question is, shall the surgeon suffer an artificial anus to form, or endeavour to restore the continuity of the canal.

In considering this question, Dr. Gross leaves out of sight that an opportunity of interfering in the latter way really very seldom occurs, for the bowel has commonly become adherent above the mortified part, and we believe no surgeon disputes the rule that such adhesions are not to be disturbed. But suppose the bowel is not adherent, which Dr. Gross seems to assume is the usual condition of things, he leans to the opinion that, "when there is much inflammation beyond the sphacelated parts, it would probably be wrong to pursue any other treatment" than to suffer an artificial anus to form, but "if, on the other hand, the tube is nearly or quite sound, I should not hesitate to excise the mortified structures, and to approximate the ends by the suture." (p. 173.) M. Gely thinks that his suture may be applicable in cases of gangrened intestine, when a portion only of the cylinder is involved; but if an entire zone of the tube is destroyed, he prefers the establishment of an artificial anus, but, at the same time, proposes an expedient which seems to us extremely plausible and well worthy of consideration, as probably presenting the advantages of the suture without its dangers: this expedient is, to unite by his suture the extremities of the intestine, not completely, but in a third or one half, of their circumference only; obstruction to the course of the fæces would be thus avoided, and the bowel favorably placed for healing the artificial anus. M. Gely further asks, might not a notch be cut in the free border of each of the portions of intestine thus united. This would be, in fact, anticipating the action of Dupuytren's enterotome on the *eperon.* M. Gely has found this answer perfectly on dogs, but speaks with fitting reserve as to its application in the human subject.

Dr. Gross's fourth and last chapter, is on "Artificial Anus," and in it the pathology and treatment of the affection are fully and clearly considered. This chapter cannot be expected to present very much novelty, and but a few points call for observation.

Dessault, Noel, and others, have succeeded in curing artificial anus by obliterating the prominence of the *eperon* by pressure, applied directly on it by tents. Dupuytren also tried, but was compelled to abandon more forcible pressure on the *eperon.* Since Dr. Gross wrote, Mr. Trant communicated to the Surgical Society of Ireland an extremely ingenious instrument, which seems admirably suited for accomplishing everything that can be achieved by compression on the *eperon;* this well-devised contrivance presents the great advantage, that while very considerable pressure can be exerted on the *eperon,* there is not the least danger of the force applied tearing the adhesions of the bowel to the abdominal parietes, because the bowel is pressed from within outwards against the parietes of the abdomen, with a force equal to that exerted on the *eperon.* This instrument perfectly succeeded in Mr. Trant's hands in one case of artificial anus, and we have little doubt that future experience will confirm its utility. (Dublin Med. Press, vol. xiii, p. 305.)*

Dr. Gross mentions the unsuccessful attempts of Bruns, Liotard, and Blandin to obliterate an artificial anus by approximating with sutures its edges previously rendered raw by the knife or by caustic. Cooper, in his folio work on Hernia, mentions two other similarly unsuccessful attempts. Dr. Gross also notices the successful autoplastic operations of Mr. Collier and M. Blandin, an unsuccessful attempt of the same kind by Velpeau,

* As an adequate idea of this ingenious instrument could scarcely be conveyed by a mere verbal

and also the method of the last surgeon, which consists in uniting by suture the raw edges of the opening, and facilitating the approximation of the parts by making a semi-elliptical incision on each side, and about an inch from the preternatural orifice. There are some other very interesting cases in which artificial anus has been latterly cured by autoplasty, either alone or combined with other methods, and which show what perseverance may accomplish in remedying this lamentable infirmity. Dr. Gross quotes Reybard's treatise on artificial anus, but does not advert to his cases, further than to say that two of his patients rapidly recovered after the application of his enterotome, whereas of his three cases one only was cured by the enterotome, a second remained unimproved, and in the third, which recovered after a series of autoplastic operations, the enterotome was not used at all. This last case is the remarkable one already

description, we copy, by permission of the proprietor, from the 'Dublin Medical Press' the accompanying figures, which both represent the instrument itself, and also exhibit it as applied in the treatment of artificial anus.

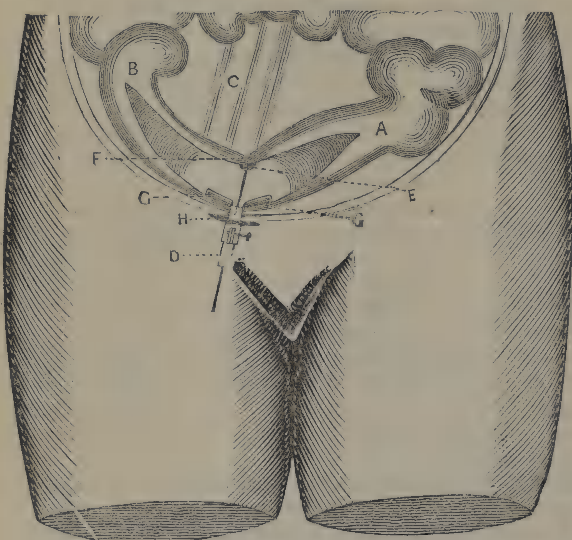

Fig. 1.

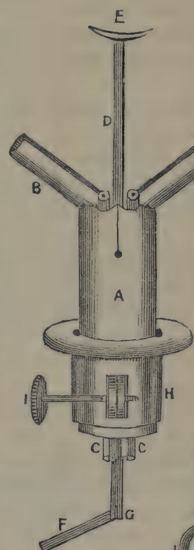

Fig. 2.

Fig. 3.

Fig. 1 represents the position of the eperon after the third application of the instrument with it *in situ:* A. The upper or ventricular portion of intestine. B. The anal or lower portion of intestine. C. The mesentery, by its retraction towards the spine, assists the propeller E in removing the eperon F from the preternatural opening. D. The instrument introduced into the cavity of the intestine, with the propeller E pressing back the eperon F towards the spine, whilst the expanded wings GG on the inside, and the shield H on the outside, act as a forceps in retaining the intervening parts in close apposition, so as to prevent any separation of the anterior surface of the intestine from the parietes of the abdomen, whilst the propeller is pressing back the eperon towards the spine.

Fig. 2 represents the instrument with its wings expanding, and the propeller pushed forward, as in the cavity of the intestine, during application. A. The body. BB. The right and left wings. CC. The screws by which the respective wings are moved. D. The propeller. E. The concave extremity of propeller. F. The end of propeller by which the concave extremity is pressed against the eperon. G. Screw for fixing propeller. H. Shield which is moveable. I. Screw for fixing shield.

Fig. 3. J. The key by which the different screws are turned.

alluded to, in which a ligature was tied round the colon. The artificial anus occupied that bowel, and was upwards of an inch in diameter. A large flap of skin was dissected from the abdomen and applied over the opening, where it perfectly adhered, except at one point, where a fæcal fistula about four lines in extent formed. The edges of this fistula were freely pared and united by suture, but union was prevented by the issue of fæces. M. Reybard was now about to abandon the patient, when he observed that the skin of the abdomen was thrown into large transverse folds when the trunk and thigh were mutually flexed: he then pared the edges of the fistula again, applied no sutures or adhesive plaster, but kept them in contact solely by making the patient sit with the trunk flexed forward, while the thigh was maintained flexed on the abdomen by a bandage passing under the ham and round the shoulders and loins. This position was observed for one month, when a very minute orifice only remained, which gradually healed under applications of nitrate of silver, and the patient finally and completely recovered. M. Lisfranc was less fortunate in an apparently more favorable case. In operating on an umbilical hernia he cut away about fourteen inches of gangrened intestine, an artificial anus resulted, and seven months after the operation he applied Dupuytren's enterotome, (the only example we recollect of its being employed in an umbilical hernia, with the exception of a case of M. Robert's*), and four months subsequently it was applied a second time; the fæces now resumed their course through the rectum, and a mere fistula remained at the umbilicus. After several months, the edges of the fistula were pared and united by the twisted suture, but adhesion failed from erysipelas: after three months an autoplastic flap was applied, but erysipelas again prevented union; eight months subsequently the twisted suture was again applied, but union again failed, though no erysipelas set in. (Gazette Méd. de Paris, 1843, p. 543.) M. Blandin, in a child who was wasting from an artificial anus high in the small intestine, after in vain trying pressure on the *eperon*, with Dessault's tent, and a special apparatus contrived for the purpose, applied the enterotome, after which the orifice contracted considerably, its edges were now pared, and the twisted suture applied, with the result of very slightly diminishing the opening; three months subsequently the edges of the fistula were again refreshed and approximated by means of two "*tampons articulés*" of Dupuytren, without using any suture; the apparatus was kept on for ten days, when union was complete, and remained permanent. (Bulletin de l'Acad. Roy. de Méd., Nov. 1844, t. x, p. 110.) In the following case M. Jobert resorted to a contrivance different from any of the foregoing. The patient had an artificial anus in the left groin, the result of strangulated hernia, which had existed nearly two years. Before he came under M. Jobert's care the enterotome had been applied several times without any amendment, and ineffectual

* In M. Robert's case the enterotome was applied within three weeks after the operation for strangulated hernia had been performed, as the patient was sinking rapidly from defective nutrition, and notwithstanding the very disadvantageous circumstance of so very early an application of the enterotome the case did well, and a slight fæcal fistula only remained. M. Voillemier, in a case in which the patient was similarly wasting away, succeeded in supporting the strength of the patient by nutritious enemata, until a sufficient period had elapsed from the performance of the operation for strangulated hernia, to allow of the enterotome being applied without incurring more than the ordinary risk consequent on the use of that instrument. The effects of nutritious enemata are, however, so very capricious, if we may use that phrase, that their beneficial influence cannot be calculated on with certainty under similar circumstances.

attempts had been made, after touching the borders of the orifice with caustic, to unite them by the pressure of a padded forceps, and subsequently by the twisted suture; repeated applications of the cautery had also proved fruitless. Under these circumstances, M. Jobert endeavoured to remedy the infirmity by applying an autoplastic flap, but hemorrhage came on, the summit of the flap became gangrened, and the residue contracted towards its base. After a suitable lapse of time, the actual cautery and nitrate of silver were repeatedly applied during a month, and produced some slight diminution of the aperture. Finally, M. Jobert performed the following operation: a portion of skin was dissected away at each side of, but at some little distance from, the artificial anus, and the raw surfaces were then brought into contact in front of the preternatural opening, and secured by six points of twisted suture. The parts thus united formed a kind of bridge in front of the artificial anus, with a re-entrant angle above and below at their point of contact. A little fæcal matter oozed from the lower angle on the second day, and from the upper angle on the third day, but the parts united in front of the artificial anus; the oozing of fæcal matter from beneath them gradually lessened, and after some months the cure was perfect. (Bulletin de l'Acad., etc. Sept. 1845, p. 1030.) Dr. Gross does not notice a particular mode of applying the actual cautery in artificial anus, recommended by Dieffenbach, which consists in destroying the edge of the bowel where it joins the integument, and also a considerable portion of the mucous surface within the orifice, without acting on the skin itself; in this way Dieffenbach closed an artificial anus, resulting from a lance wound, after an autoplastic flap had failed, (Kleiner's Repertorium, Nov. 1835,) and Dr. Fingerhuth healed two intestinal fistulæ consequent on abdominal abscesses caused by blows. (Wochenschrift fur die Gesammte, Heilkunde, No. 6, 1836.) Dr. Gross just mentions a very extraordinary case in which M. Roux endeavoured to remedy an intestino-vaginal fistula, but he does not mention the particulars, which are, we believe, without precedent. M. Roux opened the abdomen, divided the intestine in two places, immediately above and below its adhesion to the vagina, as he thought, and united the extremities by three points of suture, according to Lembert's method; the operation lasted an hour and a quarter, and the patient died in thirty-eight hours. On dissection it was found that the ileum had been divided about two inches and a half above the artificial anus, but instead of dividing the small intestine below that point, the operator had severed the sigmoid flexure of the colon, and sewed the small intestine to the lower extremity of the *upper* portion of the colon. (Recueil de Mém. de Chirurg. et de Pharm. Milit., t. xii, p. 213.)

We had hoped that Dr. Gross would have considered the question of the comparative curability of artificial anus arising from a wound and from strangulated hernia; we think that the greater difficulty of remedying the former has been assumed on far from sufficient grounds, and it is obvious how much this assumption must tend to influence the surgeon in the practice he adopts in certain cases of wounds of the intestine. But this point we cannot pursue further at present.

The length and minuteness of our examination of Dr. Gross's work sufficiently shows that we deem it an important one. The subject of wounds of the intestine required examination, and indeed we must say that, in our opinion, it still requires it. Dr. Gross has made a valuable

contribution to this branch of surgery, but the field of inquiry he has selected is far from being exhausted. Dr. Gross's experiments constitute the most valuable part of his work, and we look on them as the most important body of facts of the kind yet published. Dr. Gross evidently possesses a high degree of experimental skill, and he as evidently is endowed with the rare and valuable faculty of accurate observation: nothing can be clearer than his account of the results of his experiments. We take our leave of Dr. Gross with thanks for the information, and even more for the materials for reflection he has afforded to us.

In the foregoing brief observations on artificial anus, the various methods of treating that infirmity have been alluded to, without entering into the details of the practical application of any of them. We think it may be useful to append a summary account of the methods recommended by Dieffenbach, in the 62d chapter, of the first volume of his work on 'Operative Surgery,' as his experience is derived from the treatment of a very considerable number of cases.

Dieffenbach states that he has found the application of pressure on the ridge intervening between the two extremities of the bowel, of the greatest advantage in the treatment of many cases of artificial anus. He applies it by means of

"A crescentic ivory crutch of the thickness of a quill, the staff of which passes through the pad of a gum-elastic truss, and is prevented from falling through by screwing on a small plate. The crutch is so placed upon the partition that one horn passes into each intestinal opening, the staff is passed through the pad, the small plate screwed on, the truss applied and fastened; and, lastly, the long staff projecting through the pad is pressed backward by a strap which runs over the small plate, and is fastened by a buckle. The pad lies immediately on the skin as in a common truss, and is protected by laying beneath it strips of dressing. The bandage is removed several times daily, in order to clear away the excrements, and, when the parts are cleaned, the crutch and truss are reapplied. Gradually the strap is to be buckled a hole tighter, in order more strongly to depress the partition." (Dieffenbach, p. 705.)

While this instrument is being worn, an attempt should be made to restore the function of the lower portion of the intestinal canal, which may have been partially or completely suspended, perhaps for years, and Dieffenbach says enemata of ale and porter are remarkably efficacious in fulfilling this indication. This treatment to be conducted safely, must be conducted slowly, for a hasty increase of pressure produces pain and inflammation, and obliges the treatment to be suspended for some days. If we thus succeed in obliterating the ridge, and bringing the orifices of the two extremities of the intestine opposite to each other, so that their posterior walls shall form a continuous surface, it still remains to close the external opening, and this Dieffenbach effects by the application of the cautery and of the running suture, as described in the following extracts from his work.

"*Application of the cautery.* The bandage is removed, the parts cleaned and dried, a roll of charpie is placed in the opening of the gut for its protection, and the extreme strip of intestine, just where it is united with the integument, is destroyed by a small bean-shaped iron, the narrower rounded ridge being applied. The iron is carried round slowly, a second is taken, and again carried

round, so that a burnt ring two lines in breadth surround, the abnormal anus. The part is covered by a narrow roll of charpie, and then the bandage is reapplied without the crutch. After the separation of the slough a circular granulation springs up, the ends of the intestine sink still deeper, and the excrements pass off more freely by the natural channel. After a fortnight a surface of at least an inch of the skin around the opening is to be cauterized, and its inner edge also slightly so." (Dieffenbach, p. 706.)

Lint is applied, first a mild, then an irritating ointment is used, the patient well nourished, and an enema of beer given daily. By the process of cicatrization the opening is soon converted into a small fistula, which another cauterization and the pressure of a soft truss-pad suffices to close.

"*Application of the running suture.* The running suture is necessary after treating a very large preternatural anus in the way just described by the crutch or cautery, when the external loss of substance is very great, and the cautery has not produced the concentric cicatrix so efficient in closing the opening. A strong curved sewing-needle, armed with four or six threads of waxed silk, is carried around the opening, a third or half an inch from its edge. The needle is passed in and out, and again in and out, always being again passed into the same place where it was brought out, until the opening is completely surrounded. Six such punctures are generally necessary to encircle the opening completely. Both ends of the threads hang out from the first puncture, and are tied together with a loop upon a small roll of plaster laid below them, and thus the opening is either completely closed, or only narrowed according to the degree of tension. The threads should never be tied together tightly, because they would then cut through in a few days, as they lie beneath the skin, and only surround soft parts. As they become loose they are tied somewhat tighter, and this is repeated until the parts are cut through. Contraction of the opening is sometimes very considerable after the first suture, still a repetition after the cicatrization which succeeds, and another cauterization of the edges, is necessary for its perfect closure." (Dieffenbach, p. 708.)

When artificial anus results from a penetrating wound of the abdomen, from gangrene of a portion only of the circumference of the intestine, or from an abscess in the parietes of the abdomen opening both externally, and into the bowel, there is often great difficulty in closing the aperture, especially when the small intestine is involved. In these cases the prospect of effecting a cure is most favorable when the disease has existed for a considerable period, and all the parts connected with the preternatural opening (particularly the adhesions with the intestine) have become relaxed and extended; here, of course, the crutch is not applied as there is no ridge to be obliterated, and the cure must be effected by the actual cautery, the running suture, and autoplasty; singly or successively practised, according to the circumstances of the case. The following are Dieffenbach's observations respecting the application of those methods in this class of cases:

"A small hook-shaped iron red hot is placed upon the edge bordered with the mucous membrane, and this edge is destroyed; then the point of the hook is passed in through the opening, and some lines' breadth of the internal circumference is burnt. A roll of charpie, the middle of which is tied together with thread, is introduced for the protection of the neighbouring parts, and held fast by the thread. The opening is covered with charpie and plaster, and when granulation begins to show itself around, the wound must be gradually drawn together by strips of plaster. Afterwards the cauterization is repeated, especially around the external edge and border, and if the opening does not speedily contract,

the running suture is applied around it, between the intestine and integument. There is more necessity for transplantation of skin in this than in any other form of preternatural anus, particularly when hard cicatrices surround the opening, as these render replacement impossible, and other means are ineffectual. In some cases I have perfectly cured with the cautery alone even when excision of the edges and suture, lateral incisions and transplantation of skin, either as a bridge or in flaps, have failed. One of the most difficult cases of preternatural anus occurred in a Polish officer, who, in battle, was wounded in the abdomen by the lance of a Cossack, the transverse colon being injured. After many fruitless attempts to cure, after transplantation of skin had proved unsuccessful, the application of the cautery succeeded, the destruction of the mucous membrane around the edge and on the internal circumference contributing most essentially to the cure." (p 713)

Though Dieffenbach considers pressure on the ridge by means of his crutch the best and safest mode of proceeding when applicable, viz. when the partition lies at some depth, and the ends of the intestine form a right or an obtuse angle with each other: he considers that Dupuytren's enterotome may be indispensable when the ends of the intestine lie parallel to each other, like the barrels of a double-barrelled gun, or when the partition is tense and hard, and reaches to, or extends beyond, the external opening. Nothing requiring notice is said respecting the application of this instrument. We may, however, mention that, in Dieffenbach's opinion, none of the numerous modifications of Dupuytren's enterotome have increased the utility of the instrument, while many of them have impaired it; this latter observation particularly applies to all the modifications intended to remove a circular or oval portion of the partition, a proceeding which, however apparently advantageous, experience has shown to be prejudicial, because the concentric contraction consequent on the cicatrization of the circular aperture renders the opening much smaller than when Dupuytren's enterotome has been applied; but even his instrument may cause such great contraction as to render the disease incurable. Dieffenbach objects to Dupuytren's practice of opening fistulæ connected with an artificial anus, and removing any hard irregular cicatrices surrounding it, because loss of substance is the result, and new cicatrices form; he says, he has found it much more advantageous to dilate the fistulæ with sponge tents, and soften the cicatrices with poultices and embrocations.

Artificial anus, connected with an unreduced hernia, is a rare affection, but Dieffenbach appears to have met with several cases of it. The treatment he adopts is modified by the circumstances of the case. 1. The cure of the preternatural anus may be undertaken *without attempting to cure the hernia;* when the hernia is very large and adherent in several places, when one wall only of the intestine is perforated, when the intestine is not contracted, and the inguinal canal is so large that after closure of the abnormal anus, the excrement can pass freely through the protruded portion of the gut.

After the patient has been prepared for a long time by horizontal posture, spare diet, poultices, and beer enemata containing carbonic acid, cauterization of the opening is performed with the red-hot iron.

"A roll of charpie is first passed into the opening, and then its lip-like border of mucous membrane is destroyed with a small conical iron. Then a large iron, of the form of a piece of money, is taken and applied flat, so that the opening is

under its middle. When the opening, for example, is of the size of an English silver twopenny piece, the round cautery which is applied should equal in circumference that of a shilling. The cauterization of the circumference must be slight, so that only a layer of skin is destroyed of the thickness of strong letter paper. When the operation is finished, the roll of charpie is removed from the canal of the intestine, and the whole of the parts are covered with some handfuls of soft cotton and a compress, over which a broad suspensory or T bandage is applied. The bandage is as frequently renewed as it is soiled, the patient kept on low diet, and a clyster administered daily at least. The granulations which spring up after the separation of the slough are to be protected from the excrements by dressing, afterwards a stimulating ointment is used, assisted by caustic, and any small opening which remains is closed by the running suture and repetition of the actual cautery. This method is best adapted for large scrotal herniæ. When there are several small openings, they are treated in the same manner, but when it can be determined that some of the openings are in the lower part of the intestine, or that the smallest portion of excrement passes through them, they are to be first closed, as their cure is more easy." (pp. 723-5.)

Cure of the preternatural anus of a hernia, and of the latter, can only be affected when the scrotal hernia is moderately large, when it contains only a simple loop of intestine, and when the ring is small. Only a strong young subject is fit for the operation.

The condition here is as follows. The contents of the intestine pass from the abdomen into the protruded gut, and then escape through the preternatural anus. The gut at the situation of the opening is large, the other part, particularly when it passes through the ring, is contracted, and when this state has existed long, it is so narrow that only a thick catheter will pass. The intestine frequently lies free in the ring like a reducible hernia, but sometimes adhesions have formed. When the intestinal opening communicates with that in the skin, the skin, sac, and intestine are all adherent to each other.

Treatment when the hernia is moveable. When the loop of intestine is only adherent at the situation of the preternatural anus, and the lower part of this loop is still in some degree wide and extensible, we must endeavour to dilate the rings by division with the knife without opening the sac. Then by gentle manipulation the upper part of the protrusion is gradually returned. This is possible without the use of the knife, if the rings are not very small. Before the wound has become cicatrized, the cautery is to be applied as before directed, and as the opening is diminished the intestine is pushed farther back into the abdomen. When the opening is perfectly closed, the intestine is replaced as far as possible. The portion of intestine which adheres to the integument is drawn outwards as a diverticulum, afterwards the cellular connexions become lengthened, and the hernia is prevented from reappearing by a truss.

Treatment of preternatural anus in a small adherent hernia consists—1, in destruction (verödung) or removal of the adherent loop of intestine; 2, in forming a common preternatural anus; 3, in healing the latter. We shall follow our author's description of this operation, as he says he has thus exactly detailed the state and treatment of a man 28 years of age, who, in a year, was cured perfectly of a scrotal hernia of the size of a fist, and of a preternatural anus which opened into it.

"The operation commences with division of the scrotal integument, and opening of the hernial sac, just as in the operation for hernia. The opening being

carried to two thirds of the length of the tumour, the spot is reached where the loop of intestine passes through the ring. Without separating the adhesions at the ring, their union with the neck of the sac is to be loosened. When this is completed, the intestine is cut through close before the ring, first dividing the part in which the anal opening exists, and then, at the same height, the part which passes into the abdominal cavity. If the two intestinal extremities are not closely adherent to each other, their sides turned towards each other are to be united at their edge by a thread passing between their serous surfaces. The separated perforated loop, with contracted canal and hypertrophied walls, is loosened and removed from the hernial sac, and also any degenerated portion of the sac and scrotal integument.

"After the bleeding has ceased the cavity is filled with loose charpie, strips of plaster laid over it, and the whole covered by charpie, a compress, and T bandage.

"The bandage is removed as often as it is soiled, free evacuation of the excrement attended to, and after some weeks, pressing backwards of the intestinal extremities into the abdomen is effected by balls of charpie laid against their openings. The condition is then the same as if the loop of intestine had been lost by strangulated hernia, and a common preternatural anus had been formed. After some months, when the hernial sac is obliterated, when there is a free passage to the intestinal extremities, when the adhesions have become extensible, then the preternatural anus is to be treated exactly in the manner before described, by the application of the ivory crutch and elastic truss. If the inferior extremity be considerably contracted, it is to be gradually distended by tents; but if the crutch can be first brought to bear upon one of its walls, its gradual dilatation will be thus readily effected. Lastly, when the two extremities have been pressed deeply backwards into the abdomen, so that they lie with their openings towards each other, the excrements begin to pass by the anus, and the cure approaches. Closure of the external opening in the scrotum here offers little difficulty, as the intestinal extremities lie at a considerable distance from it, and no integument has been lost. The actual cautery is here seldom necessary, as the use of caustic will close any remaining fistula." (pp. 726-7.)

"In case the separated adherent loop of intestine should be left, in order to avoid an extensive wound, it would be advisable to destroy (veröden) its canal, by drawing a thick cotton wick through it, and, if necessary, this may be covered with an irritating ointment." (p. 728.)

The transplantation of an autoplastic flap is only required in the treatment of artificial anus, when the skin surrounding the orifice is hard, inextensible, and altered in structure; if it be soft, pliant, and extensible, the opening, even if large, may be closed with the running suture. If a ridge exist, it must, of course, be first removed, either by pressure or by the enterotome. It is unnecessary to describe the details of the operation; but Dieffenbach insists that the transplanted flap shall not completely obstruct the exit of the fæces in the first instance. His earlier operations sometimes failed from neglecting this precaution, and he, therefore, fastens the flap by suture in two thirds of its circumference only, the anterior border being left free to allow the fæces to escape. When the operation is completed, water is injected under the flap, and then charpie introduced beneath it, and the whole supported by strips of adhesive plaster, the free edge of the flap, however, being left uncovered. The chief point in the after treatment is to endeavour to prevent the passage of fæces for as long a period as possible, which is sought to be effected by perfect quiet, dry food in small quantity, small doses of opium, and small enemata of castor oil and camomile. If the dressings, on careful examination, do not appear soiled, they are not disturbed for three or four days; if soiled, they must be immediately renewed. The sutures should

be removed about the eighth day. When the attached portion of the flap has completely united, the issue of fæces should not be opposed by strong pressure, but merely impeded by charpie, and endeavours made to heal the opening by granulation, by the application of resinous ointments, &c.; generally the opening gradually contracts to a mere fistula, which, by proper management, finally heals. If, however, the flap unites imperfectly, it is very apt to contract towards its base; which must, as far as possible, be counteracted by the judicious application of pressure, and keeping it on the stretch by adhesive strips of plaster, while granulation of its edges is at the same time promoted by resinous ointment. Fomentations with camomile sometimes appear to be extremely useful.

The formation of *a bridge-like flap* is preferable when the skin on, at least, one side of the opening is healthy and extensible—the orifice is circumscribed by two long concave incisions, vertical, transverse, or oblique, according to the existence and position of cicatrices, and the included skin is smoothly dissected away. An incision is now made parallel to one of the wounded edges. The strip of skin thus isolated must be one third wider than the opening. Its inner border is fixed with hooked forceps, and it is dissected from the subjacent parts in the interspace between the two incisions, and when thus completely free, except at its two extremities, it is drawn over the opening and attached to the opposite edge. The after treatment is the same as when a pediculated flap is transplanted.

We have space for but little comment on the foregoing summary of Dieffenbach's practice. We regard simple pressure on the ridge with great apprehension, because of the absence of a *point d'appui*, on the wall of the intestine, where it is attached to parietes of the abdomen, whence the risk is incurred of the adhesions in this situation being torn. But Mr. Trant's instrument (which, as we happen to know, Dieffenbach has seen) is free from this objection; for it supplies the *point d'appui*, and, consequently, there is no danger of the bowel being torn from its attachment. We cannot pretend to deny that the running suture may often be useful, and sometimes indispensable; but we are strongly inclined to the opinion that Dupuytren's compressor would in many, perhaps most, instances efficiently and more conveniently replace it. The use of the actual cautery is, we are sure, quite too much neglected by British surgeons. The extraordinary tendency of the cicatrix of a burn to contract is a familiar fact; whence we may appreciate the amount of contraction that may result from a *circular* cicatrix thus produced.

We always feel much hesitation in pronouncing any opinion respecting a point on which we have had no practical experience; but we must confess that we have detailed Dr. Dieffenbach's proceedings for the cure of a hernia concurrently with that of artificial anus, as a curiosity of operative surgery. When Dr. Dieffenbach published an account of his outrageous operation for stammering, by sub-mucous section of the base of the tongue, he said it was an operation not to be undertaken save by those endowed with the "operative temperament;" and we think the temperament in question should be possessed in very full measure, and, perhaps, coupled with the absence of some other qualities which we need not specify, to induce a surgeon to imitate the proceedings detailed at the commencement of our 35th page.

ART. II.

Nosologia Positiva. Scritta da VINCENZIO LANZA, Dottore in Medicina, Professore nella Cattedra di Medicina Pratica della Regia Universita' degli Studii di Napoli, Medico Primario e Direttore della Clinica dello Spedale della Pace, &c. &c.—*Napoli*, 1841-2.

Positive Nosology. By V. LANZA, M.D., Professor of the Practice of Physic in the Royal University of Naples, &c.—*Naples*, 1841-2. 2 vols. 8vo, pp. 608-760.

WE feel that we have been too long in directing the attention of our readers to these remarkable volumes; because although we have before us only two of a work which it is anticipated will extend to five, still it is of a character which, we humbly conceive, claims for the author the general encouragement of his brethren, and which promises, in return, to be of signal service to the profession, especially under the peculiar circumstances in which it is now placed. If, at the present day, there is one feature of medical literature more striking than another, it is its superfluity and redundance. With much, very much, that is excellent and valuable, there is vastly more that is trashy and contemptible. How many are the cases published which have no existence save in the distorted views of their authors? how many isolated facts brought to light with almost the assured certainty of being lost? and how many the ingenious speculations which scarcely outlive the day that gives them birth? when to these are added the many absurd professional fancies and fallacies which prevail among physicians and their patients, regarding the fashionable medical follies of the day, it must be allowed that there is here an amount of credulity and misdirected labour which with infinite benefit might be entirely suppressed. But how, it may be inquired, is this to be accomplished? We believe that, effectually, it never will;—inasmuch as the cause lies deep in human nature, and will ever and anon, in revolving cycles, be forcing itself into notice. All that can be reasonably expected is that the evil may be restrained though not suppressed; and we believe that works such as the one now before us, by enforcing better principles and especially showing a better example, are calculated efficiently to promote this most desirable end.

The range of Professor Lanza's work is most extensive, including practical medicine and medical surgery, which, scientifically considered, can never be dissociated; and its great object is to withdraw the healing art from the dominion of hypothesis "so that becoming positive" it may advance *pari passu* with the other natural sciences, and attain that elevation they are now so rapidly reaching. When, some half dozen of years ago, we had occasion to bring the subject of the science in Italy under the notice of our readers, we stated that "the Italian physic was still guided by the spirit of hypothesis;" and it would certainly be interesting if the evil in that country, long unchecked, and so rendered intolerable by its profusion, were there first to receive a check which in its influences would extend over the rest of Europe, and to Britain, which of all other kingdoms seems at present most to require the pruning of such re-

dundancy and prurience. Nor is it the general object and end only of this comprehensive work that we would commend. The execution, as partially exhibited in the volumes before us, has been the labour of the author's lifetime, not only among his patients, but in his study; and with the most intimate knowledge of the sentiments of his predecessors and contemporaries, there is conjoined a remarkable amount of independence and originality. His views frequently startle by their boldness, as much as their novelty, and are withal founded upon a most careful scrutiny of Nature, and a most patient observance of her laws. Seeing the eminent author prominently putting forward these claims, we shall take some pains to represent the ground on which they rest; though, from the perfect originality of the views, and, we venture to add, the provincial peculiarities of his style, it is not always easily done. At the same time, we trust that our readers will not suffer from any deficiencies of ours, nor fail to derive that benefit which we believe that a patient and careful perusal of the pages themselves would assuredly afford.

The preface of a work may be considered in the light of a personal introduction, wherein the author makes his salutation and first impression on the reader. Under this conviction we give Professor Lanza's nearly entire.

"Can medicine," demands our author, "ever be rendered one of the positive sciences, more especially in its application to the cure of disease? Is it necessary ere the healing art attain a character in every way satisfactory and reasonable, that the essence of disease, and the modus operandi of morbific causes, and the appropriate remedies, be ascertained? Lastly, may the processes of ratiocination in medical science from elements which are necessarily obscure, but yet probable, be reduced to conjectures which are simple though isolated? Or, on the contrary, is it necessary, as a first principle, that we start with hypotheses more or less numerous,—hypotheses which are themselves to constitute the system of medicine? The important questions associated with these interrogatories we know, from the writings of Celsus, engaged the serious attention of the ancients, and it is high time now, as most reasonable, that, though long neglected, medical men should again revert to their most serious consideration.

"Hippocrates, as sufficiently appears from his first aphorism, was quite alive to the difficulties which must be encountered in placing the healing art among the positive sciences; but throughout his entire writings he shows that he did not consider the object unattainable; and occasionally he propounds principles in perfect keeping with the design. Celsus, in his examination of the Greek writers, clearly propounded what physic as a positive science ought to be; and accordingly, in the preface of his work, requires the Empirics to accumulate not only clear and positive medicine, but probabilities also concerning the hidden seats of diseases, their anatomico-pathological form, and the influence of their latent causes; and to reject not only from their minds, but also from the art, all that is really obscure; so that idle speculation on the essence of diseases, and the operations of causes and remedies, might be discouraged. But though this celebrated author inculcated those principles in the abstract, he did not steadily observe them in such of his writings as have reached us; nor did he proceed regularly from the lucid description of disease to its cure; but in his interesting monographs often advises remedies without assigning the reason of their selection. Hence, although we may concede to Celsus an apprehension of what physic should be as a positive science, it is certain that he left no precepts for its cultivation in this particular manner.

"Galen was an advocate for the doctrine that the science of medicine should be

based on hypotheses; commencing with disquisitions concerning the essence of disease, and the peculiar agency of causes and remedies, and that all medical reasoning should proceed from the combination of such hypotheses into systems. The influence of this celebrated physician was great, and has been felt injuriously even to the present day.

"In the progress of time physicians arose who may be designated prognosticators, or the predicating sect—*predicatori*—and who, defending experience whilst they abused systems, were useful in their way, inasmuch as they showed by facts how pure medical science, free from all hypothesis, might supply positive facts whereby the science might be enriched.

"To render medicine, however, one of the positive sciences it is not enough that old speculative systems should be condemned and abandoned; a scientific method must, moreover, be discovered, whereby, when once placed in this desirable position, it may be there retained, supplying rational principles to guide us in the art of cure. Our own age especially requires the adoption of such a method."

Our author's life, he informs us, has been devoted to this object, and he believes he can somewhat promote its advancement. A work of this kind, however, he remarks, is a task not for a single individual, but for many, and their co-operating with devotedness and perseverance. His labours, which he flatters himself will throw new light on the science, as now presented to the public, are in such a shape that every one may correct his fallacies. The critic becomes an improver, and the names of master and scholar are no longer fraught with those injurious influences which have for ages been associated with them. The wisdom of the professional chair, and the erudition of the author's pen, may hereby be readily compared with the practice of the palace, the cottage, and the hospital; and thus every cultivator of the science, in whatsoever situation he may be placed, may assist in promoting that improvement for which their united labours are so essentially required.

The FIRST BOOK of our author's work is upon THE PHILOSOPHY OF MEDICINE, a most interesting topic; and is divided into eleven chapters. It embraces discussions upon *nosology*, *natural medicine*, the errors of *transcendental medicine*, of *empirical*, *methodical*, *hypothetic*, and *eclectic*, upon the *nullity and worthlessness of the homœopathy of Hahnemann*, upon the foundation of *positive medicine*, and its *necessary criticism*. This bill of fare extends over between fifty and sixty pages; and we shall now supply a short analysis of three of the most interesting chapters. This will familiarise the reader with the distinctive characters of M. Lanza's style and manner of thought. Beyond doubt, he has most praiseworthily spent many a weary hour in studying our science and elaborating his opinions, so that many new views and incentives of thought are furnished for consideration.

CHAPTER I. The first chapter of the first book is entitled, *The necessity of a common agreement* (*consentimento*) *among physicians, in order that medicine may become one of the positive sciences.* This agreement, beyond all doubt, is a great desideratum, but is opposed by obstacles which by many will be thought insurmountable. Not so, however, in the apprehension of our author. He defines medicine to be the science which deals with the bodily infirmities of our nature; and it becomes an art when it adapts the means it possesses to assist Nature in overcoming disease. The means employed by the ancients for this important end

they divided into hygienic, pharmaceutic and surgical. The word *indications* expressed the changes they wished to accomplish on the disorder; and by remedies which, according to circumstances, were either indicated or contraindicated. After the enumeration of other generalities of this kind, the author continues:

"Let us now inquire into the reason why medicine is one of the least certain of the sciences. It arises not from its own proper quality, but from the position it is artificially forced to hold. Other sciences have conventional limits for their cognizance, all exclude from their pale data that are uncertain, and rather than admit a doubt, throw the blame upon the art; and these are canons which none have thought it worth their while to dispute. Very different, however, is it with physic. Men are so anxious about their health that, rather than be sick, or run the risk thereof, they require from their medical advisers a statement of the probabilities to which they are liable, and insist upon protection and relief in the midst of doubts;—*melius remedium anceps, quam nullum.* Quadrating by these requirements, physicians are absolutely forced to admit conjectures into the science,—to deal with cases where the indications are most obscure, and adopt a treatment which is little more than tentative. In this view, physic can be little more than a conjectural science and dubious art, because what is required from it is far more than is required from any other of the sciences.

"Medicine, as a positive science, must address itself solely to the human intellect, and obtain conviction from evidence alone; it must peremptorily reject all dogmata which are incomprehensible, or beyond the grasp of the mind; whilst it retains those conjectures which will prove serviceable to the art. That individual is in ignorance regarding what he affirms who maintains, that medicine can then only be positive when it entertains nothing but what is certain. By excluding probability the indications of the art would be abandoned, and the sick would be much dissatisfied with the sacrifice. Positive medical science must, therefore, recognize the science of probabilities; such probabilities as can have a correct value attached to them, and with neither more nor less of what is their due. This condition is absolutely essential to the employment of conjecture; which, moreover, must never be employed in the support of mere hypothesis. And it must be remembered that although probabilities are thus admitted as necessary elements of medicine viewed as a positive science, still it is nothing less than the possession of what is of real and positive value, resting on the broad basis of enlarged experience and observation." (pp. 12, 13.)

The author regards hypothetical medicine in a very different light. In it the speculator fancies that he entertains the idea of a fact, which he regards as the common exponent of numerous other facts, and of the relations existing between them. Such a system is called hypothetical or systematic (an appellation which we conceive to be very unhappy):—hypothetical, because its worth is measured not by its real value, but by that which is ascribed to it; and systematic, because all the conjectures being alike supposititious, if one be erroneous, the whole fabric totters to its fall.

This clearly is the abuse, not the use of system.

"Thus," the author continues, "the different value of positive and hypothetical medicine appears as it regards the science and the art, and all who are engaged in cultivating them. The votary of medicine as a positive science, like the cultivator of the natural sciences, far from avoiding the difficulties which present themselves, investigates and overcomes them; sifting every conjecture till doubt gives place to certainty. The hypothetical physician, on the other hand, far from boldly meeting and overcoming such difficulties, avoids them; and if necessarily obliged

to encounter them, endeavours so to set them aside, or smooth them down, that they shall not oppose his suppositions, or contradict his system. Positive medicine is that part of natural medicine which has been accumulated from age to age; hypothetical is artificial, has no prototype in nature, is the creation of man's fancy, the imagery of his idolatry. In a word, medicine as a positive science is a transcript of historical truth; hypothetical medicine, a romance. In positive medicine, art rules the science, since, from the prognosticated result, and the accomplishment of the expressed indication, every anticipation is realized; in hypothetical medicine, on the contrary, speculation rules art; for whatever be the issue, the practitioner has in view not so much the interests of his patient, as the protection and maintenance of the mere child of his fancy." (p. 14.)

Such is a partial analysis of the first chapter of this treatise. We proceed to the second, which more distinctly develops the author's views.

Chapter II. *The scientific discrimination of the facts of nosology.* In our endeavours, says the author, to create a philosophical system of medicine, and to render it one of the positive sciences, we shall attempt, first, to enumerate exactly the facts derived from clinical observation, and determine afterwards their scientific value. Disease, in his opinion, is a compound entity which, on being analysed at the bedside of the patient, may be resolved into *ten* facts, neither more nor less! Seven of these facts are possessed of a positive value, because they are directly, or indirectly, subject to the senses. They are partly cognizant to sense, partly the product of sage and indisputable conjecture. And they are the more entitled to be arranged as positive, because they may be studied as they exist in nature, and without the interposition of the slightest hypothesis. The other three facts may be designated transcendental, supposable, or hypothetical, because they are beyond the actual reach of the senses, and are merely inferences which must be drawn from supposition. Many will here be tempted to inquire if M. Lanza, notwithstanding his decided opposition to rationalism and hypothesis, has here escaped that danger he so assiduously exposes. We confess we are not prepared to give a categorical answer to this surmise. But sure we are that it is not without the most careful investigation and consideration that he has reached his conclusion. We shall notice his facts in order.

First fact. The physiological state of the patient. Physiological phenomena present themselves both in those in health and in those labouring under disease; and the variety they exhibit must ever command the attention of the practitioner. The physiological condition of the patient always exhibits a combination of marked appearances, which may be readily discovered upon the first examination of the patient and his disorder; and the relation between the physiological condition and the nature of the disease may usually be satisfactorily established. The conclusions are always conjectural, frequently not very evident; sometimes only probable, and occasionally dubious. The problem is to determine how much the existing and apparent irritation, increased energy, debility of the individual, and other phenomena belonging to the category, depend upon the disease; and how much, on the other hand, the disease in the various states of the patient is aggravated by these conditions. Although this fact is placed by the author in the first rank as it regards pathology, yet he states it is very generally omitted in the descriptive nosology of disease, because differing in different patients afflicted with the same dis-

ease, every practitioner regards it according to those pathological principles with which he is most familiar.

Second fact. Combination of symptoms peculiar to disease. All the changes which a complaint produces in the human body, whether it respects the condition of the body, the exercise of its functions, or the infliction of uneasiness and pain, are called symptoms. They are the visible elements whose union produces the external phenomena of disease; and the combination of symptoms being patent to the scrutiny of the senses, may always be examined and described. They are not all, however, of equal value in the production of disease, and should, accordingly, be distinguished as principal symptoms, and accessory ones. The former are peculiar, and so essential that they exhibit the true external characters of the disease itself. The latter do not arise directly from the disease, or necessarily appertain to it; but in whole, or in part, depend upon the peculiar concurrence of the causes, on some symptoms, as the secondary cause, or they originate accidentally. It may here be remarked that this distinction is sometimes most apparent—as conspicuous as are the symptoms themselves; whilst at other times it is made out by means of analogical conjecture. Hence the true character of the different symptoms must always be made objects of the most careful consideration.

Modern medicine has made rapid strides in discriminating the symptoms of disease, and by methods far more satisfactory than were employed by the Greeks; so that the advance of our knowledge regarding the seat of a complaint, its pathological anatomy, and the concurrence of the causes of each disease, have greatly contributed to perfect our knowledge of the exact and correct connexion of causes. At the same time it must be confessed that the moderns have not hitherto equalled the Grecian writers in the simplicity of their expositions and descriptions of disease. This may, probably, have arisen from our erroneously supposing that such pains were unnecessary; or from our being more under the influence of hypothesis than were the Greeks. So soon as medicine attains its proper position as one of the exact sciences, it will outstrip the ancients as much in this lucid and accurate description of disease as it has in the accumulation of facts.

Third fact. Course of the disease. The course of a disease, whether transitory or abiding, acute or chronic, continuous or periodic, stationary or declining, regular or irregular, conspicuous or latent, having remissions, alternations, paroxysms, relapses, or other variations, is sometimes so marked that it cannot be overlooked; at other times it will be readily noticed by every attentive observer; whilst in other instances it will be detected only after the most patient scrutiny. But, however this may be, there is scarcely ever room for anything like mere conjecture respecting it, and it is the occurrence of irregularities alone which demands discrimination. Upon the appearance of any such irregularity, therefore, we must ascertain its relation to the other existing circumstances, and especially to the cause of its appearance. The Greeks were particularly careful in indicating the courses and histories of diseases, and especially of acute ones; and it appears they have left us nothing to do but to follow, imitate, and equal them.

Fourth fact. Seat of the disease. The seat of a disease is understood

to be that part, or those parts, of the body which are chiefly or solely affected by it. The external seats of disease are almost always evident, and the advance of general anatomy, physiology, and pathological anatomy is such that the seat of internal complaints may generally be readily ascertained. Hence the number of cases is inconsiderable in which the proper seat of the disease remains a matter of probability only, and still more of doubt. It is, however, true that the principal seat of a disorder is not always so apparent in internal diseases as in external ones, and especially when various and numerous parts or tissues are implicated. In cases of this kind, accordingly, the assigning the true centre and focus of the disease will always be more or less difficult and conjectural. . . . . . Modern science is far in advance of that of the ancients, as it respects the exact position of diseases; and the contrast on this point would have been still more marked, had not some modern authorities treated these physical facts so hypothetically.

Fifth fact. Anatomico-pathological form of disease. The visible alteration of the natural and vital properties of a part, or parts, of a patient's frame constitutes the anatomico-pathological form of a disease. Inflammation, obstruction, suppuration, may be adduced as examples. These states exhibit changes in the affected parts which are referrible to this category, and they are evident to the senses. Our acquaintance with this important subject is almost wholly due to the moderns, as the ancients were in ignorance of its very elements. It will be his painful duty, however, the author states, to explain in the next book that this fact, as marked as the preceding ones, concerning the *course* and the *seat* of the disease, has been completely obscured by the gratuitous hypothesis of some modern authors, who have endeavoured to discover one primary form of disease upon which they consider that all other morbid forms depend. If he succeeds, he continues, in demonstrating this error, and in introducing the adoption of a more judicious course, rapid strides will speedily follow in the improvement of the healing art.

The anatomico-pathological form of most external diseases is sufficiently clear, being cognizant to the senses. Some internal diseases, also, have so marked an anatomico-pathological character, that in these cases, likewise, it may be held to be decided and satisfactory. In the majority, however, of these internal disorders, it must be confessed that the character is only conjectural; and in a few instances it is still but doubtful and obscure.

Sixth fact. Concurrence or combination of causes. The agency of causes in producing a disease, as has already been shown, is a supposititious fact; but the combination of causes producing its effect in the production of a disease is a positive fact, and important when viewed in relation to the others with which it is here associated.

"This important fact," says the author, "has long lain latent—from the era of Grecian celebrity down to the present day; and this unfortunate obscuration has presented the chief obstacle to the advancement, not to say to the origin, of positive medicine. We were early alive to the importance of this arrestment; and by the most sedulous study for a number of years have been labouring for its removal. By a more judicious arrangement of the established classes of causes, and especially by the introduction of a new method not heretofore proposed—

namely by the prominency given to what we have termed the *primary* or *radical diseases*—we flatter ourselves that we are now in a position that we can offer this fact in connexion with those upon which we have already insisted, as a valuable addition to the science. By this distinction primary diseases are separated from secondary ones; and the true origin of all complaints may be discovered by careful investigation at the bedside of the patient, and with little fear of disappointment or error. That the combination of internal causes produces diseases is an isolated fact which is not always conspicuous, because it does not fall directly under the cognizance of the senses. But we will show that we have so far succeeded in elucidating the problem, that it may always be profitably appealed to. The particular part which each and every one of the concurrent causes plays in the production of disease will always remain conjectural; whilst, at the same time, we shall be able to distinguish betwixt those which are the true efficient causes and those which are the secondary or accessory ones. Whatever honour may accrue to us from having opened up this path, we anticipate that posterity will so improve upon it that, ere long, they will convert our probabilities into moral certainties." (p. 24.)

Seventh fact. The agreement and tolerance of the remedial means employed. All ingesta, whether the usual non-naturals or drugs, when swallowed by the patient, produce immediate effects, good or bad, upon the physiological condition of the patient, and more or less calculated to benefit or aggravate his pathological state. This fact, without doubt, is most important, as initiatory to all others bearing upon the cure. Accordingly, the circumstance has been conspicuously put forward by the Greeks from the time of Hippocrates, although it has not been sufficiently attended to either by them or by the more recent cultivators of the art. A neglect of this important fact has, perhaps more than any other cause, proved the rock upon which systems and improvements have made shipwreck. This criticism applies as much to the mere hypothetical speculator in physic as to the most practical of his brethren. Medicine, upon attaining the rank of a positive science, must endeavour to elucidate this circumstance above all others, as correct views concerning it will most contribute to its progress. In some instances the necessary conditions will appear conspicuous, in others doubtful, and in many obscure; and especially when, the different substances prescribed harmonizing in their effects, the favorable symptoms give place to those of an opposite kind. Willingly, says M. Lanza, will we contribute our personal knowledge, and anxiously do what we can to promote this desirable end; and it is the final goal beyond which we cannot pass.

Such, very much in his own words, is an account of Professor Lanza's seven positive facts. Next follow his three transcendental ones, which he also calls incomprehensible (*incomprensibili*), meaning thereby, we presume, not so much that they are positively incomprehensible, as that they have not yet been fully comprehended; and which may, moreover, for ever transcend the reach of man's limited powers. He thus introduces them to notice, somewhat, it must be confessed, in the *incomprehensible* style of their own nature:

"Disease, like every other entity in nature, has an essence which must consist in that change which the unknown entity displays in the course of the disease, and which is connected with the essence of vitality. The operation of the morbific cause must affect the condition of that essence of vitality necessary to constitute health, producing that special change which belongs to the particular disease

it excites. Remedies, in order to cure, must operate effectually upon the essence of life, by changing the condition it has acquired in disease into that which it maintains during health. Every one must see that these three conditions are the three chief facts or circumstances, because they comprehend the intrinsic and sufficient cause of all the other facts. Hence the other seven, forming parts of the disease, must play their parts in a manner that is in conformity with the disease itself, and with the operation of its causes and remedies." (p. 26.)

But are these facts, demands the author, comprehensible? or rather, do they not transcend the powers of the human intellect? We invite not our readers to follow him in his answer to these inquiries, but proceed at once to the remaining chapter (X) of this book, on which, as before hinted, we purpose to enlarge.

Foundation of positive medicine as a science, and an art. 1. *As a Science.* In that system of medicine which is to be regarded as positive science, it is necessary to bring into one category all the facts that have been accumulated, such as they appear in nature; from the known and established, it must proceed to that which it does not know; and it must clearly indicate that of which it is still ignorant. It thus recognizes conjecture as a legitimate instrument and means of improvement and advance, and indicates to those who follow in its wake the path they should pursue. Hence the only object of positive medicine is to determine what is *common* and what is *peculiar* to diseases and to remedies. The science must investigate that which is common in the greatest number of diseases, and in the greatest number of cases in the same disease; and then determine what *that* is in which this community and similarity consist. It must likewise investigate what is peculiar in different diseases, and in what that peculiarity consists. This ought to be the great object of every scientific physician, and he should and will be satisfied with this grand aim in the prosecution of that science to whose cultivation he is devoted. The same remark applies to remedies. The physician should ascertain what *common* powers or virtues reside in the greatest number of remedies, and in what that virtue consists; also what is the *peculiar* virtue of each remedy, and in what that peculiarity consists; and in this matter he ought to trouble himself with no other inquiry. The science thus viewed is to be mastered by an examination, sometimes conjectural, indeed, but sound, of the seven positive phenomena or facts, as they are termed by M. Lanza, which each disease presents, as determining, not only what is *common* and *peculiar* in the disease itself, but also as ascertaining the power which experience shows every remedy exerts in the same condition of each disease. Thus, positive medicine, by cleaving to the principle of what is common and what peculiar, can alone render the curative art rational and satisfactory. The following rules are laid down for the guidance of all who would labour in this field, although, perhaps, they cannot in every case be observed to the very letter. They are altogether opposed to the mere speculator, whose one canon is, that his data and premises shall not run counter to his system:

"1st. In order to ascertain and adopt that which is *common* between two diseases, or between two cases of the same disease, and also between the powers of the remedies employed, a similarity between the observed facts must be demonstrated.

"2d. On the other hand, in order to demonstrate a peculiarity, or, what is the same thing, a difference between two diseases, or in different cases of the same disease, the direct proof arising from the examination of the different facts alone is not sufficient; it is, moreover, necessary that there be that which is common in these several items; and this demonstration must be decided and apparent.

"3d. That a perfect community and absolute peculiarity being very rare, but, in most cases, diseases and remedies offering much of what is common, with little of what is peculiar; or contrariwise, much of what is peculiar, with little of what is common, we should not in these cases unhesitatingly decide upon what is common except it were very evident, nor determine what is peculiar, unless it be equally clear." (p. 52.)

2d. *As an Art. Communia communibus, et singularia singularibus curantur.* That which is *common* in diseases being determined, not after the method of empirics, but by the sameness of the physiological state of the patient, as well as of the whole combination of symptoms; also, by the course, the seat, the anatomico-pathological form, the concurrence and combination of causes, as well as by the agreement of the remedies employed, and the patient's tolerance of them, the art must necessarily be governed by the analogy of the known common virtues of remedies. Let the proud rationalist boast as he may, he is himself obliged in practice to disregard his hypotheses; and, in analogous diseases, and in analogous cases of the same disease, he prescribes those remedies which have been found to have been useful by experience. It is clear that such analogies as are based upon the whole of the seven positive facts must prove most beneficial, whilst those of the mere empiric will for ever be despised. Induction is of the same use in the elements which are *peculiar,* as analogy is in those which are *common.* But here the question occurs, upon what rational principles can inductions be based, so as to lead to the true indication of the cure of the peculiar element in different diseases?

The errors of transcendental medicine being admitted, positive medicine must be appealed to as the only one whose principles come within the range of human intelligence. In a case belonging to the category supposed, the feelings and instincts of the patient, some fortunate occurrence, sometimes a marked trait of the disease itself, and occasionally a safe experiment or trial, supplies information regarding those curative means which the peculiarity of the disease requires. And when, as often happens, no such hint can be obtained, we must then have recourse to induction from that which is common in the given instance with other diseases, and draw our indications from analogical conjecture. Science yields no more credence to an analogy of this kind, than it draws from the groundwork of the induction; and art exerts itself with an energy proportionate to the amount of the probability. The whole history of our art exhibits the reasonableness of this principle; for it was by these means, and not by the help of mere hypotheses, that, from the very commencement, art has ever sustained itself and made progress. And throughout its onward career, as novelties and difficulties have arisen, and obstinate and unheard of cases occurred, still new methods of treatment and of cure have been discovered. For the most acute diseases hypotheses never did more; and subsequently it has fallen back again to its wonted speculation. "*Non post rationem medicinam esse inventam; sed post inventam medicinam, rationem esse quæsitam.*" From this it follows that induction in medicine

as a positive science has a double object to pursue—1st, as it regards the science, it is to discover among conjectural facts the obscure elements, common and peculiar; 2d, as it regards the art, it has to discover the cure of peculiar cases, leaving the discovery of that which is common to analogy alone.

Thus, according to our plan, have we given, and without comment, a very partial and condensed epitome of Professor Lanza's first book upon the Philosophy of Medicine, insisting more particularly on the parts treating on the agreement among medical men as to the manner of cultivating their common science,—on the scientific discrimination of the facts of what our author calls nosology,—and on the foundation of positive medicine, as a science and an art. Fully are we aware that these are insufficient data, whereby any one can judge of the true value of the volumes, and it may probably exceed our powers to furnish them within our assigned limits. Nor are we disposed to follow any short or sleight-of-hand method, and say, *ex lapide Roma!* Well, however, may we here rest for a moment, and inquire what are we to think of this philosophy, and these facts? We suggest the inquiry, not so much in a spirit of captiousness as of caution. When a man for the first time turns to the elements of some even of the severer sciences, and commences with its lemmata and axioms, he may for a moment fancy that all that is in hand is sufficiently jejune and profitless, whilst in truth it is all-important. And so in the case before us. The Professor, in this portion of his work, is dealing with generalities and abstractions. His philosophy, like that prevalent among his countrymen, though much admired, is of a lofty and subtle character. It is of a species that is rare in this country, and of a class, notwithstanding all his protestations, which we cannot help arranging more with that of the speculative Idealist, of Sir James Macintosh, than with that of the Experimentalist. Our author belongs to a school very different from our own, and uses a phraseology with which we are not familiar: and who knows not that language influences thought, not less than thought language? Hence we would here tender a word of advice, to the effect, that the assiduity and enthusiasm of the Italian Professor in this early stage of its manifestation be not depreciated, and that the varied results of his painful and protracted labours be neither disparaged nor despised.

SECOND BOOK. *Clinical Pathology.* The author affixes this title to the second book, for reasons which may be best expressed by himself:

"I dare not say that I can here supply such a pathology as is peculiar to positive medicine,—such an one as it should have, and as it will ere long possess. Mere hypothetical medicine may easily invent a pathology, based upon a corresponding nosology, and emanating therefrom, because it is formed upon the theoretical principles of a system, and the treatment recommended exhibits nothing else than an artificial union of particular facts adapted to hypothetical pathological principles. The object of positive pathology, however, is very different. It has to reduce the analysis of general and compound facts, to the particulars of which they are composed, so as to determine what is *common* and *peculiar* in diseases,—in other words, the *community* and *peculiarity* of each disorder. This, again, can only be founded upon a positive nosology, because the general system can never be remembered if all the elementary facts have not been thoroughly examined. So soon as medical men concur in cultivating the science after a positive method, and our knowledge of each disease is admitted

by common consent to be positive, and, as it were, perfect, then will positive pathology assume the rank it should ever maintain in our art. At present, however, and till the work here referred to be accomplished, pathology can legitimately be directed only to two objects—1st, to the exposition of what is general, in order to exhibit how very much is still unknown, and how little has yet been learned; and 2d, to indicate, by the way of clinical observation, how we are to attain, step by step, true pathological science. For such reasons as these, limiting the object of the present book, I have given it the simple title of Clinical Pathology." (pp. 57-8.)

This book contains thirteen chapters, which are mainly commentaries upon the seven positive facts already enumerated; the author still confining himself to generalities.

CHAPTER I is on *the physiological state of the patient;* by which expression is meant the vital condition in which an individual is placed when suffering under disease. We cannot, the author remarks, better estimate such a condition than by considering it in relation to the existing state of the patient, and to the energy exhibited; the whole of the functions being in this way brought under review, and life under this peculiar phasis examined. With regard to the energy—the vigour of life—we shall regard it in no other aspect than as a state of exaltation on the one hand, or of depression on the other; which conditions we avoid designating by the terms hypersthenia and asthenia, because these terms express an hypothetical cause of the condition, and the hypothesis clings most pertinaciously to the very words. In regard to this existing state, truly impossible is it that in disease the functions should maintain their natural and regular conditions; except in so far as occasionally a disorder may be of use, as well as order itself—if we be right in regarding disease as a state of disorder. In Italian nomenclature, Professor Lanza informs us that irritation is understood to be a general tremulous commotion, an agitation, in the exercise of the functions of the body. No one, perhaps, has more felicitously expressed the idea than Francesco Redi, when he said that irritation was a sedition of the vital spirits. Rejecting the hypothesis, and adopting the phrase, irritation, in the Italian acceptation of the word, may be defined to be life in a state of sedition. And thus understood, it is ever present in disease; showing itself in different degrees according to the constitution of the patient, the form of disease, and external circumstances. In mild cases, the constitution being good, the disease mild, and extrinsic excitants absent, the irritation may be in the lowest degree, and scarcely perceptible. On the other hand, whenever the disease attacks the whole frame, or even an important organ, and affects the exercise of an important function, sense, movement, or formative process, the patient never maintains his natural vigour. A change is always perceptible, and to the worse, in proportion as the disease distresses; and weakness is the invariable result. It may, indeed, happen that at the commencement of the disease the patient may appear in a state of exalted vigour; but this soon subsides, weakness supervenes, and increases in the ratio of the length and severity of the disorder. The degrees of exaltation and depression are more or less according as the constitution of the patient is more robust and hardy, or weak and feeble; also, according as the complaint entails privation or loss; also, according to the circumstances and causes in which the disorder commenced—as, for example, with copious discharges; and, most of all,

according to the amount of the irritation and of pain, the worst of irritations, when intense and protracted.

Such knowledge as this is the more to be relied upon on account of its familiarity. It has been noticed from the earliest times, and is generally admitted both by professional men and others. Pathology, in its present state, can avail itself of this information only as it presents itself, and usually as somewhat generic; as is freely conceded by all. What light, then, it may be inquired, can positive pathology shed upon this information? None other, it would appear, than so far as it bears upon the physiological condition of the patient; also on the precursors of the complaint—on its course, seat, anatomico-pathological form, and its causes; likewise on the agreement and tolerance of the remedies employed; so that we may thus have general and positive rules regarding the reciprocal influence of the state of the disease, and the state of the patient. All this knowledge should be derivable from nosology, and this, conjoined to the positive method advocated above, will furnish a true pathology.

The foregoing description of increased energy, of weakness, and of physiological irritation, so frequently offers itself to observation, that it may be noticed by every one at the bedside of the patient. From the fulness of the vessels, the colour of the skin, the heat of the body, the state of the pulse, and the general debility, the inference is direct as to the excitement and depression, and every change which may have occurred; and from the symptoms—from delirium, to the most trifling alteration of function, the irritation is made manifest. To this is naturally conjoined that habit which every practitioner acquires of judging from the appearance of his patient, what previously was his state of health, and what it again would be were his trouble removed. From this the inference as to how much he is excited, and how much depressed, is at once easy and satisfactory.

Ratio medendi. Indicatio prima sit vitalis. In regard to the physiological powers of those agencies which operate upon the frame, they are naturally divided into revivifying (*vivificanti*), dissolving, and irritating. By the introduction of these terms the author avoids the use of the terms stimuli and counter-stimuli, which are inseparably connected with hypothetical notions, which he is solicitous should be wholly excluded from the pure matter of fact with which he wishes to deal. He defines *revivifiers* those substances which nourish the body, or which concur in doing so, increasing the means whereby it is so supported. Thus alcohol and aromatics alone would not nourish, but the revivifying powers of other substances are greatly promoted by them.

Dissolvents (*Scioglienti*) are those substances which being incapable of nourishing the body, allow its vitality to be exhausted, and its substance to be dissolved; and detract from the power of those substances which would nourish the body, as often as they are conjoined with them.

Irritants (*Irritativi*) may be distinguished into *necessary* and accidental. The necessary irritants necessarily and invariably act as such upon the whole race, upon the sound and the infirm. Frequently, however, the same substance proves not only irritant, but poisonous to one class, while it may be indifferent, nourishing, or medicinal to another. The *accidental* irritants are those substances which act on the frame, reviving or

disordering, and withal disquieting and disturbing its functions, and apparently for the four following reasons: 1st. From the constitution of the patient, in reference to sex, age, temperament, habit, and especially idiosyncrasy. Thus wine is an irritant to some abstemious persons; ipecacuan, antimony, &c., to many. 2d. From the form of the disease. Thus is it with water in hydrophobia, and so with certain remedies in various diseases, or in certain states of the same disease. 3d. From the quality of the substance. And here this rule is constant, that the more powerful and energetic the article, the more likely is it to become irritative, especially when employed under unfavorable circumstances. 4th. From the time, method, or place in which the various substances happen to be administered or applied. Thus is it even with common water, if it happen, for example, to enter the larynx, or be drunk at improper times, and in unusual quantities. The application of such observations to nosology is evident, inasmuch as they are express facts, generally acknowledged and received. All that at present must necessarily be attended to is to reject them from those hypothetical uses to which the rational dynamists have applied them, and to take the high ground of adopting into our service that only which is most strikingly common in disease, and in the physiological state of the patient. Thus protesting, it behoves us to wait patiently until the progress of positive nosology has determined the relations of such physiological distinctions with the special curative powers which the articles used as remedies properly possess. Waiting in this attitude of expectancy, it appears that in the present state of clinical pathology, as it regards the regulating the powers of the patient, we can do nothing better than adopt the following maxims, which are now acted upon by all the Neapolitan physicians of standing and experience.

1st. That the physiological state of the patient be maintained in the greatest possible tranquillity, so that the powers of life may to the utmost extent be preserved, and the irritation be removed, subdued, or, at all events, not increased.

2d. That in the special cure of disease such substances as are too dissolving be avoided, so that the patient be weakened as little as possible, and the treatment may be readily tolerated. Less than this is never required in the special cure of disease.

3d. That in the special cure of disease, avoiding all agents which are too reviving, the energies of the frame be not roused more than is required; and especially that this kind of treatment do not aggravate the disease.

4th. It so rarely happens that remedies are prescribed for a disease which prove artificially irritating, whereby the treatment is hurtful, that no general rule has hitherto been provided against it, and directions must be postponed until we come to consider the treatment of individual diseases.

The foregoing is an abridged translation of the preface and first chapter of this second book, and will suffice as a specimen of the author's method of treating this part of his subject; the remainder must be given more shortly, and more in our own words.

CHAPTER II. *Combination of symptoms.* The general heads to which

the symptoms of disease are referrible are two, namely, the altered quality of the body, by the moderns arranged under the head of physical signs; and the alterations in functions, arranged as physiological and pathological signs. The author informs us that it is not the object of the present part of his work to give an exposition of the external symptoms of disease, but only of such symptoms as can be demonstrated to be positive. In the present chapter the morbid symptoms are summarily announced, and in such a manner as that general conclusions may be deduced from them, and be made available to the general principles of the art.

1. *Alterations of quality* consist in physical changes of the whole body, or of some of its parts in relation to harmony, colour, heat, situation, form, density, and contiguity; and this not as a consequence of a former disease, or of original malformation, but as a necessary element of the existing disease. 2. *Alteration of function.* According to the author, sensation, vital movement, and formation are the three actions which are simultaneously maintained in the human frame, and are the manifestations of life. Every individual part, moreover, executes an office—a function which is a particular sensation, movement, or formative process, subservient to the well-being of the whole frame. Hence he retains the old distinction of functions into the animal, vital, and natural, in preference to the modern one, of those belonging to animal and organic life.

There being no disease in which the animal functions are not affected with a disagreeable sensation, with uneasiness, which again must be considered painful, such pain is to be regarded as a symptom of disease. And, as in the combination of the symptoms, as it regards the animal functions, this uneasiness and general oppression is the only common one, it may be considered as a generic sign of disease; and the severer it is the more unfavorable. If, in addition to the general uneasiness, there be also particular uneasiness, this becomes the sign of a particular disease. The disturbances of the animal functions may be divided into five classes, namely, into pain, stupor, torpor, convulsions, and aberrations. The different guises in which these symptoms manifest themselves in different diseases is reserved till the consideration of the neuronosi in general, and individual diseases in particular.

Vital movement is manifested in the unceasing current of the fluids throughout the solids of the body. The pulse and temperature, the general condition of the body, the quantity and quality of the secretions and excretions, show how they may be affected in such motions by disease. All this, however, is more particularly described by Professor Lanza, when treating of the *angionosi*, and of various individual diseases. Also, how the vital movements becoming altered, may be accelerated, retarded, or become motionless. Most diseases exhibit both an oppression of the senses, and an acceleration of the vital movements.

The formative process consists of those operations whereby the living body, incessantly composing and decomposing itself, preserves its integrity. This process continues satisfactorily only in health, and in proportion to the natural vigour of the individual. An excess of vigour produces redundance, which may be a cause of disease. Disease again always wastes the body, reducing redundancy where it exists, and subsequently preying on the frame, and leading to debility and emaciation. These

several phases of the animal and vital functions is elucidated under the consideration of the *sarconosi*, and of various diseases in particular.

From this exposition it is evident that the aspect of disease in connexion with the visible alteration of functions, is thereby divided into affections of the senses, of the vital movements, and of the formative processes. In the *neuronosi*, pain, paralysis, convulsions, dementia, and such like predominate; in the *angionosi*, palpitations, hemorrhages, dropsies, fluxes, &c.; and in the *sarconosi*, cachexia, marasmus, tabes, &c. &c.

Ratio medendi. Bona similibus, mala contrariis curantur. In every age medicine has been searching for particular remedies for every morbid symptom; so that for a long time there has been no bad symptom without its corresponding title in the materia medica. Thus, there are narcotics, sedatives, carminatives, cordials, rubefacients, refrigerants, hemostatics, astringents, aperients, sialogogues, cholagogues, emmenagogues, expectorants, diaphoretics, diuretics, and many more. Certainly, in many instances, it is most important to relieve the urgent symptoms, because otherwise great damage may result ere the disease is removed. Hence, too, it will be seen in the nosology of such diseases, that previous to, and in conjunction with the proper cure, relief should be afforded to the dominant system. This, however, is not always easy nor possible; and especially if it be so associated with the disease, that what benefits the one is prejudicial to the other. This unfortunate coincidence must have occurred in the experience of every practitioner; and the author offers the following sage suggestions concerning it. Considering the symptom not only as in combination with the other symptoms, but also as in relation to the positive facts—to the physiological state of the patient, to the seat of the disease, its pathological anatomy, the combination of causes, and the effect of remedies, you can judge of its true nature and consequences, and determine whether it is more advisable to oppose or favour its progress. Upon this depends the proper application of the rule affixed as the motto to this chapter, *quod bona similibus*, &c.; a maxim which, though directly opposed by homœopathy, has been guaranteed by the judgment and consent of all practical observers in all ages. The symptoms being thus considered, not absolutely, but in relation to all the facts, each of which requires an indication for itself, the choice of means must be determined by a reference to them. If the immediate relief of the symptom be opposed by the physiological condition of the patient, or any other circumstance of the disease, the calculation of probabilities alone may lead to the conjecture whether any given remedy will be successful in relieving the symptoms, or be prejudicial, from being in opposition to some fact, and chiefly, perhaps, the anatomico-pathological form of the disease. Pathology in such circumstances teaches *ad quod magis urget, occurrendum;* and the urgency is not so much from the complaints of the patient, as from the danger to his life, and its being an obstacle to his cure. In such cases the choice of the remedy must be left to the experience and sagacity of the physician.

CHAPTER III. This is entitled *On the course of disease*, and must not detain us long. According to a law which the learned Professor explains in another of his works, it happens that no morbid symptom continues long in the same degree or form, but always proceeds either increasing, de-

creasing, or alternating, so exhibiting itself under a variety of aspects. It is by the same law that life is transitory; and disease, including its origin and termination, but a passing occurrence in life, Hence there are no truly stationary diseases, a fact which holds true even with chronic and incurable ones. Curative indications vary with the various courses which diseases present, and with the various stages which occur. Moreover, the variations which appear in such changes are not always secondary, or subordinate to the primary or principal indication which the diagnosis would lead us to expect. These changes, therefore, require to be more particularly considered under the treatment of every particular complaint. At present all that Professor Lanza attempts is to determine the primary and natural distinction of diseases, according to their course. And here two conditions are to be observed, 1st, morbid states; and 2d, morbid processes. By *morbid states* are meant those states which are maintained by an existing and present morbific cause, on which their existence immediately depends; so that when the cause ceases to operate they suddenly disappear. By *morbific processes* again, are meant such diseases as are produced indeed by a present or passing cause, but operating in such a manner that they originate a regular series of morbid events which, in regard to their course, depend no longer upon their producing causes, as they terminate only when they have run their natural and necessary course.

The *ratio medendi,* in reference to the course of disease, our author very much makes a comment upon the following axioms and phrases—*Quò natura vergit, eo ducere oportet. Dum natura movet, noli movere tu.* Every disease, he considers in its nature a combination of elements, good and bad—*bona mixta malis.* The ancients, he reminds us, were wont to refer to the various fortunes of the struggle, under the terms of *natura vincta, natura pugnante,* and *natura vincente.* Hence, according to the view which the physician takes of the part which Nature plays in each disease will be the character of his practice. If he regards Nature as always passive, he will feel urged very decidedly to interfere, and have recourse to bold sanative measures; his practice will be heroic, or, as our author has it, *furiosa.* Again, if he considers Nature to be always active in the cure, he will himself be prone to be passive, and observe the expectant plan—invariably a negligent and dilatory one. And lastly, if he look upon disease as a struggle between the complaint and Nature, he will conclude that he has nothing more to do than to restrain the vehemence of the disorder on the one hand, and to rouse Nature to the struggle on the other. In opposition to all this, positive medicine, forgetting what is passed, must commence anew the study of the powers of Nature in the cure of diseases. The facts previously expounded are the true and solid basis of such a study, the true symptoms whereby Nature shows itself vanquished, contending and triumphing, remaining still to be determined. Such an exposition as this in general terms is yet a desideratum; and in particular cases, all that it is possible to say in the present state of our knowledge will be propounded, says our author, when treating of particular diseases.

On Chapter IV, *Respecting the seat of disease,* we must also be brief. Our author views the seat of disease under two aspects.

1. An anatomical and an etiological, or as he prefers calling it, a therapeutic, point of view. The progressive advance of anatomy, physiology,

and pathological anatomy makes it certain, he remarks, that no disease can be equally distributed as health is, throughout the whole body. Hence it may be laid down as a positive law, as it respects its anatomical locality, that there is no disease but what has a particular seat, and displays a particular anatomico-pathological form. At the same time, however, different diseases assume different guises, by invading, abandoning, concealing, and changing their locality. Hence various distinctions which are reserved for future consideration. At present the author only specifies such as subserve the general indications of the cure. 1st. There are diseases which have a central seat in one part only, and sometimes in one spot of this part, whilst others have their seat extended to one or more regions, or to similar parts, as to glands, muscles, serous or other tissues, capillary and other vessels, &c. &c. 2d. Some diseases remain fixed in their seat, whether central or extended, without affecting other parts, whilst others spread abroad their malign influence to neighbouring tissues, to distant parts, or over the whole body. 3d. The particular anatomical seat of a disease may be considered as necessarily elective, or changing. And 4th, there is metastasis.

2. The etiological or therapeutic seat differs widely from the anatomical; because wherever situated it produces a general effect, and is hence a morbific cause. Diseases, therefore, may certainly be distinguished into general and local, in reference to their respective causes. Hence those are called general which, however much they may be concentrated anatomically in one part, or extended into tissues, and remain there, or may shift about, yet art, for their removal, must employ general means; in other words, remedies whose powers extend over the whole body. Local diseases again are such, that whatever may be their anatomical seat, experience shows that every attempt at what may be called a general or constitutional cure is useless, and that they are to be removed only by dislodging them from their favorite haunt. By this arrangement and definition, Professor Lanza considers we get rid of all questions concerning the inscrutable essence of morbific causes, and their inscrutable operations upon life; since by changing the etiological question into a therapeutic one, we confide the decision to experience. Accordingly, then, as experience, from such rational principles, indicates the diseases wherein a general or a local cure may be the more suitable, we are led to expect it in the particular treatment of a disease.

CHAPTER V. *On the anatomico-pathological form of disease.* We find the interest of the work increase with acquaintance, and the author's views appear more important as they are developed. This fifth chapter is perhaps the most important we have yet encountered, and we cannot therefore pass it slightly by. Professor Lanza regards pathology as a branch of nosology, a union which has not been generally recognized; and hence the departments have been severally cultivated as if they had no alliance with each other. He endeavours to supply a general view of pathological anatomy, pure, positive, and absolutely free from hypothesis, whereby it has heretofore been so greatly injured. He divides morbid appearances into nine forms; including alterations, lesions, degenerations, pseudo-organic productions, fitozoid productions, sarconotic entozoi, organic vices, morbid habitudes, and dissolutions; under this last are com-

prehended gangrene, sphacelus, and death. In thus treating the subject he believes he is rendering pathological anatomy a service, by demonstrating the true and positive method in which it ought to be cultivated, and ensures to nosology, whilst treating of each disease, the possession of the fifth positive fact, constituting the pure science of the anatomico-pathological form of every disease. To these we must shortly advert.

1st. *Alteration.* The author gives this name to those morbid forms in which there is a simple change of structure in a part, which does not require any change of anatomical condition to reduce it again to the sound state; so that if lesions, degenerations, &c., of any kind be added, these evidently originate in the alteration, necessarily preceding, accompanying, or following, but not as necessary or integral parts of it. Alterations usually originate of themselves, and are simple and alone.

Inflammation, obstruction, and *congestion* are next in a similar way defined; and it is then noted that the pathological *community* of these morbid forms is evident, whilst the *peculiarity* will be insisted on under the particular diseases. These three forms may have their seat in any texture of the body, and they run into each other. Inflammation may be acute, or rapidly chronic; obstruction may be chronic, or slowly acute. Congestion may appear in either of these forms, and any of the three may modify the others. The same internal morbific cause may, under varying circumstances, produce any of the forms. Lastly, there is nothing more important in practical therapeutic analogy than what is exhibited in such morbid states, respecting the agreement and tolerance of the remedies which may have been employed. Sometimes two or more of these morbid forms appear combined together, a state which has been called *sub-inflammation.* To this term the Professor has no objection; because it cannot be used as if implying that the state specified has any control over the other conditions, or more, at all events, than they have over it. It is nosology which must ascertain the peculiarity of each state. To this section the author adds the general indications of cure, as he does to the following. Here we must refer to the work itself.

Neuronosi. The neuronosi, or diseases of the nerves, are the next variety of affections which are introduced under this head. Though not so well defined as the preceding by visible changes in the anatomical condition of the parts, yet the physiological symptoms, or the alternation of the function, as pain, stupor, convulsion, &c., are most marked. Not that these express the nature of the affection. Hence the neuronosi, as diseases of the nerves generally, compel us to have recourse to anatomico-pathological investigation. This department of our science arranges under two heads the anatomico-pathological forms assumed in the diseases of the nerves. *Common* neuronosi are those which consist of any pathological form, as inflammation, congestion, obstructions of all lesions, degenerations, pseudo-organic productions, sarconotic entozoi, organic vices, and dissolutions attacking the brain, medulla spinalis, as organs, in the same manner as they attack other parts of the body. *Peculiar* neuronosi again, consist of those in which there is a morbid alteration of the intimate structure of the proper substance of the brain, medulla spinalis, and nerves. It is apparent that such neuronosi have no claim at present to hold a place in pathological anatomy, because we are quite ignorant of the true nature of

the alteration. They ought, nevertheless, to have a provisional not a hypothetical place, especially in reference to nosology.

Angionosi. The angionosi include such diseases of the blood-vessels as are not represented by anatomico-pathological characters, but only by the combination of symptoms peculiar to the functions of the parts. Among these are abnormal movements of the heart and arteries, morbid states of the temperature, as heat and cold, hemorrhages, dropsies, and also all excesses, deficiencies, and deviations of the excretion and discharge of the secondary fluids. It is clear that the angionosi do not form a primary morbid form, but one that is always secondary, or, in other words, dependent upon some alteration, lesion, degeneration, or other morbid form; and that the symptoms are referrible chiefly to the functions of the vessels, and manifest themselves not so much from a particular state, as from peculiar circumstances which must be indicated by nosology. The author here postpones to a future time the hemorrhages, and other forms of these affections, because the peculiarities they present will then be studied practically, so that the angionosi like the neuronosi may have in pathological anatomy a general ascertained principle in the commonality and peculiarity which nosology discovers in practice.

The author's general views of *fever* must not be passed over; but whether they refer to continued fevers in their varied forms, or to the fever which accompanies inflammation, does not clearly appear; they probably, however, include both. He observes that there may be inflammation so slight, and so confined within the affected part, that it does not excite marked fever. Boerhaave taught this ages ago, and re-echoed herein the sentiments of Hippocrates. Hence, if there is a primitive form of any disease, be it obstruction, neuronosis, lesion, or other, and if fever be added, it is held, and with the concurrence of physicians of all ages, that inflammation will be found participating in the constitution of the anatomico-pathological forms of the disease, either as preceding, succeeding, or accompanying, and from the character and vehemence of the fever, the share which the inflammation contributes to the disorder may be determined. Modern pathological anatomy, with great advantage to practice, has elucidated a proposition which is the reverse of the foregoing, namely that not only does inflammation cause fever, but that there is no fever which is not dependent upon or represented by inflammation. The ancients did not state this, because they were ignorant of the extended seats of diseases, and of the spreading of inflammation into tissues, as exhibited both in the living and the dead. Fever having an extensive range, the ancients held it to be a primary morbid form, independent upon any known inflammation; and hence they called it essential or idiopathic. The speculations of the Rationalists on the point have hitherto been most vague and unsatisfactory, and it has been an observation of our own day only, that fever is always a symptom, and of all symptoms the most certain, of the vehemence and character of inflammation, whether its seat be confined to one part, or extended into a tissue.

2d. *Lesions*. Wounds, contusions. Here we must be short, though the author is most minute and ample in his details, in some points impugning some of the most popular doctrines of the day, we allude to some of the most important doctrines of inflammation, and likewise anti-

cipating opinions which have been put forth with high claims, and we humbly conceive, not more high than just, to novelty.

These lesions are divided into—1st, *Concussion;* 2d, *Ecchymosis;* 3d, *Ecchymoma*, soft tumours from circumscribed ecchymosis; 4th, *Contused Wounds;* 5th, *Compound Wounds.*

These morbid forms may present the combination of their symptoms under three pathological phases. As *lesions*, they induce symptoms of wounded anatomical integrity of the part, and corresponding to the physiological exercise of its functions. As *irritants*, they produce a morbid state represented by symptoms of disturbed vitality in the injured part. As *morbid processes*, they invariably produce inflammation when the divided parts are not immediately healed, and the effused and extravasated fluids speedily absorbed.

Ratio medendi. Healing or incarnation is the natural reunion of parts which have been divided. The ancients believed incarnation to be a purely physiological fact, entirely a work of Nature, and one of the manifestations of the natural formative power of every living body. John Hunter, however, conceived that for the effecting of this work, Nature must participate in the diseased process; and, more particularly, that a slight inflammation must be lit up in the wound, an inflammation to which he gave the name of adhesive, and which he conceived to be essential for every reparatory process. On the wide-spread prevalence, we might say the universality of this opinion we need not enlarge. But surely, says our author, this Hunterian notion of adhesive inflammation is nothing better than a capricious fancy, and one which is opposed to the first elements of our physiological knowledge of the formative process in living bodies. As early as the year 1825, Professor Lanza opposed this hypothesis, and demonstrated then, as he is prepared to do again, that not only the natural carnification, but also that morbid adhesions, as they are called, are not to be ascribed to disease under any modification, but are entirely Nature's work. So too it happens, that when during the healing of a wound there is slight inflammation or phlogosis, this adjunct does not prove an obstacle to the cure, any more than an assistant to the natural powers, in the healing process. Here we cannot forbear inquiring, if this be aught else than the doctrine of Macartney, and an anticipation of Dr. J. H. Bennett's *normal nutrition?*—his statement that the exudation of blood plasma is the essential phenomenon of inflammation; and that this process is a modification of the function of nutrition as explained by the doctrine of cytogenesis? We have not hitherto been able to ascertain the fact, whether the opinion of the Neapolitan physician is based upon the same careful inquiry into the results of modern researches, made by means of organic chemistry and the microscope, as has been that of our acute and intelligent countryman. If so, all the more creditable. But be this as it may, the coincidence appears remarkable, and cannot fail to be satisfactory to the distinguished teachers whose views on this important matter are so far coincident.

The ratio medendi succeeds; and long disquisitions concerning suppuration, the varieties of pus, sanies, &c., and other matters more particularly belonging to medical surgery, which we must pass by.

3d. *Degenerations.* This section must be summarily passed over. It

comprehends the subjects of *hypertrophy*, and *atrophy*, relating to structure; and of *induration* and *softening*, relating to the consistence of the part.—So, too, with

4th. *Pseudo-organic productions*, which the author arranges according to their appearance, under six divisions, namely, *morbid adhesions*, *false membranes*, *transformations*, *excrescences*, *anomalous tumours*, and *anomalous substances*, under which last title are considered *melanosis*, *colloid* matter, *cyrrhosis*, *silerosis*, and a fifth, to which he has given the name of leuconosis.

5th. *Fitozoid productions*. Concerning this important group of disorders, our author states that he considers himself even now entitled to give a positive definition, inasmuch as he is convinced that it has an accuracy which will speedily be universally conceded to it. He defines fitozoid productions, those bodies which have an inherent life and peculiar structure, which take origin in the living frame, infect it, increase at its expense, and there remain implanted as fitozoi, or parasitic animal plants.

He divides them into two genera: 1st, those which are essentially malignant, having a special tendency to degenerate, to consume the part in which they are located, and exhaust the constitution. Such are *scirrhus*, *fungus hæmatodes*, *encephaloid*, and *tubercles*. The second genus consists of such as are not essentially malignant, not having the same spontaneous tendency to fatal degeneration. At the same time they may acquire this tendency from disease, bad treatment, or other causes; such are *polypi*, *lypomi*, *sarcoma*, and *cystic* growths. The peculiar attributes of these productions the author states will be more fully expounded in the book on sarconosi, and under those diseases to which they have the nearest nosological arrangement. In this place he gives only their general anatomico-pathological characters,—for which, we need not add, we have no room.

6th. *Sarconotic entozoa*. Among the vermes, or rather those tiny creatures which are engendered and lodged within the human frame, there are some which merit consideration less from their natural-history characters, than from their vitiating the parts in which they may be lodged. In this way they produce diseases which are generally arranged with sarconosi; and hence M. Lanza designates them *entozoi sarconotici*.

Of this number are *hydatids*, which, as now demonstrated, are endowed with vital properties, and live at the expense of the structure in which they reside. Anatomically they appear like clear vesicles filled with fluid, or with lymph of various composition, as will be subsequently noticed particularly. Meantime be it remarked that they possess all the characters of fitozoid productions, raised, however, to a higher state of existence, so that they have a more decided inherent life, and are less dependent upon the parts among which they lodge; so that they are more in mere contact with them, that implanted in the locality where they originated. Hence pathological anatomy rigorously maintaining its pretensions to a positive science classes hydatids as entozoi. For nosological reasons, however, our author deems it improper to separate them from the helmintosi; he, therefore, confers upon them the epithet of

sarconosi, places them among the sarconotic diseases, and there assigns them their place among the anatomico-pathological forms.

Among swine there are examples of the death of such entozoi, of their dissolution, and the absorption of their cysts, and the final cure of the disease. No such occurrence has yet been witnessed or accomplished in man. Hence it remains for the art, by the help of analogy, to obtain their eradicative cure; and, in the meanwhile, endeavour to alleviate the symptoms, and oppose their injurious effects.

7th. *Morbid habits of body.* By a morbid habit of the body is understood that combination of symptoms which produces a general effect upon the whole frame, and is characterised by the general diseased appearance of the whole man. The following states belonging to this category are brought under review: *turgescence, languor, cachexia, tabes, marasmus, caccochimia,*—a good habit of body to appearance, with signs of the operation of particular or general morbific causes.

These are never primary but always secondary affections. But concerning them we must be silent.

8th. *Organic vices* succeed, which may be either acquired or original; and may be divided with a reference to their effects as being—1st, merely *deformative,* interfering with the natural appearance without producing any other annoyance; 2d, as *morbiferent,* acting as causes producing or aggravating disease; and 3d, as *mortiferous,* by impeding the exercise of some important function, and so producing death. As to the nature of these affections it may be considered as they influence the size, harmony, continuity, form, colour, appearance, and structure of the part, which is done accordingly, at length in the volume.

Chapter VI. The sixth chapter is entitled *the concurrence of causes in producing diseases;* and here we have room for little more than cataloguing the causes enumerated by the author, displaying in striking terms how minutely and comprehensively he has gone to work.

I. *Producing causes,* according to the part they play in inducing disease, may be classed as follows: *disposing causes, occasional, efficient, accessory,* and *concurrent;* in all five.

II. *Natural causes* may be arranged under seven heads: 1st, the *natural anatomical structure of the parts,* as of the brain in apoplexy, and the lungs in phthisis; 2d, *peculiar physiological idiosyncrasy,* in reference to the peculiar mode in which a morbific agent or a remedy may act in certain persons; 3d, *age;* 4th, *sex;* 5th, *temperament;* 6th, *hereditary predisposition;* and 7th, *habitude,* by which nature is much influenced—a second nature. The first six of these can be only disposing causes; the last, besides being disposing, may also be preparatory, and, in other cases, occasional; because it may render a man liable to disease to which he was not naturally disposed, or may slowly prepare him for an attack to which he was not otherwise inclined. The efficient cause, it should be remarked, is often found among those regarded as constitutional or habitual, because it receives its character from the habits of the patient; and, in fact, has no other cause than his habitual neglect and disregard of the rules of health. The power of art is naturally limited in these constitutional disorders.

III. *Non-natural causes* are then enumerated; and

IV. *Unnatural causes,* so called, because, by their peculiar influence, they predominate over the natural and non-natural causes. Four of these are enumerated *epidemics, endemics, contagions,* and *poisons.*

V. *Radical causes.* Some diseases possess two constant etiological characters; the former, that they never originate as effects or transmutations from other diseases, each having its own proper cause, natural, non-natural, unnatural, and often unknown; the latter, that they always act as causes—disposing, occasional, efficient, concurrent, &c. From this it follows that, etiologically, diseases may be divided into two classes; in other words they originate, on the one hand, as radical primary diseases; or, on the other, originate as secondary, resulting from the foregoing, and following them in their origin, condition, and issue. These radical diseases of M. Lanza amount in number to twenty-two, and may be divided into three categories: 1st, *vitiating radical diseases*; 2d, *excreting radical diseases*; and 3d, *virulent radical diseases.* In the first category are included, 1, habitual catarrh; 2, colonosi, bilious disorders; 3, habitual plethora; 4, idioneuronosi, nervous diseases; 5, helmintosi, worms; 6, hemorrhoids; 7, uric lithiasis, gravelly complaints; 8, gout; 9, the sequelæ of periodic disorders. In the second category are arranged the primary excretory diseases: thus, 10, herpes; 11, scaldhead; 12, chilblains; 13, the sequelæ of acute contagious diseases, and 14 suppressed secretion of milk. The third category consists of the virulent radical diseases, and contains, 15, tinea; 16, rickets; 17, scrofula; 18, scurvy; 19, syphilis; 20, scabies; 21, leprosy; 22, sarconotic tendency.

The author conceives that the greatest service he has rendered to positive medicine, is that he has created an etiological science for these twenty-two radical diseases. He maintains, moreover, that unless due attention be paid to them, the curative art can never render the principles of the indications which are directed against the causes certain.

VI. *Injuring causes.* Finally, we have to add that injuring causes are introduced, and are divided into external and internal. The *external* injure mechanically and chemically. The *internal* are impurities, morbid products, &c., which generate within the body, injure the parts where they collect, and vitiate it either mechanically or chemically. Among the mechanical effects are to be placed all those mechanical lesions which were enumerated in the preceding chapter, (the fifth) and considered as causes of the secondary diseases originating from the mechanical disturbance, distension, and pressure they occasion. The chemical, again, consist in the materials retained, collected, stagnant, absorbed from one quarter, and elsewhere deposited. Such are—1st, blood, inclosed in aneurisms, collected in ecchymosis, and effused in cavities, so as to be removed from the influence of vitality; 2d, the secondary fluids when they become stagnant in their canals, and especially if they have been previously altered by serious disease,—as vitiated bile, dysenteric fluid, and, beyond all others, urine when long retained; 3d, matters absolutely morbid, as the fluid of dropsy, pus collected in abscesses, or effused, as in empyema, the sanies of cancer, and all other degenerating sores, &c. &c.; 4th, worms, wherever they make their appearance, indurated fæces, &c. &c.

The curative art, in treating external causes belonging to this section, must adapt particular means to particular cases. In internal causes a primary indication is to oppose by every means the morbid product, and, if possible, to remove the morbific cause; a palliative indication is to alleviate the symptoms, and the secondary effects produced by these offending causes.

Chapter VII, *On the tolerance and conference of the remedies employed,* must, for the sake of brevity, be omitted, and we must draw our analysis to a close by a short account of the succeeding one.

Chapter VIII. *The distribution of diseases, into classes,* or, in other words, the *nosological arrangement* of our author. Two questions are first of all put. First, whether the nosologies of Sauvages, Sagaro, Pinel, Vogel, Cullen, Frank, and many more, afford a natural arrangement? This is answered in the negative, And secondly, whether they are practically useful? and here the finding is, that, though studied by scientific men, they are disregarded by practical ones.

Professor Lanza submits the results of his labours as a small contribution towards the natural distribution of diseases; and first separates the twenty-two radical complaints, which we have previously had occasion to enumerate, from the others which are more or less to be regarded as secondary. In establishing these secondary diseases, having no wish to pervert Nature, or interpret her doings where indications are insufficient, he has always preferred and followed what is useful, and associated those diseases which have so much of what is *common* in them, that the art actually assigns to them an analogous treatment and cure.

First class. *Radical diseases.* In this class is included diseases differing in appearances, course, form, and cause; but agreeing in this, that each has a peculiar and primary cause which may be the root of every other disease, and present an obstacle to every mode of cure; on this account they are named radical, as controlling the curative art.

The second class consists of *acute inflammatory diseases.* These diseases have a common course and anatomico-pathological form, consisting of inflammation concentrated in some part. They are made to precede rather than follow fevers, because they throw much light on the diagnosis of these latter.

Third class. The *acute continued fevers* have also a common course. Their anatomico-pathological form, as revealed by pathological anatomy, consists in inflammation, which may pervade any of the tissues of the frame.

Fourth class. *Periodic endemic fevers.* These exhibit such a singularity in their course, and owe their origin to a cause so peculiar, that however much they may resemble the continued fevers in their anatomico-pathological characters, yet requiring particular curative indications, they have been placed in a distinct class.

Fifth class. *Transitory contagious diseases* have an acute course, and an inflammatory anatomico-pathological form, quite agreeing with that of inflammatory diseases and continued fevers. The whole of them have not their seat in the skin, and hence the name of exanthemata is not bestowed upon them. Other complaints having an analogous cause

are brought under the same head. The cause of them all is a contagion peculiar to each, but *common* in respect of its transitory effect, which commonness throws much light upon their treatment.

SIXTH CLASS. The *sarconosi.* Under this class the subjects both of growth and wastings are included; nor can these, henceforward, ever with propriety be dissociated. How could fitozoid productions be handed over to the surgeon, whilst tubercles, so essential a part of the history of phthisis, be retained in medical nosology. That which is common in the appearance, course, form, and cause of these complaints is so marked that they must occupy a single class.

SEVENTH CLASS. The *angionosi* comprehend those diseases which exhibit, as a marked symptom, functional disorder of the heart and blood-vessels. Hence the class includes the anatomico-pathological affections of the heart and blood-vessels, hemorrhages, dropsies, effusions, and morbid collections. The present state of the science requires that these diseases should be united in the same way as if they actually had been so.

EIGHTH CLASS. That of *neuronosi* comprehends those diseases which exhibit well-marked symptoms of altered function of the nerves. It is of importance to distinguish these with the degree of accuracy that is necessary to guide us in the art of curing them.

NINTH CLASS. *Diseases peculiar to women.* This class comprehends those complaints which, appearing to have their efficient causes in the sex, are natural to them. Among these are hysteria, chlorosis, diseases connected with the catamenia, the puerperal state, suckling, &c. The ordinary complaints which attack women in common with the other sex, and sometimes preferably are, of course, excluded.

TENTH CLASS. *Diseases peculiar to children.* This class includes those diseases only which have their efficient and natural cause in the age of the patient. All complaints, therefore, are to be excluded which attack them in common with adults, and sometimes it may be in preference. The ratio medendi is by this arrangement much elucidated, and great advantage, as in the case of female complaints, accrues from considering that which is common in their respective categories.

This constitutes the whole of Professor Lanza's nosology; and under this arrangement he treats of all the diseases to which man is heir. Very different is it from any previously proposed, and on its real merits it is not easy to decide until the details are reviewed and scrutinized.

The six remaining chapters of this book treat, in a general way, of the *distinction of remedies,* the *diagnosis* of disease, the *prognosis,* the *cure generally,* the *methods by which the cure is to be promoted,* and lastly, the *method in which positive medicine is to be advanced.* With a few sentences upon this last important topic we must bring the survey of the whole chapter to a close.

According to the Professor, the chief obstacles which impede the advance of experimental medicine are the three following: First, the extreme variety which prevails in different countries, provinces, and cities, in the municipal customs, plans of cure, popular remedies, and medical usages, generally, whereby all common grounds of comparison are wanting. Secondly, there is the monster abuse of polypharmacy, on which nothing need be said, the injury it has caused alike to humanity and the art being

notorious. Thirdly, there is the number of compound remedies still in vogue, a clear relic of barbarism, which should long, ere this time, have been banished from amongst us. Leaving all this behind, positive medicine must commence anew to determine the true physiological and pathological power of remedies, and in order to do so must proceed in the following manner:

1st. We must ascertain the real effects of remedies on the healthy frame. Such experiments, however, must not be executed after the fashion of the rationalists, or of M. Hahnemann, by employing the most powerful and extraordinary remedies, and in doses which prove irritating, and almost poisonous. On the contrary, we must employ remedies of medium power, common, ordinary, and perfectly well known; and we must adapt their doses to the display of their true normal effect, whence we shall be able to appreciate their virtue and efficiency on the physiological state of the patient.

2d. Having determined the physiological power of ordinary remedies, we must then use them in the most simple manner, and solely in the cure of those common diseases with whose nature we are best acquainted. It is thus alone that we can become familiar with the physiological and pathological power of a number of common remedies in common diseases.

3d. Step by step, from the knowledge of the ordinary powers of common remedies, we must subsequently proceed to that of the more powerful remedies in rarer diseases; holding it clear that our advance will be more sure and certain, in proportion as such a mode of experimenting becomes more common and familiar.

We have no doubt, adds the enthusiastic Professor, that medicine will have such a progress; and, in order the more rapidly to advance it, he should like to see the arduous labour undertaken by a society of enlightened men collected in one of the great capitals of Europe which, a centre of scientific intercourse, had correspondents throughout the world. The united efforts of such a union might succeed in greatly expediting the removal of the three great obstacles to the advance of the healing art upon which we have previously dwelt, not less than promote its progress by accomplishing the three most desirable objects we have just enumerated.

Thus have we presented our readers with an analysis of the earlier portion of our author's volumes, full as our space admits, and the manifest merits of the work demand. It includes the whole of the author's exposition of his general doctrine, which must have cost him an amount of careful consideration to which, we doubt not, he attaches a high value. But although the most original and novel part of the work, it does not, therefore, follow it will prove the most popular or pleasing; on the contrary, it may be quite the reverse. The dealing with general principles and abstractions, however imperatively necessary and important, suits not every taste. Our object, however, has not been so much merely to please, as within a moderate space to convey distinct ideas of the peculiar character of this portion of his labours, the exposition of which, we trust, will prove both interesting and useful.

The concluding part of the first volume and the whole of the second are occupied, as the remaining ones will be, with the application of the

principles above propounded to the details of disease under its multifarious forms, and the best method of encountering and, if possible, relieving and removing it. The third book, accordingly, is occupied with the twenty-two radical diseases, already catalogued, in so many distinct chapters; the fourth, with common continued fevers; the fifth, with periodic endemic fevers, and the sixth with acute inflammatory diseases, whose consideration, if at all, must be reserved for a future opportunity.

Meanwhile we feel entitled neither to ask the judgment of others, nor to volunteer our own—definite and conclusive—as to the merits of this work. We have been much struck with its originality, with its rich store of facts, and the industry and success with which they have been grouped and generalized. We have also been surprised to find that the whole plan is so ingenious, pregnant with thought, and endowed with good sense and sound discretion. We have throughout, ourselves been anxious to follow an advice preferred by M. Lanza, which we recommend to all: "Let the reader examine with a kindly spirit what we have proposed as the best method of studying disease in a positive way; let every one endeavour to find a better,—we doubt not there are many,—*e viva felice!*"

ART. III.

Elements of Pathological Anatomy; illustrated by Coloured Engravings and Two Hundred and Fifty Woodcuts. By SAMUEL D. GROSS, M.D., Professor of Pathological Anatomy in the Medical Department of the Cincinnati College, &c. Second edition.—*Philadelphia*, 1845. Large 8vo, pp. 822.

WE have, in our first Article, noticed an important work of Dr. Gross, published four years since. The present one is of more recent date.

It seems a remarkable fact that the literature of Great Britain does not possess a complete treatise on morbid anatomy. The 'Elements of Pathology' of Mr. Mayo must rather be regarded as a series of notes, than as actually a work, on the subject; besides, throughout its pages the impress of the special surgical pursuits, tendencies, and style of knowledge of the author may be found prevailing to the detriment of the purely medical divisions of his topics. Dr. Hodgkin's treatise, elaborately worked out as far as it goes, is but a fragment; and such, we fear, it is ever likely to remain. Dr. Carswell's fasciculi, models in their way, leave a multitude of subjects either untouched altogether, or but very lightly handled. Besides, no single one of these works contains any account of the microscopic appearances of diseased parts and products. Their publication dates, indeed, before the era of modern or true micrology. Much they contain has been proved absolutely erroneous from the revelations of this, as yet, short-lived era—much, too, requires remodelling and rearranging.

But the deficiency of a work on morbid anatomy, brought up to the level of existing knowledge, is neither traceable to *individual* apathy towards the subject, or inaptitude for the task of its production. There are not a few persons in London at this moment whom we believe both zealous enough and learned enough to accomplish the labour, both with honour to themselves and advantage to their brethren. But these persons

seem, naturally enough, disposed to invest their talents in capital more likely to supply a remunerating profit. Experience and inquiry show them that works on morbid anatomy are not prone to return even the outlay necessary for their production. And so long as the subject continues uncared-for in what it is the fashion to style "high places," this must continue to be the case. Few things connected with medical education in this country are more striking than the total indifference on the part of examining boards, as to whether those to whom they grant their diplomas possess even rudimentary knowledge in the facts and principles of this science. Candidates for the licence to treat disease in the human species are required to know minutely the anatomy and physiology of plants, and of the textural changes produced by disease in man it is not cared how ignorant they may be. Youths are peremptorily required to be "thoroughly up" in that which, in nine hundred and ninety-nine of a thousand cases, their subsequent lives will afford them no opportunity of employing, namely, a knowledge of the mode of life of vegetables; they may luxuriate in deficiency, more or less complete, of that on which much of their utility as practitioners of medicine must hereafter depend, namely, a sound knowledge of the nature, sequence, and influence of the anatomical changes preceding, accompanying, or following the development of disease. Hence it is that in no department of medical science does ignorance so generally prevail as in morbid anatomy—a fact of which illustration is too often publicly afforded by evidence given in the courts of law, and (more innocently in respect of consequences) by the "reports" of ambitious tyros in the pages of our medical journals.

As Examining Corporations neglect morbid anatomy—as they do not accord it its due weight and importance as an element of practical education—the majority of medical schools, of course, care not how profoundly ignorant their alumni may remain of its mysteries. The prospectuses of some schools make no mention of the subject at all. Conscious of their deficiency in the reality, the conductors of these establishments make no attempt to produce the semblance of giving instruction in its principles. In the announcements put forth by other schools, instruction in morbid anatomy is advertised by the teacher of some one or two other branches (of which branches, be it remembered, he has scarcely time to expound the facts); and the advertised promise is kept by the chance introduction, some twenty times or so during the course, of allusions, sufficiently profound and mysterious, to John Hunter and coagulable lymph. We appeal to these very teachers themselves, if this be not an unexaggerated statement of their mode of proceeding. We remember, now many years since, meeting the late Dr. Macartney at Paris, to have fallen into conversation with him on the relative conditions of home and foreign medical instruction. The Irish Professor could not contain his indignation at what he called the division and subdivision of subjects at the Parisian School of Medicine. "What outrageous absurdity it is," he exclaimed, "their having distinct professorships of descriptive anatomy, comparative anatomy, surgery, and morbid anatomy! Why, sir," said the doctor, "I have taught every one of these subjects in one course every year for the last twenty years, and I always had time enough, and to spare!!"

There are but two schools, we believe, in London where separate teacher-

ships of morbid anatomy exist; and here, unless our informants have sadly deceived us, the students exhibit their deference for the opinions of the great Examining Corporations—they neglect the teachers. It is, indeed, matter of notoriety that no one has ever succeeded in what is technically termed "making a class" in morbid anatomy in London.

But it may be said that the Society of Apothecaries includes "morbid anatomy" in its curriculum. We admit this, and we give that body full credit for their good intentions: this is not the only instance in which the lowest of the three medical estates has set a good example, which the others have scorned to follow. But good intentions are one thing, and good deeds another. It is well known the desire of the Apothecaries' Company concerning morbid anatomy is virtually a dead letter. Actuated by some evil influence or other, the examining body accepts as evidence of instruction in morbid anatomy the attendance on hospital practice; and thus utterly does away with the intent of those who recognize the importance of serious and systematic study of disease and of morbid changes. Under the present state of things, any pupil who has entered to "hospital practice" obtains his "certificate for morbid anatomy" (though he may never have seen more than a body or two opened) from any one of the hospital physicians!

Despairing then, as we do, for the present at least, of the appearance of a work on morbid anatomy of British manufacture, we have felt no little pleasure in receiving from a transatlantic school the bulky volume of Dr. Gross. This treatise has reached a second edition; and the tribute to its merit thus implied is, within certain limits, not undeserved. Deficient altogether in original facts or views, exhibiting no particular quality of ingenious inference, occasionally mistaken in the statement of opinions and conclusions, incorrect in the allotment of discoveries to their authors, and not unoften displaying want of acquaintance with the existing state of advancement of European science, the volume, nevertheless, is free from errors of importance, and contains a plain and succinct account of the chief facts of morbid anatomy. Dr. Gross, in arranging his materials, has followed the plan of Andral, Carswell, and many others—that of first describing the various morbid states, which occur in the body, by themselves; and then, in connexion with each tissue and organ, dwelling upon the peculiarities which distinguish them therein.

Dr. Gross is a disbeliever in dynamic diseases. "All diseases, I feel confident, will ultimately be found to have 'a local origin and habitation;' and if this opinion shall ever be proved to be true, the whole class of febrile maladies, with its hundred varieties and subdivisions, will cease to have a place in our medical treatises." (p. 26.) We fully coincide in the views here expressed; our admission of dynamic diseases at the present day is merely provisional, and becomes from day to day almost less necessary, in consequence of the discovery of organic causes for numerous diseases presumed formerly to be merely functional.

Dr. Gross holds a doctrine concerning the effusion of serum, the heterodoxy of which is not relieved either by its own plausibility, or by any ingenious argumentation on the part of the author. "Serous effusion," says Dr. Gross, "is the result of inflammation, usually of a very mild grade. That this is true, as a general rule, cannot be doubted; the ex-

ceptions, if there be any, are certainly very rare, and have not yet been satisfactorily pointed out." He regards anasarca, for example, of the limbs produced by venous obstruction, as a result of inflammation in the actual part in which the serous effusion occurs. There is, according to him, no such thing as simple mechanical œdema. The sole arguments put in requisition to support his notion are, that (1) the anatomical signs of inflammation may be deficient, or imperceptible, in parts where that process has existed; and (2) when obstruction of a large vein takes place, "the blood is interrupted in its circulation, and congestion of all the vessels, both large and small, is the result. This congestion is not transient, but permanent; and it is scarcely reasonable to presume, judging from our knowledge of the circulation, that this state could exist long without producing an altered condition of the sensibility of the parts affected, attended with more or less redness and effusion of serosity." This is sufficiently hollow reasoning. And, as often happens with propounders of inadmissible notions, the author himself absolutely relinquishes his crotchet (inadvertently, we doubt not) in the very next page; where we find him concluding his inquiry with a statement that serous effusion "is the result invariably of a process *analogous to*, if not strictly identical with, inflammation." (p. 45.) Here the whole position, against which the author contends, is conceded. Everybody knows that there *must, ex naturâ rerum,* be analogy between some of the phenomena occurring in the inflammatory and mechanical processes of serous effusion; but analogy is not identity.

Much of Dr. Gross's volume relating to the subject of inflammation and its effects would be unintelligible, unless upon the understanding that he employs the term organization as synonymous with vascularization. Such employment of the terms would carry us back in pathology more than we should have expected to have been carried back under the conduct of Dr. Gross. The day has passed when the possession of vessels was mistaken for a *sine quâ non* of the organization of an animal substance. Epithelium is surely organized, and the transparent cornea is among the most beautiful specimens of organization the animal body contains; yet vascularity exists in neither the one or the other. Nor does Dr. Gross appear to us happy in his sketch of the mode in which the vessels of solidifying coagulable lymph are produced. The sketch is too sketchy; the real points of anatomical difficulty are scarcely glanced at. But we hold him to be right in his exposure of the pertinacity with which French surgeons refuse to admit the practice of closing amputation and other wounds by first intention. In all probability they will continue to jeopardize (to say the least) the lives of their countrymen by their refusal, until some crafty and most learned Parisian shall have discovered that the practice in question originated in France, and not among "*nos chers confrères d'outremer,*" as we on this side the Straits of Dover are affectionately styled.

Dr. Gross seems particularly fond of detecting and exposing lurking inflammation in conditions where others are agreed that it does not exist. In hemorrhage by transudation, under the influence of mechanical obstruction, he discovers inflammation. At least he doubts whether it does not exist. And upon what evidence? Upon the analogical argument

that during menstruation "many women undergo great suffering, labouring under all the symptoms which indicate the presence of inflammation." That the symptoms of ordinary dysmenorrhea are those of an inflammatory state we altogether deny; while, on the other hand, it should have occurred to Dr. Gross that (as is now well known, thanks to the use of the speculum) actual inflammation of the cervix uteri often exists in women who suffer extremely during the menstrual period.

In speaking of "transformations," Dr. Gross discusses as follows a question of some interest. The passage affords a good specimen of his style and manner:

"Is the osseous transformation, when it takes place in the cellular tissue, always preceded by the fibrous and cartilaginous states? Upon this point pathological anatomists are still at variance. If the process be carefully examined, as it occurs in the subserous cellular tissue in different parts of the body, it will be found to involve a series of successive stages corresponding with those that are observed in the fœtal skeleton. The first change which this substance experiences is a diminution of its natural transparency, accompanied with a slight degree of thickening of the part, and a deposition of turbid cream-like matter, which is diffused through its aerolar texture. As the morbid process advances, the part becomes more and more opaque, is rendered flexible and elastic, assumes a grayish colour, and grates under the scalpel. It is now distinctly fibro-cartilaginous; it is next *converted into cartilage*, and finally into bone, the particles of osseous matter being deposited at different points, which gradually augment in diameter; and at length, running into each other, thus completely change the primitive character of the part. The period required for the perfection of each of these changes cannot be determined. In some instances there is reason to believe that it is very short, whilst in others it embraces several months, or some years." (p. 98.)

Dr. Gross has a chapter on "Polypi." We do not believe that in a work on pathological anatomy such a chapter should exist. Polypi possess no special character of *structure* entitling them to consideration apart in a scientific point of view. The word was originally employed to signify a pecularity of *shape* or *form*, and merely this. Polypi are pedunculated masses, composed of structure which is capable of existing in other forms. To us, therefore, it appears that the structure, be it cellular, cellulo-vascular, erectile, or fibrous, &c., should be described generally, and the accidental quality of polypoid shape described as a matter of secondary consequence. The mode of proceeding (the usual one on the part of writers) adopted by Dr. Gross has been productive of the extremely prevalent, and quite as erroneous, notion that the nature of a so-called polypus is peculiar and essential to the fact of its shape. We are still more at variance with Dr. Gross in respect to the views he entertains concerning the "transformations" of polypi. They become carcinomatous frequently, he says; "and, further, the fibrous polypi is by far the most liable to degenerate into malignant [meaning cancerous] disease." (p. 109.) We are well aware that this doctrine has been professed by other persons; but we hold it to be altogether destitute of foundation, as we know it to be destitute of proof. Has Dr. Gross *seen* true fibrous tumours (polypoid or not), wherein a development of cancerous substance had occurred? If so, we sincerely trust he has preserved the specimens. On this side the Atlantic, at least, they would rank amongst the *mirabilia* of morbid anatomy.

Dr. Gross embraces the theory that hydatids owe their origin to inflammation; and the argument he himself adduces in support thereof is in the following wise. In Cincinnati, it appears, the hog is very frequently infected with acephalocysts:

"Whole droves, consisting of three or four hundred heads, are sometimes thus affected. These animals, most of which are young, are raised in the prairie districts of Ohio, Indiana, and Kentucky, and are literally stuffed with fresh corn for six or eight weeks before they are sent to market. The consequence is, that the portal circle is kept in a state of constant congestion, which finally leads to inflammatory irritation, and the development of acephalocysts in the liver and other viscera." (p. 118.)

Dr. Gross's affection for inflammation, as the prime agent in the production of almost all diseases, leads him occasionally into strange forgetfulness of the principles of logic.

Between the investing cyst of a company of acephalocysts and the animals themselves lies, as is well known, "a soft, pulpy, dirty-looking substance." This intervening substance is supposed by Dr. Gross to be "an *important structure,* designed to assist in the elaboration of a fluid for nourishing the parasite." (p. 119.) It will be admitted that even if this idea be correct (of its correctness no shadow of proof exists), the language in which it is clothed is unfortunate. Dr. Hodgkin, with perhaps greater plausibility, but certainly equal deficiency of proof, regards this same "dirty-looking substance" as an "excrementitious secretion from the hydatid itself." True knowledge is unquestionably not advanced by the promulgation of such gratuitous assumptions.

Dr. Gross's account of tubercle contains two or three opinions and views, which may as well be noticed. For instance, he thus speaks of the gray granulation of the lung:

"To me the gray granulation seems to be merely a variety of the common tubercle, modified by the action of the affected part, the state of the blood, and the condition of the general health. Serum, lymph, and pus are modified in this manner, and why should not tubercle be? To maintain that the gray granulation is merely a nascent tubercle is, to say the least, not very philosophical. That it is liable to be transformed into the common yellow tubercle is certain; but that death may, and often does, take place before any such change is effected is equally true. The gray granulation may precede the yellow tubercle, or it may be depoposited simultaneously with it. Under whatever circumstances it is found, it always contains a disproportionate quantity of animal matter, and is endowed with a much greater power of resisting the influence of such agents as have a tendency to destroy it. It is evidently [?] the product of a more healthy action; it indicates a better state of the solids and fluids; in a word, it is a more plastic organizable substance than common tubercle." (p. 133.)

We freely confess that this train of argument appears to us both obscure and hypothetical. The writer goes on to say that tubercle, being in the first instance liquid, "becomes solid only by the abstraction of its serous particles;" in other words, by a sort of evaporation. Far from this; it is held generally (and we believe with perfect correctness) that the solidification of tubercle depends on the generation of solid particles within its fluid matrix.

Dr. Gross is of opinion that tubercles are always of inflammatory origin. He advances no argument of the slightest novelty in favour of this notion;

which, it is needless to inform the readers of this Journal, has, on the evidence of old arguments, been refuted *usque ad nauseam*. But we cannot help commenting on the example here afforded us of the perversity with which exploded errors are sometimes again and again brought back by persons who, in common phrase, should know better. The fact of inflammation, says the author, is proved by the experiments of Cruveilhier and others, who produced "well-characterised tubercles" by dropping mercury into the trachea. Now ought this author not to have known (it is a more unfavorable view to suppose that he concealed this knowledge) that Andral, Gluge, and many others, have shown that the so-called *tubercles* were nothing but small collections of purulent matter? And that, in point of fact, their production throws about as much light on the generation of tubercle as it does upon the means of squaring the circle. Another of Dr. Gross's arguments in favour of the inflammatory origin of tubercle is, that "scirrhus and encephaloid, in fact, all carcinomatous growths, are of inflammatory origin." It is plain that no disputation can be engaged in with a writer who conceives himself justified in making dogmatical assertions, like these, directly at variance with almost universal opinion at the present day.

Writing of muscles, Dr. Gross illustrates his descriptions by woodcuts, copied, without acknowledgment, from British sources. This is, however, a matter of secondary importance,—in one point of view, at least. But it is not a matter of secondary importance, in any point of view, that the rude and totally incorrect drawings of the ultimate texture of muscle, put forward several years ago by Hodgkin and Lister, should, at the present hour, be circulated as representing the state of existing knowledge. Similarly defective is his exposition of the structure of the walls of arteries, which, with anatomists of the old school, he describes as consisting of only three coats. A writer on their morbid anatomy should surely be well acquainted with their structure in the healthy state.

The well-known atheromatous deposit in the coats of arteries Dr. Gross describes under the title of tuberculous; he thinks the disease tuberculous in fact.

"The term which I have here ventured to substitute is, I think, altogether preferable, as it designates at once the true nature [?] of the lesion to which it is applied. Should it be objected that the deposit is not in reality of this description, and, therefore, this name is equally as unphilosophical as the others, I reply that there is nothing to justify such a conclusion, inasmuch as the physical properties of this substance, its mode of secretion, and its final conversion into purulent fluid, all conspire to show its identity with tubercular formations in other parts of the body, especially the bones, testicle, uterus, and seminal vesicle." (p. 230.)

Now this is pathological anatomy gone mad, in our humble opinion. What does Dr. Gross mean by direct observation of the "mode of secretion" of this substance? or, granting that he does know something of the matter, wherein has he found the *peculiarity* in the mode of secretion of this atheromatous matter and of tubercle—the peculiarity (according to his argument) showing that these two products should be separated from others owing their origin to a process of secretion? Again, "its *conversion* into purulent fluid." Who has ever known that the veriest particle of tubercle that ever existed has been, or could be, *converted* into pus? And the

looseness of reasoning with which Dr. Gross supports his own notions is rendered more remarkable by his rigid unwillingness to admit the ideas of others (though he adduces no sound objection to them), unless based upon irrefragable proof. Thus he is acquainted with the opinion put forward by Gluge that this change in arteries is really of fatty nature; but, according to this morbid anatomist, "the fact that it contains cholesterine and oily matter does not prove that it is not of a tubercular nature." True enough, in so far as fat and tubercle may be, and occasionally are, associated; but suppose the microscope exhibits *much fat* and *no tubercle*, what then? Will not that prove that the deposit is not tubercular? Now the microscope *has* done this.

"To deny that the articular cartilages are vascular," says Dr. Gross, "as has been done by some, is not less absurd than unphilosophical. Does it follow, because the naked eye is incapable of discerning the produce of vessels, that there must needs exist none? Who has ever demonstrated the vascular structure of the healthy cornea, the arachnoid tunic, or the synovial membranes? No one; and yet that these *organs* [?] are highly organized [vascularized] every pathologist must admit from his own observation." (p. 255.)

Now this passage contains numerous incorrectnesses. In the first place, the opinion that the structures in question are devoid of vessels is not founded alone on examination with the naked eye, but with the microscope; and, besides this, supported by the physiology and pathology of the cartilages. Secondly, no physiologist, that we know of, admits the transparent cornea to be vascular. It has its place among the extra-vascular textures. Dr. Gross appears to have no acquaintance with the physiology and pathology of this class of textures; and hence his description of the diseases of cartilage is a failure. He seems imperfectly acquainted, also, with the history of loose concretions, or "cartilages," of joints,—speaking, as he does, of their being invested with synovial membrane, as of a fact concerning which no doubt may be entertained. Now the truth is, that it is very doubtful whether these bodies are produced outside the synovial cavity in any instance, and certain that in many cases such is not the locality in which they originate. Dropsy of the joints (hydrarthrosis) Dr. Gross sets down as the *invariable* result, when it exists, of chronic inflammation. We do not *know* this idea to be incorrect, but we *believe* it to be so; and unquestionably all the ordinary attendants on inflammation are wanting in many cases of the affection.

Dr. Gross's chapter on diseases of bone is, on the whole, good. He justly understands by the word exostosis "simply a bony excrescence, similar in its structure to the osseous tissue in the normal condition. Nothing, it seems to me, can be more unscientific than the classification of Sir Astley Cooper; who has described under this head some of the most malignant diseases to which the bones are subject." In the justness of this criticism of Sir Astley Cooper's classification we most heartily concur.

The account of diseases of the skin opens with a description of the tegumentary textures in the natural state—a description remarkable for its deficiencies. The microscopical structure of the epidermis is not touched on, and various questions regarding its structure *argued*, which have been long since finally *settled* by the microscope. The whole chapter

is bad. Porrigo is described as it might have been described fifty years ago; and not a syllable said of its connexion with the development of vegetable matter of low form. The statement that the vesicles in herpes zoster do not spread beyond the median line is accredited to "Cazenave," apparently under the impression that the statement is of questionable, or, at least, not generally acknowledged, accuracy, and that it originated with the French writer named.

"The arachnoid, like other serous sacs, is liable to effusion of blood." Dr. Gross proceeds to illustrate this statement by an analysis of the papers of some French writers on hemorrhage into the arachnoid cavity in *children*, and appears ignorant of the fact that such hemorrhage is not excessively uncommon in the *adult*. Dr. Carswell's fasciculus, "Hemorrhage," contains every anatomical fact of the least consequence on the subject. That these Frenchmen should know anything of what an English writer may have written on this or any other subject was, of course, not to be expected; but why should Dr. Gross tread in the steps of the ignorant and conceited authors of the Parisian school?

In the first edition of his work, Dr. Gross expressed the opinion that softening of the cerebral tissue is always of inflammatory origin, and "further observation" has, he says, only contributed to confirm this belief.

"Indeed, I cannot see how it is possible to arrive at any other conclusion, especially if we take into consideration the important fact that this lesion (1) occurs at all periods of life, as well as (2) in all parts of the encephalic mass, at one time (3) as an acute, and at another as a chronic affection; (4) that it is often produced by external injury, and (5) by the pressure of certain tumours, or (6) apoplectic effusions; that (7) it is frequently combined with suppuration in other parts of the brain; (8) and, finally, that it occasionally supervenes during the progress of malignant and other fevers." (p. 357.)

We have introduced the figures here for purposes of reference. For the soul of us, we cannot see the force of the arguments from one to four inclusive. The conditions enumerated are not peculiar to diseases of inflammatory origin, and are, therefore, without the kind of significance Dr. Gross would attach to them. Arguments five and six carry little, if any, weight with them. The statement (No. 7) is absolutely incorrect; and (8) the softening during fevers is of a different nature altogether from the "white softening" causing paralysis and contraction. And here, nevertheless, are the arguments which Dr. Gross employs, in proof of the inflammatory nature of the change, in preference to the close anatomical reasoning of Durand-Fardel, whereby the colourless softening is connected, step by step, with a previous condition of red and (by all admitted to be so) indubitably inflammatory softening. This is the more strange, as Dr. Gross makes much use in respect of other points of the volume of Durand-Fardel;* but pathological anatomist though he be, the tendencies of his mind evidently lead him to accord more favour to loose general reasoning than to close deductions from *observed facts*. To the question "does mollescence of the brain ever get well?" he gives as a reply a vague

* We beg to refer the reader to a former Number of this Journal (July, 1843) for a full analysis and estimate of the labours of the French writer.

statement (unintelligible, from its conciseness, it must be to those who have not previously studied the very difficult subject) of the opinions of Cruveilhier and Sims.

Is it not a curious thing to find the author quoting Dr. Stokes of Dublin as an authority for the extreme constancy of the law, that paralysis arising from cerebral disease affects the side of the body opposite to that of the diseased hemisphere? Why any particular authority should be quoted for the fact does not very readily occur to us; still less can we understand why Dr. Stokes should be the individual named. We should have been better pleased to find Dr. Gross attempting to explain upon sure and accurate grounds the cause of the occasional exceptional cases, in which the paralysis exists on the same side of the body as the disease producing it.

Dr. Copland, according to Dr. Gross, has only met with hypertrophy of the brain "*three* times in *several thousand* cases." This is an interesting observation. In order to assign it its entire value, we only require to have some insight *into the number of thousand times* Dr. Copland has opened the cranium and carefully examined the brain! Dr. Gross's description of atrophy of the brain is extraordinarily defective. He makes not the slightest allusion to congenital atrophy (a state of deep interest in all its relations), and leaves his readers without a syllable of information as to the characters of the atrophy of the convolutions (attended with serous effusion) so frequent in lunatics affected with general paralysis.

In turning over the writer's chapter on post-mortem softening of the mucous membrane of the stomach, our attention is attracted by two statements: first, it is not correct to affirm that Louis regards this state as one produced during life, and, "caused by a high state of inflammatory irritation." He *did* once so regard it no doubt, but, in the second edition of his work on typhoid fever (published so far back as 1840), he distinctly renounced his error, and made due acknowledgment of the truth of Dr. Carswell's notions. Secondly (and this is much more important), Dr. Gross states that "if the gastric juice be neutralized by a small quantity of magnesia, no softening, whatever, will happen in the stomach of an animal that has just been killed;" and this statement is of, at the least, excessively doubtful accuracy. We are well aware that the proposition is Dr. Carswell's; but it was the business of Dr. Gross to show that others (Dr. Simpson and Imlach) have found that neutralization of the acid does *not* prevent solution. Admitting this, the admission by no means throws any shadow of doubt on the general truth of the idea of the cadaveric nature of the change,—but it constrains us to look for the solvent agent in some other element of the gastric secretion besides its acid or acids.

Dr. Gross has not properly distinguished the two forms of cicatrization of "simple chronic ulcer" of the stomach,—that form which is attended with puckering of the surrounding mucous membrane, and disappearance of all direct evidence of loss of substance, and the other form, in which a distinct loss of mucous substance is apparent to the eye, while puckering is altogether deficient, and the cicatrix-tissue is a flat fibrous lamella free from corrugation. He commits, too, a practical error in affirming (p. 573) that "the symptoms of carcinomatous disease of the stomach are never so urgent when the lesion is seated near the cardiac extremity, as when

it involves the pylorus." Far from this statement being of uniform truth, in some of the most terrible cases (symptomatically considered) of cancerous disease of the stomach on record, the cardiac orifice was the part of the organ implicated. What Dr. Gross *meant* to say was (we descry reason for imagining) probably correct, but he has expressed himself ill.

Dr. Gross treats fully the subject of internal strangulation of the bowels, and we regret that we cannot afford space for an analysis of his chapter on the subject. However, as the merit of his observations consists rather in his excellent arrangement and clear description of the causes of strangulation, than on any actual novelty in his facts or inferences, this is less to be regretted. We like the observation "of the hundred and one *symptoms,* which have been enumerated by authors as indicative of the presence of worms in the alimentary tube; there is only a single one that is of any value, and that is the appearance of these bodies in the evacuations." (p. 633). Perfectly and absolutely true,—and would that, of many of the diseases of the alimentary canal, for which *a hundred and one symptoms* are set down by systematic writers, we had *one* as sure *clinical* indication.

The perusal of Dr. Gross's book leaves, on the whole, a favorable impression on our minds. He may not be a man of first-rate abilities, but he is zealous and honest. On the one hand, he gives no evidence of originality in research or in thought; and on the other, he is unquestionaby deficient in that critical faculty which is essential for the successful sifting of collected materials. But we, nevertheless, willingly admit that he deserves great credit for having executed his labours so well; we receive his book with thanks, and, looking on it as a *transition* work, we recommend it as the most complete and, on the whole, the least defective compilation on the subject in the English language.

Art. IV.

Recueil de Mémoires de Médecine, de Chirurgie, et de Pharmacie Militaires; redigé sous la Surveillance du Conseil de Santé des Armeés. Publié par Ordre de S. Ex. le Ministre Secrétaire de l'Etat au Département de la Guerre.—Paris, 1815-1846.

Collection of Memoirs on Military Medicine, Surgery and Pharmacy. Published officially.—Paris, 1815-1846. Fifty-eight volumes, 8vo.

In taking a review of the periodical literature of the present day, a class of publications which exists in France cannot fail to attract our attention, as being of a description to which we possess nothing analogous in this country. It consists of journals published under the control and at the expense of government, and consequently possessing in a great measure the character of official documents. Their importance as affording the means of disseminating information among particular classes of individuals, and of thereby leading opinion in some measure, by an appeal to reason, is deserving of deep consideration. This would involve a greater amount of space and leisure than we can spare, and would moreover lead us into a discussion of too general a character to be suited to the

pages of a medical journal. Among the periodicals of the kind referred to, however, is one which is recognized by the French government as the official organ of the army medical department, and which was instituted with a view to promote sound instruction among its officers. We propose in the following pages to give a brief sketch of the origin of this journal, and to notice one or two of the most striking papers which have appeared in its pages.

I. In 1763 Dr. Richard de Hautesierck, inspector of military hospitals, pointed out to the Duc de Choiseul, then minister of war, the advantages which would accrue to the medical department of the army, from calling upon the surgeons attached to hospitals to give a regular account of their practice, and to correspond on the subject with the inspector-general, who should be empowered to publish the results of that correspondence. The minister authorised Dr. Richard to carry out his plan, and to collect and publish at the expense of government any interesting observations and rare cases which might thus be communicated to him. In 1766 he accordingly brought out a quarto volume, entitled, 'Recueil d'Observations de Médecine des Hôpitaux Militaires,' wherein, after laying down the plan on which the journal was to be in future conducted, he pointed out the necessity of studying the medical and physical topography of the countries commonly occupied by the troops, and especially the salubrity or insalubrity of the various garrison towns, barracks, prisons, and hospitals. He also gave several reports of cases, descriptions of epidemics, some topographical memoirs, particularly of the towns of Montpellier, Châlons-sur-Saône, Toulon, Lille, Bitche, and Strasburg, and a formulary of prescriptions for the use of the military hospitals. The gratuitous distribution of this work excited the zeal of the medical officers of the army, and increased the amount of correspondence on these subjects. In 1772 a second volume was published, which contained four memoirs on topography, five on epidemic diseases observed in France between 1764 and 1770, with many medical and surgical cases. Dr. Richard, for his services, received the riband of St. Michel, and was created Baron de Hautesierck.

In 1781 an ordonnance was published on the subject of the medical department of the army, by which, among other things, the 'Journal de Médecine, de Chirurgie, et de Pharmacie Militaires' was established; it was to appear every three months, and to be compiled by a retired consulting physician of the army. The object of this journal was to promulgate facts and opinions relative to the preservation of the health of soldiers, or to the successful treatment of their diseases, and nothing foreign to the medical department of the army, or of the military hospitals, was to be inserted. The first volume was published in 1782, and it continued to appear regularly till 1789, forming seven octavo volumes.

The changes in the administration of the army by the council established by the minister of war in 1788, caused the publication of the journal to be suspended. It was not intended to suppress it altogether, but the new directory of the hospitals announced, in 1789, that it would no longer be brought out at stated times as a periodical work. From this date till 1801, the instability of affairs in France, and the numerous calls of duty

on the council of health of the army, prevented the preparation of another volume. In that year several officers were appointed to prepare a summary of the most important papers which had been collecting during the preceding twelve years, but before this was completed their services were required with the grand army. Nothing further appears to have been done till 1815, when the journal was re-established, MM. Biron and Fournier Pescay being appointed the editors. It was at first brought out in bi-monthly numbers; but this having been attended with many disadvantages, the editors resolved in 1817 to publish it for the future in half-yearly volumes, and the title was at the same time changed to that which it at present bears.

The minister of war, in his letter to the inspectors of hospitals in 1815, states the object of the journal to be, "to diffuse sound instruction among the medical officers of every rank, and to communicate to them without delay the discoveries which shall be made in the theory and practice of the healing art. All the medical officers are called upon to contribute materials to the journal. The publication of their labours will have the double advantage of being useful to the service, and of maintaining among all a noble emulation. In short, this journal will become a depôt where each one may treasure up the result of his researches and the discoveries he may have made."

To obtain the materials necessary for carrying on this work, the principal medical officers of hospitals and the surgeon-majors of regiments were directed to forward monthly reports embracing all subjects relating to the health of the troops, either in the prevention or treatment of disease; they were also to give a detailed history of rare cases of disease among the soldiers; an account of any epidemics, with their probable causes and most successful treatment; meteorological observations, &c. The principal medical officers of hospitals were likewise to transmit quarterly, numerical returns of the admissions and deaths, and of the diseases by which these were caused. If these were furnished regularly, but little use appears to have been made of them, which we the more regret, as army medical officers possess opportunities of compiling satisfactory reports which rarely fall to the lot of the medical profession in civil life.

The editors being fully impressed with the importance of the study of military hygiene, called the attention of the medical officers to the advantages to be derived from a careful examination of the "rules and precepts relating to the preservation of the health of soldiers, and to the most suitable means for removing or diminishing the fatal influence of the numerous causes of disease to which they are exposed both in peace and war." M. Biron, in the second volume of the journal, published a valuable memoir on this subject, in which he directed attention to the principal objects of study. These he arranged under seven general heads:—1. Of the choice of the soldier—his physical and moral qualities—the influence of military discipline on the recruit. 2. Of the diet of soldiers. 3. Of the clothing of the troops. 4. Of their quarters:—*a*, barracks; *b*, military prisons; *c*, hospitals; *d*, camps and bivouacs. 5. Of marches, exercises, and military works; the influence of, *a*, victories; *b*, retreats; *c*, captivity. 6. Duties of officers—discipline and habits of soldiers—inculcating the

maxim "qu'il faut le défendre contre lui-même, et lui faire du bien malgré lui." 7. Of the duties of surgeon-majors of regiments.

Fifty-eight volumes of this journal have now been published, a monument of the industry of the medical officers of the French army, and of the zeal and good sense of the council of health. The subjects chiefly treated, besides numerous interesting cases in medicine and surgery, are hygiene, medical topography, histories of epidemics among the troops, clinical reports from various military hospitals, surgical histories of campaigns, reviews of works on military medicine and surgery, biographical notices of deceased medical officers of the army, extracts from the addresses to the pupils of the military hospitals at the annual *concours*, and the names of the successful candidates at these *concours*.

It is much to be regretted that a work of a similar nature has never been established in the British service. The Army and Navy Statistical Reports already published, furnish much interesting and valuable information regarding the topography of the military stations, and the prevalence of disease and mortality among the soldiers and sailors employed in several of our colonies and dependencies; but the observations on the other subjects above mentioned pass unrecorded, or, if recorded, are at least very rarely made available to the use of the profession. That there is no lack of zeal in the collection of materials, let the accumulated documents at the Army and Navy Boards bear witness. But for what purpose such accumulation, if the papers are to lie untouched, their contents uncommunicated to those likely to benefit by them? These reports and observations, contributed during upwards of a quarter of a century, by officers employed in all parts of the world, many of whom have been men of considerable literary and scientific acquirements, doubtless contain much that would interest the statesman, the ethnologist, and the natural philosopher, that would conduce to the advancement of medical science, and that would prove useful and instructive to the junior officers of the department. It is a source of much regret that no synopsis of the best papers thus immured in the record rooms of the Boards has ever been published, or that the materials of so large a collection have not been methodically digested, arranged, and made easily accessible to the profession. We understood, some years ago, that it was in contemplation to condense and publish the more important documents at the Army Medical Board; and although this plan seems for the present in abeyance, we trust it has not been finally abandoned.

It has been remarked of this country that we possess no military literature; we are, certainly, by no means rich in military medical literature. The field of observation and exertion is wide and inviting; and the opportunities enjoyed by officers of acquiring information, in regard to climate, and the diseases of troops in the stations where they are employed, are in many respects excellent. The medical department of the army, however, have much cause to lament that so few of their body raise a memorial of their talents, zeal, and industry, by publishing the result of their extended and varied experience. From the communications of those who have enjoyed superior opportunities of observation and of acquiring knowledge, we may naturally expect to gain much useful information upon the in-

fluence of climate, and the means of preserving the health of soldiers; and we think it will be generally admitted that those officers who have attained high rank, and filled desirable appointments, owe it as a duty to their country, and to the service in which they have gained their honours, to place upon record, and thus render available, the results of their long experience. Sir John Pringle and Dr. Jackson, by their highly valuable works, established a claim to the perpetual gratitude of the army, and left behind them imperishable monuments of their great talents and indefatigable industry, and an example worthy of being followed by their successors.

But in thus asserting the claims of the army and navy upon individual officers, we by no means wish to underrate their claims upon Government.

M. Begin, in an address delivered at the annual *concours* of the Military Hospital of the Val de Grâce, remarks—

"If the surgeon in civil life commits any mistakes, they are isolated, injure only the individuals, and are reproduced but at long intervals; while the errors of the military medical officer, acting upon masses, may assume the nature of a public calamity. The first, when his practice is irrational, or very unfortunate, loses the confidence of his patients, and the evil checks itself; the other, armed with military rank, pursues his course steadily, while the men who are intrusted to his care cannot escape from his treatment. An extreme caution, therefore, is imposed upon the army medical officer. While adopting the progressive improvements of science, he ought never to forget that our soldiers cannot, guiltlessly, be made the subjects of rash experiments."

Such being the case, it surely becomes the duty of Government to afford to medical officers the means of acquiring early and authentic information on all subjects connected with the preservation of the health, or the treatment of the diseases of soldiers; since, from being scattered over the face of the globe, they have not the same opportunities as their brethren in civil life of becoming acquainted with the recent discoveries in medicine.

The 'Journal de Médecine Militaire' does not appear to us to have accomplished all the objects for which it was instituted, or to have realized the anticipations which might reasonably be formed of its usefulness. But this has resulted rather from the manner in which it has been conducted than from any fault in its original constitution. Indeed, we are inclined to think it is mainly owing to the original plan having been neglected, and a faulty system substituted. For instance, the reviews of new works on medicine and surgery have been almost altogether omitted, and the biographical sketches are so few as to justify the inference that this branch is considered of very secondary importance. The compilation of numerical returns from the hospitals, one of the most striking and important features in the original plan, has never been carried out, while an undue proportion of the volumes has been devoted to the history of cases. Besides, the distribution of the journal is confined, we understand, to the principal medical officer of each hospital and to the surgeon-majors of regiments. This appears rather to savour of sending coals to Newcastle; for if the object of the journal be the diffusion of sound instruction among the medical officers of the army, it would seem most natural to place it in the

hands of the juniors, who have yet much to learn of the practical part of the profession, and not confine the distribution to the seniors, whose practice may naturally be supposed to have been matured by long experience, and many of whom would not alter their own views were all the books written since the days of Solomon arrayed against them. Whatever may be the reason, it is certain that the journal is not in high favour among the medical officers, whose best papers generally appear in the ordinary medical periodicals, and not in this, their official organ.

II. We shall now proceed to extract a few of the most striking observations which we have met with in looking over this work. The first we shall submit is a contribution to the statistics of gunshot wounds, from a ' Surgical History of the occurrences which took place at Lyons, during and after the six days of April" (1834), by Dr. Laroche, P. M. O. of the Military Hospital there (vol. xxxvii).

GENERAL TABLE OF WOUNDS.

				Admitted.	Died.	Recovered.
Gunshot Wounds	of the Head	Cranium		12	4	8
		Face		14	2	12
	of the Trunk	Neck		8	2	6
		Shoulders		8	3	5
		Chest	penetrating	6	5	1
			non-penetrating	12	2	10
		Abdomen	penetrating	5	5	0
			non-penetrating	9	0	9
		Pelvis	penetrating	7	5	2
			non-penetrating	13	0	13
	of the Upper Extremities		Arms	20	8	12
			Forearms	5	1	4
			Hands	10	1	9
	of the Lower Extremities		Thighs	39	9	30
			Legs	41	8	33
			Feet	22	2	20
	of the Articulations		Elbow-joint	4	3	1
			Hip-joint	1	1	0
			Knee	7	5	2
			Foot	3	3	0
Fractures	of the Arm			1	0	1
	of the Leg			1	0	1
Slight contusions				29	0	29
				277	69	208

This table does not show a very favorable result of the treatment, 1 in 4 of all the cases admitted having died. It would be unfair to compare this with the results of wounds received in action, because, in street fighting, from the greater proximity of the combatants, the nature of the weapons employed, and the advantage the populace have in firing from windows, &c., the injuries received by the soldiers are usually of a very severe description—the only correct standard of comparison would be that of wounds inflicted under similar circumstances. In the 30th volume of the journal, M. Hippolyte Larrey has given a surgical history of the cases admitted into the Hospital of the (ex) Guard in Paris, during the three days of July, from which it appears that 20 died out of 266 admitted. He has not, however, given any tabular statement of the nature of the wounds, and we are consequently unable to ascertain in what particulars they dif-

fered from those reported by M. Laroche. This is a point of great importance in making a comparison, as will be seen on examining the results of the wounds of the chest, abdomen, and pelvis—15 having died out of 18 cases of penetrating wounds, while only 2 died of 34 in which the cavities were not entered. Wounds of the articulations also proved remarkably fatal, 12 having died out of 15 admitted.

The following were the results of the capital operations performed in the hospital at Lyons:

			Died.
Trepanning		1	1
Amputations	at Shoulder-joint	2	1
	of Arm	7	6
	of Forearm	1	0
	of Thigh	4	3
	of Leg	5	3
Ligature of Femoral Artery		3	1

M. Larrey has given the details of 16 amputations which were performed in the Hospital of the Guards, from which we have obtained the following results:

			Died.
Amputations	at Shoulder-joint	2	1
	of Arm	4	2
	of Thigh	5	1
	of Leg	5	3

Of these, 8 were primary, and 2 died; while of 8 secondary amputations, 5 terminated fatally.

The mortality arising from disease in an army on active service is usually *much greater* than that caused by the immediate casualties of the battle-field, and the success of military operations often depends on the General being able to bring up his forces unencumbered with sick, and in a condition to go through the arduous fatigues of a campaign. It is, therefore, one of the most important, as it is one of the most difficult duties of army surgeons, to prevent disease and maintain the troops in a state of health and efficiency. This can only be done by a careful examination into the causes of sickness among them, by diligent attention to the subject of their diet, dress, quarters, drills, &c., and by prompt treatment of disease the moment it manifests itself. The following heads of recommendations by the medical officers, for the preservation of the health of the troops, promulgated at Burgos in May, 1823, when the French army was advancing into Spain, are taken from a history of that campaign, by M. U. Coste, vol. xvi:

"1. The soldiers always to wear their *cloth* trousers.

2. Not to be permitted to undress, as they usually do, on arriving at the halting-place or at their bivouac in the evening.

3. To wear their great-coats whenever the air is chilly, and when they are not on the march or working.

4. In wet weather, to halt for the night on ground a little elevated, sloping, and sheltered from the wind.

5. To increase in such cases the number of fires, and to keep them up till the time of starting.

6. In wet weather, to make an additional distribution of brandy on starting, and on arriving at the bivouac.

7. In hot weather, to march the troops early in the morning, or in the evening, resting during the middle of the day in valleys or sheltered places.

8. If linen trousers are permitted to be worn, to make the soldier put on a girdle of cloth or woollen stuff round the belly.

9. To make frequent halts, selecting places where the water is good, and to take care that the men do not drink cold water when greatly overheated.

10. To give them regular exercise when in cantonments.

11. To be careful that their clothes and shoes are kept up and repaired.

12. When compelled to use bad water, to mix a little vinegar or brandy with it.

13. In marshy places, to encamp, if possible, on the high ground."

A most important branch of the study of military hygiene is medical topography, as by it we are led to consider the influence of the various physical agents upon the health of the soldier, and to remove him when practicable from exposure to the causes of endemic diseases. This subject has received a considerable share of attention from the medical officers of the French army, and in this journal we find many excellent topographical memoirs. As these have been mostly drawn up on a uniform plan, it may not be amiss here to quote from a paper, by M. Estienne, the subjects to which the attention of the officers was especially called in preparing them:

"1. To point out the nature of the soil of the country to be described.

2. The longitude, latitude, and general description.

3. The prevailing winds. Are these proper to certain seasons, to certain determined periods? What are these periodic winds?

4. What are the principal physical qualities of the water of the rivers, ponds, and wells? What influence do they exert on vegetation, and on the health of man and animals?

5. What trees, shrubs, and other plants, particularly culinary and medicinal, grow in the places described?

6. What grains are cultivated? How are they cultivated? And what are their diseases?

7. To examine carefully and describe the numerous medicinal substances exported from the country, particularly to France.

8. The animals of all classes peculiar to the country. To collect as much information as possible regarding the domestic animals which lighten and share the labour of man.

9. Finally, to make known the general temperament of the inhabitants, their food, drink, clothing, the construction of their houses, their habits and manners; to point out the most ordinary diseases of the children, men, girls, and women, and the customary mode of treating them; to state at what period menstruation begins and ends; if fecundity is considerable; and what is the ordinary term of life."

In the later numbers of the journal are many topographical memoirs of the various stations in Algeria, which cannot fail to prove useful to the medical officers sent to join the army there. The numerical information, however, which is afforded regarding their salubrity is very scanty and very defective. It is probable that this information may be withheld from motives of political expediency, but what is given is very imperfect, the details relative to the French soldiers being mixed with those of the native auxiliaries, the strength of the troops being rarely stated, or other important omissions existing, which render the observations of little value to medical science.

Those who have studied the effects of a tropical climate upon the European, compared with the African or Asiatic, as illustrated by the Statistical Reports on the Health of the Army, will at once comprehend how uncertain must be the results derived from returns in which the two are mixed, while no means are furnished whereby to separate them, nor any estimate given of the relative numbers of each class. This remark is particularly applicable to the French army in Africa, because the principal diseases giving rise to sickness and mortality there are of endemic origin, and consequently of that description in which the difference is most strikingly marked. The deaths in hospital of all the troops, in 1841, amounted to 104 per 1000 of the strength. The only return we have met with relating to French soldiers alone, is one giving the amount of sickness, mortality, and invaliding in the 55th Regiment, at Bona, during the first year of its service in Africa. From this it appears that, in twelve months, they lost 250 per 1000 of the strength by death, and that a similar number were sent home to France as invalids, thus reducing the regiment, in that short period, to half its original numbers.

We had intended to have gone at some length into the subject of the sanitary condition of the army in Algeria, but, as all the returns are open to the very serious objections above noticed, and as all the deductions from them must consequently be viewed with mistrust, we have been reluctantly obliged to give up this interesting investigation.

In the fifty-fourth volume is a long and interesting paper by Dr. Casimir Broussais, on an epidemic of cerebro-spinal meningitis, which prevailed among the different garrisons in France from 1837 to 1842, and which appears to have been a disease of great intensity. We briefly noticed this epidemic in a previous number, but, as the present paper is compiled from the reports sent to the Council of Health by the military medical officers, and therefore comprehends a description of the progress of the disease through France, at least as it appeared among the troops (and it was chiefly confined to them), we shall condense the principal facts contained in it. It commenced at Bayonne, in 1837, among the military, and soon spread into Les Landes, many cases occurring among the inhabitants of the communes surrounding Dax. Thence it extended to Bordeaux, and in the same year to La Rochelle, in both of which places it was confined to the garrison. It then suddenly appeared at Versailles and St. Cloud, where it raged from 1839 to 1842. From Versailles it spread eastwards to Caen and Cherbourg, in 1840 and 1841; westwards to Metz, Strasbourg, Nancy, and Colmar, from 1839 to 1842; and southwards to Laval, Le Mans, Château-Gonthier, Tours, Blois, and Joigny, and finally appeared in the neighbourhood of Rambouillet. From La Rochelle it reached Poitiers in 1840, Lorient in 1841, and Ancenis and Nantes in 1841 and 1842. In all these places the disease was chiefly confined to the military.

But, while it thus extended in a northerly direction, it also spread among the garrisons to the west of Bayonne, appearing in 1837 at Narbonne and Foix, in 1838 at Toulon, and in 1839 at Nîmes. It prevailed at Avignon in the winter of 1839-40, and again in the following year; at Montbrison in 1840; and at Lyons in the winter of 1841-2. It

appeared also at Perpignan in the winter of 1840-1, and the following year at Aigues-Mortes.

The progress of the epidemic was not marked by regularity, nor did it pursue a steady course from one garrison to another. Occasionally it appeared at a distant point, from which it sometimes returned to places it had passed over, while at other times it remained stationary for a considerable period. In some garrisons the disease did not prevail as an epidemic, but merely a few sporadic cases occurred; in others it appeared to become naturalized, and to take an endemic character.

The disease, as has been already stated, was confined chiefly to the military, but in a few instances extended its ravages also to the civil population; for instance, at Strasbourg, in 1841. Wherever it appeared in an epidemic form it was most fatal; upwards of half the cases reported having terminated in death. This is fully borne out by the following summary of the result in such of the garrisons as furnished numerical returns:

	Cases.	Died.
Bayonne, January to March, 1841	28	21
Versailles and St. Cloud, first epidemic, February to June, 1839 .	156	69
,, second epidemic, 1841, and first quarter of 1842 . .	53	39
Caen, 1840	10	4
Cherbourg, second quarter of 1841	2	2
Metz, November, 1839, to March, 1840 . . .	40	22
Strasbourg, November, 1840, to May, 1841	184	108
Nancy, January to August, 1841	28	8
Colmar, February to April, 1842	7	5
Laval, March, 1840, to March, 1841	69	44
Le Mans, second epidemic, 1840	9	3
Blois, January and February, 1841	12	4
Poitiers, December, 1840, to February, 1841 . . .	20	8
Ancenis, December, 1841	12	4
Foix, April, 1837	16	6
Montbrison, September to December, 1840 . . .	47	16
Perpignan, October, 1840, to April, 1841	50	28
Lyons, February to March, 1842	9	4
Aigues-Mortes, November, 1841, to March, 1842 (civil population) .	160	120
Strasbourg, January to December, 1841—cases reported among the civil population	150	90

Thus it appears that in the returns furnished to the Council of Health of the Army by the medical officers, 1062 cases are recorded, of which 605 proved fatal. It is much to be regretted that the numerical details respecting this disease are not more comprehensive and more complete. M. Broussais makes some judicious remarks on the necessity of *systematic* investigation; many of the results forwarded to the council being unavailable, from the imperfect manner in which the facts are recorded. These remarks are well worthy of the consideration of the medical officers of the French army, as applicable not only to the present instance, but to almost all the statistical papers in the journal under review; and we believe that a perusal of them by the officers of our own army and navy would not be a work of supererogation. It is to incomplete observations we owe the existence of a host of *false* medical facts.

The disease is stated to have occurred chiefly among recruits and young soldiers. Several statistical documents are adduced in support of this; but

they are of no value, as they merely show the numbers attacked in each year of service, without giving the strength out of which the cases occurred. From such data it is obviously impossible to estimate its relative prevalence among the different classes of troops. The predisposing causes of the disease are alleged to have been fatigue, crowded state of the barracks, those excesses into which young men are apt to run on first joining the army, with excessive drill, nostalgia, and the excitement attending the change from civil to a military life. It is said to have prevailed especially in the months of January, February, March, and April—periods corresponding with the arrival of the conscripts at their regiments. No meteorological phenomena were observed, which could account for the prevalence of the epidemic; nor was there any greater amount of cerebral disease during these years. No grounds exist for supposing that the disease was contagious.

The epidemic meningitis presented itself under two forms: 1st, of excitement, and, 2d, of collapse.

1. That of *excitement*. In this form there was a strong, quick pulse, sparkling eyes, red face, quickness of speech, headache or neckache, occasionally convulsive movements, and the patient made frequent complaints. Consciousness sometimes remained unimpaired, while at others there was temporary delirium, alternately disappearing and recurring. This at length terminated in the stage of collapse.

2. The form of *collapse* was characterized by general collapse, insensibility, coma. This, however, did not come on suddenly. The patients at first complained of fatigue, malaise, prostration, listlessness, and were often accused by their comrades of laziness and shamming. Dull, heavy headache followed; they lay with their eyes half closed; their speech was slow and painful; pulse slow and small; they had confusion of ideas, and vertigo. Coma was generally not complete; but although the patient could be roused, he answered questions with difficulty.

Although this form was usually the consequence of that of excitement, it was often developed as the primary stage of the disease. Both forms ran into a typhoid state, and sometimes became complicated with paraplegia or hemiplegia. Rachialgia was an invariable premonitory symptom. In the course of the disease marked intermissions occasionally were met with. Many cases, which were slight in appearance, terminated unexpectedly in death. Towards the end of the epidemic, the disease usually assumed a milder character than at the commencement.

On examination of the bodies after death, the membranes of the brain and spinal cord were found to present all the usual appearances of acute inflammatory action, from those of simple sanguineous injection and thickening up to serous or purulent infiltration. "The existence of a coating of false membrane in layers, flakes, or patches, has everywhere been the characteristic lesion."

The remedies employed have been general and topical bloodletting, ice to the head, sinapisms and vesicatories to the extremities, stimulating liniments, and the actual cautery to the spine. Emetics and purgatives, calomel and opium, and mercurial frictions, and, lastly, quinine.

Large bleedings in the early stage of the disease were the only means

of treatment attended with success. The amount of blood to be drawn was measured by the effect upon the pulse, and not by the quantity. Unless treated thus energetically in the commencement, the patient had a very slight chance of recovery. In many of the cases the blood did not show the buffy coat. After the stage of excitement had passed, blisters to the head were found to be attended with benefit.

Such is a brief recapitulation of the circumstances narrated by M. Cas. Broussais relative to the progress, nature, and treatment of this remarkable epidemic—remarkable for the insidious manner of its attack, its extremely fatal character, unless treated most actively and at once, and its being confined almost exclusively to the military. We have seen one or two cases having a strong resemblance to those related in this memoir, but they occurred sporadically; and we are not aware of an epidemic of this nature having ever been observed in this country.

Perhaps we cannot conclude our present remarks more appropriately than by the following quotations from addresses delivered to the students at the Military Hospital of the Val de Grâce. These prove sufficiently that, although in the official journal of the medical department, the aid of statistical investigation has been greatly overlooked, many of the officers are fully aware of its importance; and we trust, ere long, to have it in our power to notice an increased attention to this branch of science.

"We ought undoubtedly," says M. Sedillot, "to value the truths which antiquity has handed down to us, and not to despise the experience of ages; but it is also necessary to apply to them the system of rigorous analysis which characterizes our era. . . . . . . We know that the mortality of the army weighs particularly upon the young conscripts; that it is increased by the change of garrisons; that it is greater in some places, as in Paris, or in localities affected by epidemics, than in others; but medicine has scarcely contributed to these results; they almost all belong to the labours of the administrative department. And if we are asked what are the most common diseases, and in what proportion do they attack soldiers, according to their age, length of service, distribution, their former profession, and a thousand other circumstances, we can only give approximative data, which might very easily be rendered certain, if we collected observations of the diseases. What one man could not do, several, by their co-operation, might easily accomplish. Our profession does not admit of repose; and in the intervals of practice we ought to strive unceasingly to increase our knowledge, and to extend the sphere of our usefulness."

"In military medicine, especially," says M. Begin, "statistics ought, as regards the modes of recruiting, the race of men, their regimen, drill, clothing, dwellings, removal from one climate to another, to lead to precise results, which it would be impossible to procure with the same degree of certainty in any other way. Figures, as has been often remarked, are inflexible, and cannot speak falsely; but we must exert ourselves to collect them, and group them according to their analogies; we must require from them only legitimate conclusions, and must bear in mind that the greater the numbers the more surely will the errors arising from negligence, or individual accidents, be cancelled, and truth predominate in the general result."

ART. V.

1. *Histoire de la Médecine depuis son Origine jusqu'au XIXe Siècle.* Par le Docteur P. V. RENOUARD. Tom. ii.—*Paris*, 1846. 8vo.
2. *Saggio sopra il Modo e le Regole di osservare e massime in Medicina.* Del Dr. L. EMILIANI, Prof. Emer. di Clinica Medica e Medicina Pratica nella Regia Universita di Modena, &c.—*Modena*, 1844. 8vo, pp. 107.
3. *Zur Charakteristik der Medicin der Gegenwart.* Von Dr. J. M. LEUPOLDT, offentl. ord. Professor au der Universität zu Erlangen.—*Erlangen*, 1846. 8vo, pp. 93.
4. *The Medical Police of the United Kingdom.* From the Westminster Review for March. (A reprint.)—*London*, 1846.

THE student of M. Renouard's history may make much of it, if he be a dexterous reader, and have a vivid imagination. He may conjure up the dead, mighty in medical art and science; he may pass them before him as in a panorama. The shadowy forms of Egyptian priests will head the array; then will follow the scarcely less shadowy Greek priesthood, and the silent and contemplative disciples of Pythagoras. The Cnidians, and then the Asclepiades, marshalled by Hippocrates, will follow these; Socrates, Plato, and Aristotle mingling in their ranks. Then, once more, a long array of Egyptian sages will appear, the masters of the Alexandrian school, flanked by those of Pergamos, with a multitude of disciples, Pagan and Christian, Asiatic and Latin. Close upon these tread the Arabians, intermingled with Greeks of the declining empire; and misty forms bring up the rear, a motley commingled multitude. On they pass, until again Greek figures are visible, marshalling on the men of the dawning middle ages. Strange is their array! bearded and gowned necromancers, cowled priests, short-frocked bathmen and barbers, hooded women. Motley, too, their banners! The symbols of astrology and Christianity are written on them, with the names of Aristotle, Hippocrates, and Galen, of Plato, Avicenna, and Paul of Egina, of Celsus, Boëtius, and Cælius Aurelianus. But the light dawns more brightly as the panorama moves on, and Galileo, Bacon, Vesalius, and Harvey and Sydenham, and sages, whose names are as familiar to the medical student as his own, appear in the scene. They are intermingled, however, with Rosicrucians and alchemists, pretenders, and fantastic dreamers, amongst whom we recognize John Brown and Mesmer, Hahnemann, and our old friend Dickson; he of "chronothermalism," being both last and least.

The contemplation of such a *coup d'œil*, as this history of medical progress affords, cannot but instruct us. Who were the greatest? we ask, who the most worthless? how did the great attain to their fame? why were the unworthy worthless? when did medical art advance, and why? when did it recede? A consideration of questions like these must surely be of use to guide the ambitious or the noble of the profession, and to designate the false and frivolous. Those who fully appreciate the value of medical art to society, and rightly estimate the immense power slumbering in its practisers, may surely learn something from the history of two or three thousand years, and be better able to advance the one, or rouse and organize the other.

The profession and practice of medicine has no history anterior to the early Greek age. From the meagre details handed down by historians, it would appear that a perfect medical organization formed one of the elements of Egyptian society at the time Herodotus wrote; and medical art had its fixed rules, any innovation on which was penal. It would appear, too, that at a period, long anterior to Herodotus, the laws of hygiene formed part of the moral or religious code of Egypt, and probably of the nations of India and Central Asia. What influence these ancient doctrines had upon Greek literature is uncertain, but it must have been considerable, for it cannot be doubted that there was an extensive interchange of thought between the Greek and Egyptian priesthood, and in later times, between the Greek and Asiatic philosophers.

M. Renouard divides medical history into three great ages. The first includes the foundation of medical science, and extends from the mythological age to the time of Galen, or the termination of the second century of the Christian era. The second, which he terms the age of transition, includes the time between the end of the second century and the commencement of the fifteenth. The third is the age of renovation, and includes the time from the commencement of the fifteenth century to our own era. These ages are subdivided into periods or epochs. The first is the primitive period, or period of instinct, and ends at the fall of Troy, about twelve centuries before Christ. The second includes the mystic, or sacred period, and occupies from the fall of Troy to the dispersion of the Pythagoreans, 500 years before Christ. The third is named the philosophical period; it occupies about 180 years, and terminates with the foundation of the schools at Pergamos and Alexandria, 320 years before Christ. This school commenced the anatomical period, which ended with the second century after Christ. It was followed by the Greek period (the fifth), which continued until the destruction of the Alexandrian library, in 640. Science then passed over to the Arabians, and the Arabian period commenced, to end with the fourteenth century. With the revival of letters, medical science revived, and the erudite period began, which, so soon as the recovery of ancient literature was perfected, ended in the last, or reformation period, and which includes the seventeenth and eighteenth centuries. The first half of the nineteenth century belongs to the present period, and is at present left untouched by M. Renouard.

The progress of medical art and science, and of the medical mind, through those ages and periods, is typified in the progress of each individual student. At the outset, medicine was doubtless instinctive, although we are by no means inclined to place this outset at so late a period of the world's history as the fall of Troy. There is sufficient proof, we think, that it had grown to a science in the great Asiatic empires long before this comparatively modern epoch. But M. Renouard's notion applies accurately enough to European science. There is satisfactory evidence that two points only were involved in medical practice in Europe at this early period. The one, was a succinct description of diseases, or nosology; the other, an account of remedies suitable to the cure of the diseases described, or therapeutics.

With the progress of medical observation, it would doubtless soon be found, that a knowledge of healthy structure and function conduced

materially to a better knowledge of nosology and therapeutics, and thus the sciences of anatomy and physiology would arise. The prevention of disease would also be an instinctive consideration, and hygiene would be developed. These, then, nosology, therapeutics, and hygiene, are the kernels of medical science. M. Renouard adds two other branches, orthopædy and phrenology, as completing medicine, which he defines to be the science that has for its object the preservation of health, the cure of disease, the physical perfection of mankind. A more comprehensive, but not a more concise, definition might be adopted than this; but it is sufficient for our present purpose, which is to inquire how this object can be best attained.

The works before us sufficiently indicate the agitation of the professional mind on this point. These examples taken from several such, mark the characteristics of the present epoch of medicine. They prove that the professional mind is bent upon a fuller development of medical science and practice than has been yet recorded in the world's history. Like straws on a deep and mighty current, they show its direction and course. The resolution is evidently taken, that there shall be a fuller investigation of theories and principles; a higher appreciation of the transcendent importance of medicine; a more systematic attempt to concentrate all human knowlege to work out its objects, regardless of prejudices and dogmas. We shall, therefore, take these works as text-books for our purpose, and proceed to draw such lessons of experience from the past, as may enable the profession to work out more successfully a nobler future.

Looking upon the panorama of medical history, we find it divided into two great periods, each in one degree the counterpart of the other. The one extends from the Greek origin of the science to the irruption of the Saracenic tribes, and is complete in itself, inasmuch as it comprises the first rise and extinction of medical science in Europe. Just as the knowlege of Asia and of Egypt was extinguished in passing over with empire to the Greeks, so the knowlege of the latter passed over to the victorious Arabians, and Europe sunk into barbaric darkness. This period continued until the age of renovation, which began with the fall of Constantinople, the immigration of learned Greeks into Italy, and the culminating point of Saracenic power in the fourteenth century. This second period is, however, as yet incomplete, inasmuch as the extreme point of development is not yet reached. In all other respects, it may be compared with the first. It had this advantage, however, that it commenced on a wider basis than its analogue. The accumulated knowlege from which the science of the first period was developed was imperfect, because the sciential records of the mystic age antecedent to it, were confined to an exclusive priesthood and a hieroglyphical language, and much, therefore, of Asiatic and Egyptian learning was lost to the Greeks. They began science almost anew, and transmitted their accumulated knowledge to their successors in a living language, and a language most perfect and rich. It was also expounded to them by men of acute and polished minds, who read and spoke the language in which it was written. This was an immense advantage; it was the advantage which the student with a master possesses over the student without a master. Nevertheless, the powers which guided the development of medical science in the two periods, the obstacles which

arose, and the methods which were adopted to overcome them, were the same in both. We shall, therefore, move through them on two parallel lines, and not consecutively with M. Renouard. We shall, however, adopt some of our author's rules of procedure as set forth in the subjoined extract.

"Celebrated physicians influence the progress of their science and the value of their art, not by their writings only, but by their oral teaching, their character, and their conduct. Their lives present both models for imitation, and faults and stumbling-blocks to be avoided. Often also the primary education of a man, and the circumstances in which he is placed, explain the turn of his genius, and give the key to his successes or reverses. For all these reasons, I have not neglected to give biographical details regarding the most famous physicians, especially when those details have some connexion with the general history of the art, or include some question of morals.

"The sciences are not isolated from each other in their progress; they hold each other by the hand; they are usually simultaneous in their advance. An exception to this rule occurred, however, in the history of the human mind in Europe. During the middle ages dialectics and theology were cultivated with success, while other branches of human knowledge vegetated in a deep depression. But at the fourteenth century industry, the sciences, and the arts awoke from their long sleep. On the one hand, the civil and political organization of the nations of Europe became more perfect, and their material well-being advanced; on the other, the intellectual and moral faculties of individuals were developed. Mind took a higher, bolder, and better flight. The historian of medicine would, it appears to me, fail in one of his duties, if he did not take from time to time a general view of the social condition of the people..........

"Another fact extremely remarkable, and worthy of special notice, in a history of medical theories, is that they are all derived more or less directly from some system of philosophy. Consequently, we should have but an imperfect knowledge of these theories if we did not investigate their philosophical source. Nevertheless, too great an importance should not be attached to these analogies, nor should we profess to estimate the value of medical theories by them. It should not be forgotten that a system of philosophy may be false as a whole, and yet be true in its special application to medicine." (pp 10-11, vol. i.)

There is great profundity in these views, and at the same time they are of great value, as will be seen afterwards.

Proceeding then with our task, we pass over M. Renouard's account of medicine, as taught and practised by the ancient Egyptians and by the Chinese. The medical art of the Greeks, during the mythological and sacred period, corresponds in most respects to that practised in the middle ages. It was in the hands of kings, priests, and pretenders, and was apparently traditional. Such was medicine amongst the mass in Europe during the dark ages, and such it has been found in all semi-civilized people. Amongst the ancient Gauls the Druids were the philosophers, physicians, legislators, and priests. So also in Peru, Mexico, Florida, and Cuba, the priests were the astronomers, philosophers, and physicians, and were learned in both medical and physical science.

It was the sacerdotal order in primitive Greece, as well as in Europe during the dark ages, that practised medicine as an art, and gave rise to the medical profession. The mythology of the Greeks led, however, to an earlier separation of the practisers of the art from the great body of the priesthood. The science of medicine had its patron god, and the first

Greek temple erected to his worship was a temple at Titane, dedicated to Esculapius, about fifty years after the fall of Troy. His worship soon spread through the whole of Greece, and from thence into Asia, Africa, and Italy. The most celebrated temples of Esculapius were at Epidaurus, Pergamos, Cos, and Cyrene. The priests were termed Asclepiades; each temple was at once a sanatorium and a medical college; and the medical knowledge the priests possessed was strictly confined to the initiated. The temples were at once miraculous springs and watering-places; and the priests acted both on the body and mind of their patient. M. Renouard observes, that the temples of the god of medicine were generally situate in the most healthy localities; sometimes on the summit of a hill, or the slope of a mountain; sometimes on the coast, at some little distance from the sea, or near a thermal spring, or perennial fountain. Plantations of trees pleased the eye of the sick, and afforded them cool and solitary retreats, or broad and spacious glades. The sick and luxurious crowded to these temples as they now a-days visit Priessnitz or Scarborough. The fees of these physicians were the offerings of the sick to the god, and consequently it was of some importance to their pecuniary interests that the superstition and ignorance of their patients should be suitably acted on. Aristophanes very humorously shows us one of the methods used for this purpose. They had tame serpents attached to their establishments, of which the Greek comedian indicates the use. A valet loquitur, who had been under treatment:

"The priest of the temple of Esculapius, after having extinguished all the lights, told us to go to sleep, adding, that if any of us heard the hissing which indicated the advent of the god, we were by no means to stir. We therefore all remained in bed, and made no noise........As for myself I could not sleep, on account of the odour of a basin of excellent soup, which an old woman had near her, and which tickled my olfactories most amazingly. Being therefore extremely anxious to creep near her, I raised my head, and saw the sacristan take the cakes and figs from the sacred table, and going the round of the altars, put all that he found on them into a bag. It occurred to me that it would be very meritorious to follow his example, so I arose and went to grab the old woman's basin of soup."

We have made a rather free translation of this curious passage. Of course, visions, relics, and miracles were of daily use and occurrence. M. Renouard thinks the Asclepiades secretly took down a detailed history of each case, and of the means used for the cure, and that they tabulated these according to the seat of the disorder, beginning with the head, and so proceeding downwards. They were content to observe what remedies cured certain diseases, and to employ those remedies in similar cases; a principle at the foundation of all medication, but seen in its greatest purity in the practice of lay amateurs and quacks. Nothing is more common amongst laymen than the expression, "take such and such a remedy, it cured a case exactly like yours." The difference between the scientific practitioner and the amateur is, that by study and experience, the former is able to discriminate resemblances and differences. But this power of discrimination is of slow growth. As a general rule, he cannot experiment like the physical philosopher. Nature and chance present him with *his* experiments, but their elements are never the same. It is often a

lengthened period before an analogous case again occurs, and when it does occur, it is almost impossible to separate the subject of his experiment (the sick person) from numerous disturbing causes. Hence the maxim of Hippocrates, the greatest of the Asclepiades, *art is long, life is short, opportunities rare, experience deceitful, judgment difficult.*

With the progress of society in Greece, a development of the intellect took place, and philosophy introduced new views both in theology and medicine. The reformation also began to dawn, and Pythagoras was both the Luther and Galileo of his age. We have not space for a summary of his doctrines, but we may shortly observe, that after a prolonged tour through the then existing academies and seats of learning in Asia, he returned to his own country, and became an itinerant preacher in Greece and Southern Italy of the opinions he had there imbibed. He preached the doctrine of two eternal uncreated essences, namely matter and mind; the unity of the Deity; the homogeneity of nature; its indefinite perfectibility, and a doctrine of development in animated nature closely analogous to that laid down in the 'Vestiges of the Natural History of Creation.' On this point we subjoin an extract from a commentary on his doctrines in the 'Nouveau Dictionnaire d'Histoire Naturelle:' "He could conceive a time when insects, shell-fish, or the impure reptile knew no superior in the universe, and was at the head of organized beings.... ....Who knows whether, in the eternal night of ages, the sceptre of the world will not pass from the hand of man to that of a being more perfect, and more worthy to bear it? Perhaps the negro race, now inferior, reigned at one time over the earth, before the white man was created. If Nature has necessarily granted empire to races which she has formed more and more perfect, why should she stop her progress now? Who shall place limits to her power? She is upheld by God only, and it is his powerful hand which guides her."

Pythagoras founded numerous secret societies in Greece (in accordance with the custom of the times), and especially in Magna Græcia. These societies soon exercised an important influence on the morals of society, and on the governmental policy of the states. But their success encouraged their members to become ambitious, and they entered actively into political agitation contrary to the advice of their founder. This conduct drew down upon them the vengeance of the authorities; the priests, too, whose superstitions they exposed, and whose power they undermined, thundered anathemas against them; and the ignorant and immoral hated them for the purity of their lives and the superiority of their knowledge. The consequence was, a general persecution and a complete dissolution of the societies during the lifetime of their founder. Have we no modern Pythagoreans to whom this result should not operate as a warning? Are there not philosophers in this our day who hold identical doctrines, and who are pressing imprudently on popular prejudices? We trust the enthusiastic spirits abroad will learn wisdom by this experience, and not bring those great truths of science, which ultimately will place religious truths in an impregnable position, in contact with the deeply-rooted and vulgar prejudices which with many are substituted for religion.

The Periodeutic Physicians. The dispersion of the Pythagorean societies led to the establishment of a true medical profession. Previously there

had been only two legitimate classes of physicians, the priest-physicians and the gymnasiarchs (corresponding to the bathmen, barbers, and chirurgeons of the middle ages), who superintended the health of the athletæ. But when the Pythagoreans were scattered abroad, a new state of things arose. While some became teachers of philosophy in the schools, and revealed the sciential secrets of their order, others commenced the practice of medicine, and visited the sick at their homes, a practice which gave origin to their name. Empedocles, of Agrigentum, was one of the most famous of this new class. A formidable competition thus arose against the priest-physicians, and they were compelled to abandon their monopoly of knowledge, and enter into the arena of literary and professional warfare with the rest. The priests of Cnidos were the first to publish, and they were followed by those of Cos (the temple, or college in which Hippocrates was attached), who gave to the world the basis of the series of treatises attributed to Hippocrates. These writings have various degrees of value, and were written by various authors. Hippocrates himself is reported to have followed in some degree the example of Pythagoras, and to have made a sciential tour through the Greek cities of Europe and Asia. The essays which properly belong to him are decided by M. Renouard, after a careful investigation, to be the following: 1. Some of the Aphorisms. 2. De Morbis populariter grassantibus; Lib. i et iii. 3. De Victus ratione in Morbis Acutis. 4. De Aëre, Locis, et Aquis. 5. De Articulus. 6. De Fracturis. 7. Mochlicus. 8. Predictorum Liber.

The whole collection, as it exists at present, was not made until about the period when the great scientific libraries of Alexandria and Pergamos were founded. It is supposed that the relatives and descendants of Hippocrates wrote several of the treatises in the collection, and sent them forth with the sanction of his name.

Hippocrates had probably derived some part of his high moral principles from the Pythagoreans. His love of truth and of virtue, his patriotism, his unwearied benevolence and unflinching disinterestedness, have contributed to render his name immortal, quite as much as his scientific and practical knowledge. He was the type of the philosophic physician; he had attained to that depth of knowledge, that he knew his own ignorance. He was also the contemporary and friend of Plato and Aristotle. The prejudices of the age and people prevented the cultivation of anatomy; this prevented progress in physiology, and thus the advancement of medical science could only be effected by clinical observation, and theories deduced from the philosophical systems in vogue. The Hippocratic era corresponded to the 14th, 15th, and 16th centuries, as, in that period, the superstition of the priests prevented the general cultivation of anatomy. The philosophical systems were also founded on foreign lore brought in by Pythagoras and his contemporaries, just as the dialectics of the 15th and 16th centuries were founded on the philosophy of Plato and Aristotle, brought into Italy by the fugitive Greeks from Constantinople.

When Aristotle had exhausted the philosophy of his day, he turned his attention to the cultivation of natural history, especially comparative anatomy and physiology. The philosophical period was about to pass into the anatomical. Aristotle's father was a physician (probably a periodeutic), and died when his son was young; and he, having entered the army, and

being a dissipated young fellow, soon spent his whole fortune. He then abandoned war for his early studies, philosophy and medicine; and attended Plato's lectures with the greatest assiduity. While a student, he was also a general practitioner, and kept an open shop. He was a Macedonian by birth, and had the good fortune to be selected by King Philip as tutor to his son Alexander; and when the latter proceeded to the conquest of Asia, Aristotle kept up a correspondence with him on matters of science, and advised with him as to the specimens in natural history that were the most desirable to be sent home. It is not probable that his was the first museum of natural history ever formed, but it must undoubtedly have been the most magnificent and extensive of the time. How interesting would the catalogue be at this day! The age of Aristotle corresponded to that of Vesalius, Fabricius, Eustachius, Servetus, &c. who lived at the end of the 15th and the commencement of the 16th century, and who were amongst the first to set systematically at defiance the prejudices against human dissections. Theophrastus followed Aristotle in phytology.

The Anatomical Period. The patronage which Alexander the Great bestowed upon the studies of his tutor was continued by his successors, but with increased vigour. Science now burst forth in her might. Eumenes in Mysia, and Ptolemy Lagus in Egypt, two of Alexander's generals, imitated their chief, and became rivals in scientific pursuits. The one founded the library and school of Alexandria, the other those of Pergamos. In a century after their foundation, the two libraries contained, according to report, above 800,000 volumes: of these, Pergamos possessed 200,000.

The rivalry between the two universities became so intense, that the kings of Egypt forbade the exportation of papyrus, hoping thereby to cripple the Mysian rival in the accumulation of books. This illiberal policy had fortunately a good effect, as it immediately led to the discovery of parchment as a substitute for the fragile papyrus.

Ptolemy Soter, and his son and successor Philadelphus, collected scientific men around them, arranged the Alexandrian library at a great expense, established an academy of science, or rather an university, and maintained a select corps of philosophers and teachers. Amongst these (maintained by Ptolemy Philadelphus), were the two distinguished anatomists, Herophilus and Erasistratus. The latter was reputed to be the grandson of Aristotle; he founded a medical school at Smyrna.

The whole cycle of science was cultivated by the Ptolemies. They fitted out commercial and geographical expeditions; they cultivated bibliography; they made extensive collections in botany and zoology. Medical science was, however, their favorite pursuit. They not only supplied their schools with subjects for dissection, but they dissected themselves. Such indeed, was the renown of the Alexandrian school, that, at the time of Galen, it was sufficient reputation for a physician to have studied there.

The foundations of correct anatomical knowledge were now laid, and much of the superstructure raised, although the anatomical school had but a comparatively short existence. But subtle questions in philosophy began to take the place of experiment and research; and, worse than that, the Romans conquered Egypt, and stopped human dissections. Julius Cæsar also destroyed the noble library, and although Cleopatra had that at Pergamos subsequently transmitted to Alexandria, her patronage was rendered

in a great degree useless by the folly of the imbecile Caracalla, who slaughtered great numbers of the inhabitants, abolished the salaries of the professors, and put a stop to their meetings and common halls. The principal writers of the Alexandrian school that have come down to us are Galen, Aretæus, Cælius Aurelianus, Celsus, Pliny, Dioscorides, &c. Galen was the prince of his age, as Hippocrates was of the preceding. He was a native of Pergamos,—from time immemorial the seat of a medical college, founded originally by the Asclepiades, and, subsequently, the seat of the university which rivalled that of Alexandria. When he was already celebrated for his acquirements in the languages, history, mathematics, and physics, he was twice specially called by Apollo, in a dream, to practise medicine, and he, therefore submitted to the divine will. After a professional tour, in which he visited Alexandria, he returned to Pergamos, and was appointed surgeon to the circus, and attended the wounded gladiators. He then tried his fortune in the great metropolis, and with the greatest success, for he got into practice amongst the highest classes of society at Rome. His rapid progress, his disdainful pride, which he took no pains to conceal, and his fierce abuse of his contemporaries, raised him up a host of enemies. In his book 'De Prænotione' he accuses the physicians of his time of base jealousy and stupid ignorance, calls them scoundrels, poisoners, &c., and declares that, having unmasked them, he will leave a city where a man is esteemed only for the luxury and wealth he displays, and where unblushing charlatanism usurps the confidence of a stupid and frivolous public. Many worthy members of our profession have taken upon themselves the duty of "putting down" quackery. Vain hope! Sisyphæan labour! Fools there will ever be; fat and noble fools, as well as a vulgar herd, and all will have their appropriate vermin to prey on them, just as animals (entomologists tell us) carry about with them their suitable pediculi and cimices. It is a love of truth that leads us to war with quacks and quackery. We regret to see honest minds led into mazy error, and it is from a sentiment of duty to them that we hoist aloft our beacon. If they will heed the light, well; if not, we smile at and pity them.

Galen did not take the matter as coolly as he ought. He left Rome for some quiet little town, (perhaps his alma mater, and native Pergamos), "where," to use his own words, "every man knew his neighbour, his birth, education, fortune, conduct; and where, consequently, vulgar pretence and quackery could obtain no countenance." His value was already too well known, however, at Rome, and the Emperors Marcus Aurelius and Lucius Verus called him back; he had, also, the confidence of their successors, Commodus and Septimus Severus. He died in the seventy-first year of his age. The epoch of Galen may be compared with that of Boerhaave and Cullen: it was, however, the culminating point of ancient medicine. The arrest of anatomical research led inquirers to occupy their minds in the digestion of knowledge already attained, and in the formation of sects and theories. All was confusion, but there was little progress. We shall revert to the theories and systems of this age as admirably illustrative of those of our own.

This unprogressive age was distinguished towards its close by compilers. The principal of these were Oribasius, in the fourth century (a

native of Pergamos); Aëtius at the beginning of the sixth, a student of Alexandria; Alexander Trallianus, a Lydian, the son of a physician, probably contemporary of the latter; and Paul of Egina, a student and probably a professor at Alexandria in the first half of the seventh century. The works of Paulus Ægineta constitute a complete cyclopædia of the medical literature of his day. He had scarcely passed from the scene, when the Saracens (A. D. 640) scattered the Alexandrian school, and its library of 500,000 volumes heated the public baths for six months. This catastrophe finished the Greek period. The Franks, Visigoths, and Lombards had already overrun civilized Europe, and the arts and sciences found protection only at Constantinople and with the Arabian caliphs. The following remarks on this period (with which the era of European medicine closed) are made by M. Renouard:

"At the time of Galen animals were still dissected; and this professor informs us that he used apes for his anatomical demonstrations, the structure of which closely resembled that of man. Sometimes, indeed, the army surgeons obtained permission to open the body of some barbarian which they had taken from the field of battle; but, ultimately, the practice of dissection ceased altogether, and the structure of the human body was studied only in books, the horror of the early Christians to cadaveric researches being greater than even that of the Roman Pagans, and the fathers of the primitive church launched their thunders against this violation of the mortal remains of man.

"This neglect of anatomy, without doubt, contributed much to the decline of medical art; but other causes co-operated not less powerfully. In the first rank must be placed the rapid spread of Christianity, which disorganized the Pagan schools, discredited the profane sciences, and ruined the teaching of them. It excited in all men an ardent desire for that religious controversy which created so much disorder in the rising church, and hastened, as is well known, the fall of the Eastern Empire. In the second place, the small number of those who continued to cultivate natural philosophy, encumbered by a vicious method, sought only for the explanation of phenomena in the works of the ancients, and, fearing to innovate, trod stupidly in the beaten path of the past. Only two men, Alexander Trallianus and Paulus Ægineta, during the lapse of four centuries, showed the least originality; the one adding a few observations to pathology and therapeutics, the other making some decided improvements in surgery." (Vol. i, p. 408.)

The Arabian period comes in like an interlude between the fall of ancient and the rise of modern European science. The Arab caliphs were enlightened rulers; it may be that the light of knowledge had shone upon them from Pergamos and Alexandria, and created in them a love of letters. Be this as it may, their courts were a refuge for the intellectual Christians, which each dominant sect persecuted from their country. Haroun al Raschid was the most celebrated of these. He established, at Bagdad, both hospitals and public schools. His successor made still greater efforts. The university of Bagdad, founded by him, was the most celebrated of the middle ages, and almost equalled that of Alexandria. He instructed his ambassadors to purchase medical and philosophical manuscripts at any price, and he employed a Christian for forty years to translate them into Arabic, paying him for his translations with, literally, their weight in gold. About the same period science flourished in Spain under the enlightened rule of the Moorish princes. The library of Cordova contained 224,000 volumes.

Rhazes is the first of the Arabian writers who appears in history; he

flourished at the close of the ninth century, and the commencement of the tenth. He was a Persian by birth, and was one of the most distinguished professors at the university of Bagdad, and the chief physician of the great hospital. At the age of eighty years he ceased to practise, from the loss of sight. In one of his works, dedicated to the Caliph Almanzor, he gives some advice on the choice of a medical attendant, which we think worthy quotation.

"Ascertain with care the antecedents of the individual to whom you propose to intrust that which is dearest to you, namely, your health, your life, and the health and life of your wife and your children. If that individual wastes his time in frivolous pursuits or in parties of pleasure, or if he cultivates too curiously arts foreign to his profession, as music, or poetry, or if, especially, he be addicted to wine, beware how you intrust to such hands so precious a deposit. He only merits your confidence who has applied himself at an early age to the study of medicine, attended upon able masters, seen many sick, and joins personal observation to a diligent perusal of the best writers; for it is impossible to witness everything, or investigate everything for yourself. The knowledge and experience of a single individual, compared with the knowledge and experience of all men and of all ages, is like a small thread of water by the side of a mighty river." (Vol. ii, p. 414.)

Hali Abbas, a Persian, and Avicenna, born A.D. 980, at Bokhara, were nearly contemporaries. The latter studied medicine and philosophy at the university of Bagdad; Albucasis was a native of Cordova, and lived at the commencement of the 12th century. He, as well as the former, was a compiler, principally of the Greek authors. Neither of them dissected; and their anatomical knowledge was derived almost altogether from the works of Galen.

Greece and Italy during this period were not altogether without light. Actuarius (a term signifying a court physician) lived at Constantinople, about the end of the 13th century. He wrote a book, 'De Methodo Medendi,' in which he expounded the Galenic doctrines, and introduced some Arabian therapeutics. In Italy, the school of Salerno enjoyed a high reputation. It had a reputation as early as the 8th century, or before; some suppose, indeed, that it was founded by the professors who were driven from Alexandria by the Arabs in A.D. 640. It attained its greatest splendour between the 10th and 13th centuries. Constantine, of Carthage, surnamed Africanus, was one of its most distinguished professors at the end of the 11th century. Other remarkable physicians and surgeons of Western Europe were, Gerard of Cremona, who died in 1187; Guillaume de Salices, professor, first at Bologna, then at Verona, died in 1277-80; Arnauld de Villanova studied first at Paris, then at Montpellier, and afterwards in Spain, died in 1313; Lanfranc, a pupil of Guillaume de Salices, professor of surgery at Paris, in 1295; Jean Pitard, the reputed founder of the College St. Côme, in Paris (which previously, in 1311, was what our London College of Surgeons was a century ago, namely, a community of barber surgeons); and, lastly, Guy de Chauliac, a contemporary of the preceding.

We have brought our historical sketch down to the revival of medical science in Europe. The preceding authors and professors knew little except Arabic literature; the rays of Greek science came to them by a sort of double reflection. As the siege and sack of Alexandria led to the

expulsion of the Alexandrian professors, and the foundation of the school of Salerno and of the Italian universities, so the siege and sack of Constantinople, in 1483, led to the revival of literature in Europe, by the expulsion of the learned Greeks collected in that city. These took refuge in Italy, and carried off with them all the manuscripts possible. Some years previously to their arrival, however, the study of Greek literature had commenced; as, in 1428, Nicolas Leonicenus, professor at Ferrara, translated the Aphorisms of Hippocrates, and some books of Galen, into Latin. Linacre, the founder of the London College of Physicians, was a pupil, at Florence, of Demetrius Chalcondyle, a Greek refugee, and translated some of the treatises of Galen. On his return, he founded a chair at Oxford, and another at Cambridge, for the expounding of the Hippocratic and Galenic works.

We need not retrace the ground we have already gone over. We have previously shown that this period is in all respects analogous to that in the ancient era, in which Pythagoras, followed by his disciples, by Plato, Aristotle, and Hippocrates, established the erudite school in Greece. That erudition led to dissection, the study of natural history, and natural philosophy, and ultimately to the foundation of the anatomical school in Alexandria. During the *renaissance* in Europe, dissections of lower animals only were practised; and it was not until A.D. 1482 that the University of Tubingen obtained permission from Sixtus IV to dissect the human body. From this period the prejudices against the practice began to soften down, and the modern anatomical school was founded.

Having thus marked the broad outlines of medical history, we will retrace our steps, and consider the theories, systems, and methods of treatment of the two great epochs, directing our principal attention, however, to that period of ancient medicine most nearly resembling our own, namely, the epoch subsequent to the anatomical period. We have seen that the light of science, amidst all the mutations of society, was never extinguished, but was handed down from generation to generation, that connected the past and the future with linked hands. We learn, also, that the influence of philosophical theories and systems, if once generally received into men's minds, never ceased to operate, and that the new were always connected with the old. We see, also, that there was a constant connexion between the theories of the day and the experimental researches of the day; and that the former were true and useful in proportion as the latter were numerous and profound. We shall see, too, that bold theories led to renewed researches, and renewed researches ended in renewed theories: a significant fact! and not to be passed over slightly by those who denounce all theory as useless.

Origin of nosological systems and medical theories. We have already observed (after M. Renouard) that the fundamental principle of therapeutics is founded on experience only—on the instinct to apply those means to the cure of diseases which have already been found useful in similar cases. Now, the first great difficulty in acting on this principle is to determine what cases are truly similar; and hence arose the necessity of accurate observation as the groundwork of accurate diagnosis. Without this, the physician was liable to continual mistakes, in considering cases as alike which were altogether dissimilar. This desire for more accurate

diagnosis may be traced through every period of medical history, and has never been so impulsive, and so rich in results, as in the present. The discovery of aids to diagnostic research is, indeed, the chief characteristic of modern medicine; and here we beg to state our first requisite to improvement in medicine, namely, a further development and perfection of the instruments by which observation may be rendered more minute and more accurate. There is, doubtless, ample room for this. All the senses, and also the intellect, may be much more efficiently aided than they are at present.

The first effort at diagnosis was, doubtless, an elaborate classification of histories of cases, according to their phenomena. But as all the phenomena were at first mingled together, whether essential or not, the method led to confusion. This was pointedly noticed by Hippocrates, as the fault in the writings of the school of Cnidos. The necessity thus arose to distinguish the phenomena that were permanent or transient, essential or accessary, primary or secondary, &c.; and out of this arose discussions as to the nature and essence of diseases, their causes, symptoms, progress, &c. Thus medical theories arose concurrently with nosological systems, and the study of philosophy and physical science began. By this time society had advanced in Greece; a wealthy class was formed, who partly devoted their time to intellectual pursuits, or encouraged the fine arts; and knowledge was vigorously sought after. The theories of supernatural causes were extinct amongst the better classes. This same desire for knowledge which, in recent times, has brought enterprising and wealthy Hindoos and Parsees to Europe, and has induced them to send their children to Paris and London for education, nay, which has brought students to Great Britain from distant China, operated on the ancient Greeks; and the intelligent and wealthy penetrated into Asia, that they might learn the arts and sciences of the ancient and highly civilized communities spread over its surface, and flourishing in its magnificent cities. Pythagoras is the only one of those who has left his name to posterity; but it is certain that he was not the first, although perhaps the greatest; for he left the impress of his mind on the physical and metaphysical science of Greece. The influence of his philosophy may be distinctly traced in the medical theories of the Hippocratic writings. The doctrine of the harmony of numbers is seen in the doctrine of crises and of critical days; in the theory of the four elements and the four humours (derived from Plato), of two elements, and of unity or one element.

Plato was the modifier of previous doctrines. He was, as regards medicine, the parent of numerous metaphysical theories. He considered that everything was to be discovered by meditation and mental intuition—by the force of thought only. He attempted to apply the mathematical method (already so successful) of founding physical science on axioms, or general principles, and plunged into the wildest hypotheses. The treatise 'on Ancient Medicine,' in the Hippocratic writings, contains a critique (as M. Renouard thinks) on Plato's method, which was adopted by many physicians, and shows that it is vain, and could lead to no result. The author stands up distinctly for observation and experiment in medicine. He maintains that there is no need to know first the nature of man, how he was created, &c., but that it is sufficient for the physician to observe

accurately what is injurious to man, or what is beneficial, and to study his relations to surrounding agents.

Aristotle was a reformer in doctrine, as well as his master. He opposed him. He established that maxim which has descended with unimpaired force to the present day, "Nihil est in intellectu quod non prius fuerit in sensu." And this he applied practically.

"When," he says (we quote M. Renouard's quotation), "from a great number of experimental ideas a general idea is deduced respecting a whole order of similar things, that constitutes art. To have the knowledge that a certain remedy has been useful to Callias, to Socrates, and to many others, that is experience; but to know such a remedy is useful to all the individuals of the same species attacked by a definite affection, for example, to all men troubled with phlegm, or with bile, or with burning fever, that is art."

But Aristotle's views were influenced by those of Plato his teacher, and his second principle is, "that the first ideas excited in our minds are general ideas," and thenceforward he treads the Platonic road. But M. Renouard very justly remarks, that Aristotle confounded vague, obscure, and indeterminate ideas with true general ideas. This we think an important point to notice. What is the first mental act in the investigation of a patient's case? Do we not instinctively form at once a general idea of its nature, deducing this idea from the history of the first few symptoms? When the patient complains that he has cough, expectoration, and difficulty in breathing, do we not at once form a general idea of thoracic disease? Yet this general idea manifestly differs from a general idea, principle, or fact in philosophy; for the one is vague, doubtful, and indeterminate, while the other is precise, certain, and definite. And it is to the former class of ideas that philosophical doubts are applicable. On this point Aristotle observes, "the men who wish to learn, must first know how to doubt, for science is nothing else than the solution of antecedent doubts; but he who knows not of the knot cannot untie it."

The philosophy *proper* to Aristotle, was that which was antagonistic to Plato's, and which Bacon, Locke, Hume, Condillac, and others among the moderns, have adopted. It was the doctrine of experiment and observation as the basis of science, and led to all the discoveries in anatomy and physiology of the Alexandrian school. Nevertheless, the spiritualism of Plato advanced on the same parallel with it, and was indeed fed by it; the one was not free from the other, and as facts, observations, and experiments accumulated, the spiritualists, whether experimenters with Aristotle or true Platonists, seized upon them, generalized them into vague, doubtful, and indeterminate principles, and established new theories and systems. And this was done even to the most modern times. Leibnitz, Descartes, and Kant are the most prominent amongst the moderns of this school, but inductive philosophers, as Sir I. Newton for example, often practised the method. We think, indeed, that spiritualism is as instructive in its history and origin, and as necessary to scientific progress as sensualism; its office is to raise in men's minds the philosophic doubts we have just alluded to, and urge on to further researches for their solutions; to tie the knots which can only be untied by some accurate and more extended researches. Thus TRUTH has to emerge, like the beautiful order of creation, from the action of continually antagonizing forces, and rise in ma-

jesty and glory from what appears utter confusion. Why then should scientific rixæ be frivolous, or foolish, or embittered? Let medical philosophers, at least, carry on the perpetual flux and reflux of opinions and facts, and of facts and opinions, with the same still grandeur and profound repose amidst unceasing motion, which characterize the forces of the universe.

We shall not trace in detail the progress of anatomical and physiological science, and of the art of healing, made during the anatomical period. It is sufficient to say, that when compared with the knowledge of the previous erudite period, the progress was comparatively as great in every branch of medicine, as that which has been made since the revival of letters and science in Europe, or even during the 1600 years which have elapsed since its decline. It was a wonderful step, and could only have been effected by high intellect, urged on by the regal munificence we have before mentioned. In proportion as experimental research declined, spiritualism or dogmatism became predominant, and divided philosophers and the profession into numerous sects. Some followed the old masters, and some the new, but others (the Eclectics) acknowleged no master whatever. Amongst the earliest leaders of the anatomical school, the metaphysical doctrines and theories of Hippocrates, Plato, Aristotle, and Praxagoras—the successor to Aristotle (whose pupil, indeed, he was)—held firm ground, and while they experimented, they vaguely generalized, and were lost in mazy hypotheses. The sects were then termed Hippocraticians, or Praxagoreans, or Herophilists, &c., just as in modern times, there were Aristotelians, Galenists, Animists, Brownists, &c. But these sects had in their turn antagonistic sects. Philenus of Cos, and Serapion of Alexandria, for example, attacked the fundamental doctrine of Herophilus and others (derived from the Greek masters), and asserted boldly, that the doctrines of elementary qualities, cardinal humours, essences of diseases, were all false and hypothetical. They even rejected that fundamental doctrine which their opponents had derived from the Greek medical philosophers, —*contraria contrariis curantur*. They confined themselves entirely to the results of experiment and observation, and were the determined opponents of all spiritualism, all *a priori* general ideas, all hypotheses. They were the founders probably of the sect of Empirics, and maintained views closely analogous to those expounded in the Hippocratic treatise on Ancient Medicine, already referred to as that in which the doctrines of Plato and his method of research are condemned. Medical science and skill were to be acquired, the Empirics said, by three modes. The first was, personal observation, or autopsy; the second, the study of observation recorded by others, or history; the third was, epilogism and analogism, and consisted in deductions drawn from autopsy and history, and enabled them to make discoveries, or led to further observation. The autopsy of the Empiric was made with the greatest care, that errors in observation might be avoided. It was necessary that a disease should have been carefully watched under every variety of complication and circumstance; and a method of treatment carefully followed in the same manner, before the autopsy of that disease was considered complete. In short, they appear to have used a numerical or inductive method. The practitioner who had thus carefully observed a disease, and preserved a perfect knowlege of its symptoms, course, termination, and treatment, had arrived at a theorem; and he who

had accumulated a sufficient number of theorems, was an experienced or empirical physician.

Epilogism, or analogism of the Empirics, was the application of the experience of known diseases to unknown by means of resemblances or analogies; that is to say, a new disease was treated according to the method established for one to which it had the closest resemblance. And the same plan was adopted in substituting one remedy for another.

The definitions of the Empirics never included latent causes, or essential, elementary, or primary properties. They never defined fever, with Galen, to be a heat contrary to nature fixed in the heart, or, with Asclepiades of Bythinia, an acceleration of the motion of the blood, caused by obstruction of the pores; they simply said, fever is an affection which manifests itself by acceleration of the pulse, increased heat, and sometimes thirst. These definitions were termed hypotyposes. M. Renouard discusses the question, whence the rational or philosophical empiricism arose. Some authors, he observes, thinks that it was a deduction from the doctrine of the Sceptic or Pyrrhonian sect: Galen was one of these. We have observed, that the methods adopted by different philosophers have all a foundation in Nature. We generalize instinctively, but we observe instinctively, too, previously to the generalization. Plato adopted the process of generalization only, and thought truth was to be attained by the intellect alone. Aristotle adopted more decidedly than his master the other part of the mental process, namely, observation. But the knowlege attained by this process is never stationary; the process is continually repeated, and new observations and generalizations correct the preceding, and show their imperfection. This renders the truth of our generalizations doubtful, and the exclusive adoption of this part of the mental process (the scepticism) characterized the sect of the Pyrrhonians. Now it appears to us, that the Empirics adopted all the three stages of mental action into their philosophy; with Aristotle they observed, and inferred (autopsy and theorem); with Plato they generalized, and inferred (theorems and epilogisms); and with Pyrrho they doubted both the accuracy of their observations and of their generalizations. The doubts kept them within the bounds of common sense: in short, they appear to have been the truly practical men of their day; to have adopted the methods best calculated to attain to a true natural history of disease, and to improved therapeutics, and they may be taken as the best model for imitation by the moderns to be found amongst the ancient sects.

We quote the following interesting remarks by M. Renouard; their application to our own times is manifest:

"The circumstances amidst which the system of Empiricism was promulgated, were most favorable to its propagation. Medical theories had fallen, as we have seen, into confusion All principles, all opinions, all methods were again subjected to inquiry. The recent anatomical discoveries, the introduction of a great number of new medicines, the properties of which were undetermined, the continually increasing rancour of philosophical disputes, had all shaken the ancient dogmas without substituting better, or any which commanded the general assent. Amidst such conjectures, a doctrine which promised to put an end to the continual changes of dogmatism, and to fix the barren uncertainty of scepticism, by relying entirely on the testimony of facts,—such a doctrine ought to have been received with enthusiasm, particularly by those practitioners whose daily experi-

ence demonstrated the uselessness of dialectics in the advancement of medical science."

Empiricism was active for a century and a half; it then fell, and its fall was complete. It was altogether banished for ages, and its name became a synonym for ignorance and pretension. But when it arose again in the systems of Bacon, Locke, and Condillac, it aimed at a universal dominion in science. M. Renouard very clearly shows some at least of the causes of its decline. The ancient Empirics did not go further than the construction of secondary generalizations; they did not arise to first principles or to definite axioms. They resembled in short, the builder who stops short in the erection of a great edifice. Such a doctrine could not satisfy the speculative temper of the ancient philosophers, and it was therefore neglected. We are inclined to think, however, that the cause of its decline may be more properly attributed to a failure in progressive observation. The cultivation of general, or descriptive, and pathological anatomy became impossible as Christianity spread, and physics were so imperfect, that no aids to observation seem to have been invented. There was thus an arrest of development in science, or rather a marasmus. No new matter was added to its system, and therefore it remained like a blighted creature. On the other hand, the scientific mind being turned from the observation and the accumulation of new facts, was perforce driven to expend its active energies on successive theories and systems, each being doubted and discarded almost as soon as set up.

Asclepiades of Bythinia established one of these systems; he founded, or led to the foundation of the sect of the Methodists. He went to Rome to teach rhetoric, and by his genius, talents, and tact, obtained the patronage and society of the most illustrious and aristocratic society in the metropolitan city. Not satisfied with this success in his proper profession, he turned doctor, and his ambition led him to aspire to be the founder of a new system and sect. The Epicurean philosophy being then the most fashionable at Rome, and being that which he had doubtless taught, he based his new system on the physical dogmas of Epicurus and Democritus respecting the atomic constitution of matter.

It is worthy note, that this has almost been the invariable influence of the doctrines of the day on medical science. During the climax of the erudite period of Europe, Fernel based his pathology upon the doctrines of Galen, which were then fashionable, and again established the therapeutic dogma (derived by Galen from Hippocrates) *contraria contrariis curantur.* The study of the occult sciences at the same period made their way into medical theories with Paracelsus. When these had flourished a while, a reaction took place, observations accumulated, and Galen and dogmatism gave way to Hippocrates and experimental philosophy. But of the latter numerous theories arose, not unmixed with antecedent ideas. Van Helmont's system included an "Archeus," derived from the occult sciences, and the doctrine of ferments derived from the science of chemistry, just emerging into day from the researches of the Rosycrucians and Alchemists. The theories of Sylvius, Willis, &c., were founded almost altogether on chemistry. The progress of mathematical and mechanical philosophy raised up a school of iatro-mathematicians, who attempted to reduce the problems of life to equations, and to explain vital action by

mechanical theories. The circulation of the blood just then discovered and established by Harvey, also occupied a large space in all the theories of the day, but especially in that of Boërhaave. The hypothesis of Descartes led to the medical theories of Stahl; while Boërhaave, Van Swieten, and the later iatro-mathematicians, had Cullen and his contemporaries for their descendants. We thus arrive at another important lesson, namely, that the influence of prevalent theories is invariably exercised on those which displace them; this, therefore, should be carefully estimated and guarded against.

To return to Asclepiades. His theory was only a screen for hygienic practice. He was a Hahnemann without his infinitesimal doses, or the maxim "similia similibus curantur." His watchword was, to open the pores. Exercise, agreeable varied diet, wine, &c., was very pleasant treatment for the luxurious Roman, and as Asclepiades was a finished scholar and wit, and contemned and ridiculed all medical science and literature, he was very popular.

Themison was the disciple of Asclepiades; his doctrine differed, however; it was antagonistic,—analogous to that of Brown, with strictum and laxum for sthenia and asthenia. The treatment by the metasyncritical circle was peculiar to the sect of Methodists, an account of which is given by Cælius Aurelianus, and of which Themison was one of the founders. We have not space for the doctrines or therapeutics of this sect; it is enough to say, that they were so general and so simple, that one of the sect, Thessalius Trallianus, declared he could teach the whole of medical science in six months. Methodism had a close family resemblance through metasyncrisis to hydropathy, but a course of dietetics took the place of hydriatrics.

We must here close our notice of M. Renouard's work with a summary of the lessons which we have drawn from the history of medicine. We first find that therapeutics must be founded on experience. The method of treatment to be adopted in any particular case must be that which has most frequently been found effectual in some previous and analogous case. This experience, to be perfect, must be founded on an exact diagnosis, so that the analogy on which the treatment is based may be perfect. Exact diagnosis cannot be established without—1. An accurate observation of diseases, and a true record of their natural history. 2. A systematic arrangement and perfect generalization of the phenomena observed. These necessarily demand to be arranged on some principles or laws, and generalized according to some method. The latter must be the inductive method, the former the laws of vital action deduced by that method, or in other words, physiology; which in its largest meaning includes anatomy.

Theories, and vague, general ideas, or hypotheses, may be safely used to advance medical science and art. In establishing new remedies and modes of treatment, or in discovering the therapeutics of new diseases analogies may also be permitted, as instruments of progress. But in both instances a rational empiricism is carried out, and a philosophical scepticism should be practised as to the value and accuracy of the theories, hypotheses, and analogies thus adopted, and they should be tested in these particulars by new observations, and inductive generalizations. Thus then the foundation of all improvements in medical art and science, is observation.

It is, therefore, a question of vital importance to determine how observation may be best advanced and perfected. But, first, let us define the meaning of the term observation, and let us analyse the act of mind by which an observation is completed. The etymological meaning of the term implies more than an act of the senses; it means the careful investigation of a thing, and thus it includes an act of mind; for nothing can be investigated carefully without reference to its relations, whether they be the antecedents or coefficients. Consequently, the reasoning as well as the perceptive powers are involved in an observation.*

We become so apt at observation by practice, and the accumulated knowledge of science is so familiar to us, that the process of observation seems simple and intuitive. While feeling a patient's pulse we note its condition, and determine its relations to the heart, lungs, and other organs of the body, and to the general symptoms of the case, without any distinct consciousness of our existing knowledge, or that there was a time when these relations were not known—a time when the physician was ignorant of the structure and functions of the heart—when he knew not the difference between arteries and veins, or between the heart and the liver. The relations of the pulse were formerly, in fact, less known and understood, and the observation of the true value of its variations more difficult, than the microscopic relations of excretions are now. This conjunction of two distinct mental acts is strongly marked out by Professor Emiliani, as necessary to constitute a true scientific observation. First, a clear and precise conception of the thing to be observed; next, an examination of its reciprocal relations and connexions. He observes the one cannot be separated from the other, and the judicious combination of both mental acts is necessary to the progress of science.

The clear conception of a thing, and of its relations, implies, first, a proper perfection and use of the senses: a training or education of the senses is evidently necessary to attain this point, however perfect they may naturally be, just as the raw giant requires a pugilistic training before he can enter the ring. This training should not only develop the senses physically, but should systematically teach how the errors of sense may be avoided—a matter now left entirely to experience. It should also inculcate a thorough scientific knowledge of the aids to the senses which have been invented from time to time, as the stethoscope, &c., and of the principles on which they are constructed.

The examination of the relations and connexions of the thing observed (the second process in the art of observing) implies scientific knowledge. In medicine, anatomy, physiology, chemistry, &c., are accessory or collateral sciences, and are only valuable to the physician by extending his knowledge of these relations and connexions. When the stethoscope got into general use, it became absolutely necessary to determine more accurately than had heretofore been done the whole mechanism of cardiac and pulmonary action, so that the relations and true pathological value of the sounds communicated along that instrument to the ear should be properly

* Sprengel's definition of observation is analogous to this. He says: "Sequitur animadversionem *observatio*, quæ nihil aliud est, quam attentissima et studiosa succedentium sibi phænomenorum animadversio. Quæ ut in observationem transeat, oportet primum attendere animum ad res, earum formas, qualitates, et proprietates." (V. Sprengel, Inst. Med., vol. i, in introitu.)

estimated. The relations of the urinary secretion, and of the urinary salts, remain yet almost a sealed book. The first process of the act of observing is difficult, because it demands a refined chemical analysis for its completion; and the second is impossible, because we are yet far from an accurate knowledge of the vital processes by which the urinary constituents are formed in the organism, and eliminated by the kidneys, and know still less of their intimate relations and connexions with morbid phenomena.

If medical observation be considered in all its relations, whether theoretical or experimental, it will be at once manifest that no one individual can grasp the whole. No one can have the necessary perfection of *all* the senses—no one can possibly understand the application of *all* the aids to research—no one can be the walking encyclopædia of the medical sciences, and the miracle of medical knowledge, which should understand the relations of the whole cycle of observanda. A division of labour has been found practically necessary, as well in the scientific as in the social world. Hitherto, however, the division of labour has been almost fortuitous; anything like a system is of modern growth, and is to be found only in committees of investigation, which have arisen out of scientific societies. What is wanted in the medical profession is, that the whole body shall be organized, with a view to a systematie division of labour in practising the art, and advancing the science of medicine.

The author of the article in the 'Westminster Review' has attempted a sketch of such a systematic organization. Setting aside the plan for a union of the members of each "grade," in one or two central colleges, he proposes the assimilation of all grades in local colleges, for the purposes of self-government and scientific research. He argues that this principle of assimilation is already acted upon in the social unions of the profession; in the Medico-Chirurgical Society for example, and other societies of the metropolis; in the Provincial Medical Association, and other societies of the provinces; and that this principle is the only foundation for a perfect organization of the medical profession, because it has been found that, while all others have utterly failed, this only has been found to be attended with any degree of success, or can theoretically combine the different branches into one body. It certainly is necessary that the principle of association and systematic division of labour should be better carried out into practice, and local incorporations of the profession would be a good and substantial basis for the purpose.

The formation of therapeutical theories, and the observation of the action of remedial agents, are closely connected, both with each other, and with physiological and metaphysical theories. That theories and hypotheses are necessary to the progress of medical science and art we have already shown. The advancement of the curative art by analogies, or, in other words, the application of a method of treatment found by experience to be useful in one form of disease to another analogous form, is based on theory, or, at least, on a theoretical inference, which has to be tested or modified by careful experiment. It is known, however, that the human mind, in the mass, advances by oscillations between antagonistic theories and systems; and that the old are continually reproduced as new, to combat the old revived. It is necessary, therefore, that this continual re-

currence to past theories be restricted within its proper limits; and for this purpose we think the measure recommended by Professor Leupoldt is eminently suitable, namely, the establishment of a chair of medical history and theory in every school. Professor Leupoldt proposes that the course should be divided into two parts, and occupy two sessions. The first part to include biology, anthropology, and hygiene; the other, general pathology, therapeutics, and the history of medicine. We are certain that some such course would be beneficial, inasmuch as it would fortify the student's mind against a too ready reception of any theory, or a bigoted adherence to any. He would estimate them at their proper value as subsidiary, and not essential, to the progressive advancement of medicine: as the means and not the end. He would be prevented fixing his faith upon any one theory, or system, exclusively, whether physiological or therapeutical. He would not be a medical sceptic, and indifferent to all opinions and systems alike; for he would learn that truth is in all—is as omnipresent in human knowledge as the great God of Truth is in His creation. Thus the student would be trained to be a true Eclectic, and seek for the truth in every system and every theory. He would find it in allopathic, hydropathic, or homœopathic systems; in bio-chemical, dynamical, or histological theories.

In conclusion. Whatever may be the result of the attempt now making throughout the civilized world to thoroughly reform medical science and art, we feel certain that the opinion enounced by Bacon, with reference to science in general in his day, and which is quoted by two of the writers before us, must be the watchword of the men of progress:

Instauratio facienda est AB IMIS FUNDAMENTIS, *nisi libeat perpetuo circumvolvi in orbem, cum exili et quasi contemnendo progressu.*

Art VI.

Clinical Illustrations of the Diseases of India, as exhibited in the Medical History of a Body of European Soldiers, for a series of years from their arrival in that Country. By William Geddes, M.D., &c. &c., *late* Surgeon of the Madras European Regiment.—*London,* 1846. 8vo, pp. 492.

In the application of the numerical method to medical science, practical difficulties are encountered which, from their nature, must tend to throw doubt and uncertainty upon many of the results obtained. As one of the chief of these, we may notice the constant change of persons on whom the observations are made. In general practice an individual applies for advice, and after an attendance, perhaps of no great duration, recovers from his malady and is lost sight of; or, becoming discontented at what he considers the slow progress towards convalescence, politely bows out his medical adviser. Thus the subjects of observation are ever varying, and the effects of morbid action, or of therapeutic agents on each individual, are observed only for a short period. As a necessary consequence of this, the influence of age, temperaments, habits, and idiosyncrasies must constitute an important element in any calculation, and one of which it must be extremely difficult to form a just appreciation. Besides this, there is to be encountered the difficulty of ascertaining the previous medical history of the patient; who, sometimes from interested motives, and at others from

ignorance, misleads the physician on the subject of former illnesses, which may, perhaps, bear an important relation to the origin or nature of the present attack. The uncertainty whether the prescribed medicines have been taken, the morbid love some patients have of "cheating the doctor," forgetful that they may at the same time be inflicting a serious injury on themselves, and the difficulty, frequently amounting to an impossibility, of enforcing dietetic regulations, and of restraining hurtful irregularities—all contribute to diminish the value of numerical observations when applied to medicine.

From most of these objections the public services are free. The military and naval surgeon has under his care, generally for a series of years, the same body of men, with the exception of such changes as arise from deaths, and the discharge of those who, old or disabled, are replaced by young men, selected as being apparently of sound constitution and in good health. These men are immediately brought before him when attacked by disease, which he has thus the opportunity of observing *ab initio;* he has also documents to which he can refer for the history of their previous illnesses from the period of their joining the service; he has the uncontrolled power of following out a particular plan of treatment, and the means of ascertaining that the medicine prescribed is administered, and that the regulations as to diet are rigidly enforced. He can also obtain pretty accurate information regarding their habits, and is sure that they are provided with those essentials to health—food, clothing, and lodgings. With all these advantages there are, even here, very peculiar circumstances to be taken into consideration, as exerting a powerful influence on the development and progress of disease; such as sudden change of climate, night duty, the aggregation of individuals in barracks and ships, diet, dress, drills, night watches, punishments, &c. &c. Nevertheless it is, we believe, the least objectionable field for observation, and one which we have long regretted to observe has been very inadequately cultivated.

The following observations, although now of very old date, bear so pertinently on this point in its relation to the naval branch of the public service, that we make no apology for extracting them. They form part of a paper "On the Practice of Medicine in the Royal Navy," written by the Editor of this Review, and published in the 'Edinburgh Medical and Surgical Journal,' for July 1810.

"It is a circumstance greatly conducive to his improvement that the young surgeon is appointed to small vessels, and that he is removed to a more extended practice as he has, or may be supposed to have, increased in knowledge. Among 70 or 120 sailors (the usual complement of such vessels) there will rarely occur so much sickness as might tend to perplex him; but he will have time thoroughly to study every case of disease that comes under his care. Unembarrassed by numbers and want of time, unseduced by any symptom, however prominent, and which might attract the whole attention of the cursory observer, he can leisurely and coolly review the whole phenomena of the disease, appreciate the value of the different symptoms, and thus be enabled to attain one of the most important of acquisitions to a medical man—a knowledge of nosology. Thus instructed, he will enter upon a larger field of practice with superior advantages; and, as he progressively arrives at greater heights, will be enabled to correct his speculations, and increase his knowledge by more extensive observation. The usual number of sick on board of our largest ships is sufficient to exercise the faculties of the experienced surgeon without fatiguing them.

"It cannot have escaped the observation of any one, who has witnessed the customs and habits existing in the navy, that the naval practitioner enjoys very superior opportunities for becoming acquainted with the laws which regulate health. Forming his observations on a limited body of men, leading the mechanical life of sailors, whose every action is public, the naval surgeon has it in his power to know the whole body of *juvantia et lædentia* that act upon their systems. He may not only be intimately acquainted with the *character* of every individual, but he may easily ascertain the quality, and even quantity, of every man's food and drink, the nature of his habits with regard to exercise, air, clothing, &c., from one year's end to another. The objects of his observations being placed, as it it were, on a stage, isolated in the middle of the ocean, and receiving, unadulterated, all the influences of the sky—it is in the power of the naval practitioner, I had almost said exclusively, to ascertain the real effects of the atmosphere on health. Living in comfortable apartments, with artificial means to counteract the season's temperature, and evade its violence, or placed in a thousand different situations, which modify or defeat its power, the inhabitants of the country or the city cannot be said to exhibit pure specimens of atmospheric influence. But the sailor, to whom no close apartments, or artificial heat, ever temper the season's rigour, or from whose head no roof or umbrella wards off the rain or sunbeam, must exhibit, in the conditions of his system, the real effects of the weather on health. And various, indeed, are the changes of the sky to which the British sailor is exposed.—

> ——— Quod mare Dauniæ
> Non decoloravere cædes ?
> Quæ caret ora cruore nostro ? Hor.

"In the actual treatment of disease also, we conceive the naval surgeon possesses some peculiar advantages; some relating to the patient's welfare, some to his own improvement. He sees the disease in its very commencement—a circumstance of great importance, both as tending to the discovery of the nature of the disease, as well as affording the best chance for curing it; for it is remarked that the progress of most diseases can only be stopped effectually in their earliest stages. In private practice the case is very different. During the cure the naval surgeon has the patient perfectly under his control. 'His proceedings being obstructed neither by the prejudices of ignorance, nor the weakness of affection,' he can employ and follow up the most vigorous and determined practice. And rarely, indeed, will he have to contend with the obstinacy of his patient, as frequently happens in private practice; for the simple and unpresuming seaman looks upon his medical attendant not merely as a surgeon, but, in implicitly following his directions, he not only thinks it reasonable to take the advice of the wiser man, but conceives it his duty to obey the commands of his officer. In domesticating with his patients, he can attend to every stage of the disease; is at hand on every emergency. Nor need he intrust the administration of medicines to an ignorant nurse; it is the duty of the assistant-surgeon to give them with his own hand. No food or drink can be given to the patient unknown to the surgeon. In short, as to what regards the treatment of the patient, the practice of the sick-bay is analogous to the practice of an hospital."

The Statistical Reports on the Health of the Army and Navy have put us in possession of an amount of valuable information on the influence of climate in producing disease and mortality among men in the prime of life, to which there is nothing similar in existence. But interesting and valuable as these documents are, they leave untouched the important subjects of symptomatology and therapeutics. The complex nature of the observations necessary for the elucidation of these two branches of science demands an amount of time and patient investigation which few medical men will be found able or willing to devote to it. We have great pleasure

in directing the attention of our readers to the volume before us, as a proof that these interesting subjects have not been wholly neglected, and as a model for those who may be inclined to labour in this department. It is the production of an officer of the Honorable East India Company's service, and reflects the highest credit on his talents, industry, and judgment, while it also affords evidence of the fact that the splendid field for medical observation offered by India is being cultivated by labourers well qualified for the task.

In May, 1829, Dr. Geddes joined the Madras European Regiment, as surgeon, and remained in charge of it till the same date in 1833. This is an infantry corps, in the service of the East India Company, and is composed entirely of Europeans; the vacancies caused by death, or other casualties, being filled up by recruits from the United Kingdom. Immediately on his appointment he endeavoured to make himself acquainted, through the regimental records, with the history of each individual belonging to it, and afterwards kept careful notes of their diseases; which have furnished the materials for the volume before us. The object of the author has been to "afford a practical view of all the diseases with which a body of European soldiers has been affected within a certain period, and under usual circumstances, in the climate of the East Indies; the probable cause of their sickness; the treatment employed therein, and its results."

The regiment had been actively engaged in the Burmese war, from which it returned to the Madras Presidency in 1826. Many old soldiers were then invalided, and their places, as well as those of the men who died on service, were filled up by recruits from England; so that, in July 1828, "of 503 individuals present with the regiment, only 155 had been in a warm climate beyond three years." Although our author did not join the corps till May 1829, he has, by means of the official returns, extended his observations back to 1st July 1828, and included a period of five complete years, as far as relates to the prevalence of disease; but in his analysis of the cases he has confined himself to the period of four years, during which he was actually in charge.

Till the middle of December 1829, the regiment was quartered in the fort of Masulipatam, on the shores of the Bay of Bengal, situated about a mile and a quarter from the sea, on the northern bank of a salt-water creek. It is surrounded by a saline swamp, varying in breadth from 2000 to 3000 yards; beyond which an alluvial plain extends to a distance of about 40 miles. The regiment marched on the 15th of December to Kamptee, a distance of 520 miles, and arrived there on the 5th March 1830; having moved, on an average, about seven miles daily.

Kamptee is situated in lat. 21° 16′ N., long. 79° 46′ E., nine miles north of the city of Nagpoor, on the right bank of the river Kanan, at a height of 50 to 60 feet above its low water level. It is nearly equidistant from the eastern and western shores of the peninsula of India, and about 900 feet above the level of the sea. The soil on which the cantonment is built is of a clayey nature, but that of the surrounding country is chiefly a black alluvial deposit, termed cotton ground. The river runs over a sandy bed with considerable rapidity, is generally fordable, and its waters free from mud. The barracks were composed of several one-storied tiled buildings, raised a few feet from the ground, and sufficiently lofty and well ventilated.

The seasons here are divided into the hot, rainy, and cold: the first extending from the beginning of March to the beginning of June, the second from that period usually till October, and the cold season commencing in November, and terminating in the beginning of March. The period, however, of the commencement of each, and its duration, varies greatly in different years; the occurrence of the rains is the most important. "When they are heavy, or long continued, they have the effect of lowering the temperature at the time; of lengthening the duration of the succeeding cold season; and of diminishing the severity of the hot weather. They are no less influential in their effect upon the prevalence of disease. The epidemic constitution of the following year may be said, in usual circumstances, to have its origin in the seasonable occurrence, or otherwise, of the rains." In consequence of the influence of the rainy season, Dr. Geddes has dated the commencement of his annual returns from the 1st July in each year.

During the five years, from 1st July 1828 to 1st July 1833, the admissions into hospital averaged 2609 per 1000 of the strength annually, and the deaths 49·5, or, including those from accidents, violence, &c., and which did not come under medical treatment, 54 per 1000. As is generally the case in tropical climates, the admissions varied considerably in different years, the lowest ratio having been 2108 in 1829-30, and the highest, 3081, in 1831-2. In like manner, the deaths ranged between 27·6 and 66·6 per 1000.

If we compare the relative salubrity of the two stations, it appears that the admissions at Masulipatam, during 17 months, were in the annual ratio of 2499, and the deaths of 59·4, per 1000; while at Kamptee, in 40 months, they averaged 2755, and 44·2 respectively; but if we deduct from the latter the deaths by cholera, which did not prevail epidemically at Masulipatam during the period included in the 17 months, the ratio of mortality would be reduced to 34·8 per 1000. The regiment was very healthy during the three months occupied by the march, only 230 cases of disease having occurred, and 8 deaths, 7 of which were from cholera. Diseases of the bowels were considerably more prevalent and fatal at Masulipatam than at Kamptee; while, at the latter, the admissions from fever were much more numerous, but with a greatly lower ratio of mortality.

Dr. Geddes has given, in a tabular form, minute details of the composition of the regiment, in regard to the age of the men, their native country previous occupations, period of arrival in India, and length of service there; and has traced the medical history of each man during the five years included in his observations. He has also given monthly temperature tables for that period. Into these details we cannot enter at length; but must content ourselves with noticing some of the leading conclusions he has drawn, referring our readers to the work itself for the data on which they are founded.

Of the men composing the regiment on the 1st July 1828, or who joined it between that date and August 1829, 287 were English, 363 Irish, 42 Scotch, and 2 foreigners. During the five years under review, the mortality of the English and Irish was equal, being 25 per cent., while that of the Scotch amounted to 33 per cent.; but their numbers were too

few to admit of any accurate deduction from this. Our author has classified the men according to their occupation previous to enlistment as follows: Labourers in the open air, 334; in house, 133; in constrained posture, 126; in heat, 42; simple confinement, 37; deleterious professions, 15; peculiar professions, 7. Among all these the mortality was nearly the same, from 20 to 24 per cent., except the labourers in a constrained position, or with exposure to heat, among whom it amounted to 32 per cent. in the five years.

From a careful consideration of the causes of admission into hospital of the young soldiers, Dr. Geddes comes to the conclusion that, in India, there is no acclimating fever.

"There is no disease of the nature of what has been named a seasoning one; which is usually understood to affect Europeans at some period shortly after their arrival in the warm climates of the West. It is true, indeed, that for the first few months of his residence in India, the young soldier is more frequently on the sick list than at an after period of his sojourn there. The diseases which are the occasion of this circumstance are, however, seldom of the nature of that which has received the name of a seasoning one; and may often be referred to causes connected with the drill necessary for the recruit, or with the change of habit and food consequent to his landing, after a long voyage, as a soldier, in a country so widely different from his own as that of India." (p. 78.)

After enumerating the diseases for which the men were first admitted into hospital after their arrival, he observes,

"Hence it appears that there is a greater proportion of local diseases, diarrhœa, and the class of peculiar complaints, among the earlier causes of admission to hospital than in future attacks of illness; that dysentery, indigestion, or syphilis are nearly equally numerous, either as first or after diseases; and that fever, hepatic disease, and rheumatism are more frequently met with after the patient has been once or oftener in hospital than among his earlier diseases." (p. 79.)

The annual ratio of admissions into hospital has been already stated to have been 2609 per 1000, which gives an amount of sickness a little above $2\frac{1}{2}$ admissions to each soldier annually; the average stay in hospital was $12\frac{1}{3}$ days, and the sick time, consequently, averaged 32 days to each man. The following table, which we have compiled from the work, shows how the admissions in each year were distributed, and has also an important bearing on the question of acclimatization, because it will be seen that, with the exception of 1832-3, which was remarkably healthy, the number of men who escaped disease entirely, diminished, while those who suffered under several attacks, increased in each succeeding year.

Year.	Of the men composing the regiment there were in hospital during each year					
	Not at all.	Once.	Twice.	Thrice.	Four to seven times.	Eight times and upwards.
1828-9	150	136	117	103	108	8
1829-30	145	130	121	94	109	2
1830-1	112	123	104	69	136	12
1831-2	83	92	95	88	148	20
1832-3	99	104	85	65	119	12

If, instead of each year separately, the whole period is taken, it appears that there were only four persons who had never been in hospital; 72

had been one to five times; 137 from six to ten; 107 from eleven to fifteen; 79 from sixteen to twenty; 31 from twenty-one to twenty-five; and 12 a greater number of times. "Of these, two were 44 and 45 times in hospital, and they (as well as in general the remainder of the 12) owed their frequent admissions upon the sick list to disorders the effect of their own imprudence, and chiefly of a trifling description." The average period between each admission of those who were oftener than once in hospital was 138 days, or about four months and a half.

A very important question, which has been frequently discussed in the army, is whether the age of recruits on their arrival in India exerts any influence on the mortality among them. The military authorities in this country being of opinion that very young soldiers were more amenable to fatal disease, ordered that no recruit for service in India should be enlisted under twenty years of age. Dr. Geddes gives the following as the result of his observations:

"In recruits, arriving before or at 20 years of age, the early deaths are comparatively few, apparently from the facility of change in the constitution at this period of life. The same is the case, although in a less degree, among those arriving in India about the ages of 24, 25, and 26 years; arising, it is believed, partly from the constitutions being somewhat more formed at this age than at 21, 22, or 23 years, while there is still some facility of change in it; and partly from there being, in general, greater steadiness of conduct in such individuals than among the younger recruits. A greater degree of mortality continued to show itself for a lengthened period among those coming out at the ages of 21, 22, and 23 years, than in the other two classes just mentioned; but after four years' residence in India, the proportion of deaths in those arriving at the ages from 24 to 26 inclusive, exceeded that of their earlier sojourn; while that of the youngest class became considerably less. The deaths among those men who arrived in India at a more advanced age than 26 are in greater proportion than among the younger soldiers. This is equally apparent at all periods of their residence in that country." (p. 83.)

We are by no means prepared to admit the perfect accuracy of this observation, because we believe the numbers to be too limited to counterbalance the influence of any accidental deranging circumstance; and we would require more extended evidence to convince us that the laws which regulate the increase of mortality with the advance of age in other countries are suspended in India. It, however, partially corroborates the observations which have been made of the comparative exemption from mortality of young soldiers, when not employed in the field, and militates against the recruiting regulation above noticed. The subject requires, and is deserving of further investigation.

Having thus briefly noticed some of the principal conclusions relative to the prevalence of disease and mortality generally, we shall now proceed to the consideration of our author's observations on particular classes of diseases. During the four years he was in charge of the regiment, 5517 admissions into hospital took place. The details of 4291 of these cases treated by himself were recorded by him at the bedsides of the patients, and have furnished the materials for the history of the different diseases.

I. Fevers.—1. *Intermittent and remittent.* From a careful study of this class of diseases in India, extending over a long series of years, during which he enjoyed peculiar opportunities of observing it, both among Europeans and natives, and in a great variety of circumstances, our author

adopted the opinion of the identity of these two forms of fever; "apparently the produce of the same cause, showing similar symptoms, and removed by the same means, there seems no difference between these two forms of disease but that of severity."—He includes both, therefore, under the title of Paroxysmal Fever. In the four years from May 1829 to May 1833, during which he was in charge of the regiment, 1406 cases of paroxysmal fever were admitted into hospital, whereof 1210 were treated by himself, and on these, of which he kept careful notes, are founded his observations. From a table showing the monthly admissions of these cases, it appears that they were most numerous in August, September, October, and November, three fifths of the whole having occurred during these four months. Their numbers increase after the rains have set in, and are found to diminish again with the occurrence of the cold season, towards the end of which they reach the minimum.

Our author has gone minutely into the inquiry as to the proportion relapses bore to the original attacks, and the months in which they occurred. The 1210 admissions were confined to 405 individuals, and were thus distributed—119 had one attack in the four years; 111 had two; 59, three; 33, four; 25, five; 16, six; 17, seven; 14, eight; 6, nine; 2, ten; 2, eleven; and one soldier had 12 attacks. He has also stated in a tabular form the number of days between each attack. This depends a good deal upon the period of the year, the interval being shorter during the unhealthy than the healthy season. Thus, "of 495 cases of relapse which took place in the last five months of the year, 293 occurred within two months of the former attack; while of 310 relapses which were admitted into hospital during the remainder of the year, there were only 84 instances in which a longer period had not elapsed from the previous attack. Relapses, therefore, like the original attack are most liable to occur in the rainy season; and it appears that at this time the most usual period between each return of the disease is from 15 to 30 days; while a large portion of cases also take place either in a less interval, or after a longer period—on to two months." One half of all the relapses occurred within the last-named period.

From the tables illustrating the influence of age on the liability to paroxysmal fever, it appears that there is greater immunity above the age of 25, than among the younger soldiers. "Of 193 persons, who in 1830 were under the age of twenty-five, 100 were affected in the first year of their sojourn at Kamptee, and only 38 escaped altogether; while of 276 individuals at and beyond the age of twenty-five, 99 were affected in their first season at this station, and 81 had no fever previous to the period of the author leaving the regiment." This corresponds with the results of observations (which are, however, very limited) on the influence of age on fevers in temperate climates.*

The English suffered less than the Irish both in the number of attacks, and in the liability to relapse. The Scotch suffered less than either, but as already stated, their numbers were too limited to warrant positive deductions. With reference to previous occupation, those men who had been accustomed to simple confinement suffered least; next to these, were

* Thomson's Statistical Inquiry; Edinburgh Med. and Surg. Journal, No. 136; Fenger, on the Health of the Dockyard Workmen at Copenhagen; Annales d'Hygiène Publique, vol. xxiv.

labourers in the open air; while the class whose labour had been accompanied by exposure to heat furnished the greatest proportion.

Dr. Geddes makes a most important remark in regard to the health of the men who suffered from fever, which, however, we think requires confirmation from more extended observation.

"With the exception of the attacks of fever, the general state of health in those subject to this disease does not appear to differ very materially from that of the persons who had no fever. The average number of admissions within the five years, from July 1828 to July 1833, was, in the former, $12\frac{1}{2}$, and, in the latter, $8\frac{1}{2}$ times; the difference nearly corresponding with the average number of attacks of fever; while the average stay in hospital each time of being there, was, in those subject to fever, 11, and, in the others, 10 days. Of the more important diseases with which each class was affected besides fever, a slight predominance of hepatic disease and abdominal inflammation is found in the febrile subjects; while dysentery and rheumatism rather prevailed in the other." (p. 112.)

After a general description of the commencement and progress of paroxysmal fever, Dr. Geddes proceeds to show by numerical statements the relative frequency of the most common symptoms. He has given a table exhibiting the number of cases having headache, vomiting, or shivering, distinguishing first from subsequent attacks, and of which the general results may be thus stated. Shivering occurred alone in 94 cases, headache in 215, vomiting in 14; shivering and headache in 442, shivering and vomiting in 52, headache and vomiting in 87, and the whole three symptoms in 262; in 42 cases none of them were present, and 2 are stated as doubtful. Thus there was headache in 1006, shivering in 850, and vomiting in 415. Headache thus appears to be the most invariable of these symptoms.* It may occur at any stage of the disease; in a few instances where the febrile tendency had been checked by remedies, severe headache supervened during the accustomed period of fever, while the other indications of this state were either entirely wanting, or existed in a slight or partial manner. Vomiting, when present, usually occurs during the cold stage, and is generally accompanied by an irregular state of the bowels.

The state of the pulse being a point of much importance in fever, our author has shown in a tabular form, the variations during the first 48 hours after admission, obtained by recording the number of the pulse at two periods of the day. In 862 cases, the pulse fell to, or below 72; "in 237, it had been observed in that interval at from 90 to 100; in 265, at from 104 to 120; and in 43, at from 124 to 156, which is the highest number upon the record. Again in 317 of these cases the pulse was not observed at the usual visiting hours to be so high as 90, although at some stages of the disease it had, very probably, been beyond this number." "In 348 cases, the pulse ranged above 72 during the whole 48 hours immediately succeeding the patient's admission to hospital. In 126 of those, it had been observed at 76 within this period; but ranging in 56 of such cases to 100; in 58, from 104 to 120; and in the remainder above this number. In 179 cases, the lowest range of the pulse was 80, 84, or

* This is also corroborated by another table, showing the number of leeches employed in the fever, and the sites to which they were applied, from which it appears they were used in 849 cases, and in 763 of these were applied to the temples.

88; and of these, in 152, the highest range was 100 or beyond it. In 31 cases, the pulse ranged from 92 to 96; and in the remaining 12 from 100 upwards to 140. The lowest of these numbers are of course seen in the intermission or remission of the disease."—The pulse usually increases in frequency on the accession of the cold stage, reaches its maximum in the hot stage, and becomes softer, more full, and slower as the sweating stage advances.

With regard to the state of the bowels on admission to hospital, it appears that in 650 cases they were regular, in 344 they were more or less costive, and in the remaining 216 were in various states of looseness, from "open" to "dysenteric and vitiated." "Connected with the state of the bowels in fever, it is to be remarked that a call to stool is often an immediate precursor of the cold stage of the disease."

Our author considers at some length the various other symptoms, as pains in the limbs and back, in the region of the spleen and liver, at the scrobiculus cordis, &c; vertigo, tinnitus aurium, and deafness; the urinary and cuticular secretions, but into these details we cannot enter.

In a table, at page 136, are arranged the hours of attack in the cases of each type of paroxysmal fever, and distinguishing between first and subsequent attacks. When there was more than one attack, the relapses were generally of the same type, and occurring at the same period of the day as the first. The following table shows the hours of accession in each type. We have subjoined the results of some observations on the same point made at Blidah, in Algeria, in 1842 by M. Finot, Physician in Chief of the military hospital there.*

	Quotidian	Tertian.	Quartan.	Double Tertian.	Total.	Military Hospital at Blidah.	
						Quotidian	Tertian.
Before 7 A.M.	14	70	0	46	130	260	173
From 7 to 8	9	60	0	28	97	74	71
8 to 9	25	65	0	34	124	125	71
9 to 10	38	95	0	57	190	155	72
10 to 11	34	81	1	44	160	191	118
11 to noon	41	52	1	27	121	120	63
Noon to 1 P.M.	52	25	1	5	83	209	75
1 to 2	39	21	1	3	64	64	32
2 to 3	34	12	5	4	55	112	47
3 to 4	33	8	0	2	43	73	33
4 to 5	22	8	1	0	31	65	23
After 5	71	9	3	5	88	404	132
Irregular or doubtful	9	9	0	6	24	0	0
	421	515	13	261	1210	1852	910

From this table it appears that the accession of tertian fever generally took place at an earlier period of the day than the quotidian. Thus 161 attacks of the latter occurred before, and 260 after noon; while of single and double tertians 659 took place in the former, and only 117 in the latter period. M. Finot's observations furnish the same result, although the difference is not so strongly marked. Of quotidians, 925 attacks

* Mém. de Médecine Militaire, vol. lvi.

came on before mid-day, and 927 after that hour; while the paroxysms of tertian fevers were relatively 568 and 342.

In further illustration of the phenomena of paroxysmal fevers, Dr. Geddes gives an elaborate table showing—1st, the duration of the illness of the patient previous to admission into hospital; 2d, the number of paroxysms reported to have occurred during that period; 3d, the number observed while in hospital; and 4th, his stay there; distinguishing also the different types of fever, and the influence of the seasons upon these events. The first part of the table corroborates what we have already stated to be one of the advantages possessed by military surgeons, that of seeing disease in its earliest stages. Of 1197 cases (the total admissions exclusive of quartans), 225 came under treatment on the day of attack, 224 after one day's illness, 241 after two, 302 after three days, and the remainder after longer periods. This is still more marked if we consider the number of paroxysms before admission. In 8 there were none; in 436 there had been one; in 459 two; in 182 three; and in 112 only had more than three occurred. A greater proportion of quotidians and double tertians was admitted within the first 24 hours, than of the single tertians; and the men generally reported themselves sick earlier in cases of relapse, and at the season when fevers were most prevalent and severe, than in first attacks and during the healthy period of the year. The table also affords evidence of the benefit arising from this opportunity of early treatment, 676 cases having had no paroxysm after admission; 415 only one; 77 having had two; 17, three; and only 12 more than that number.

The stay in hospital depended in a great degree on the number of paroxysms after admission, the patient being generally detained three or four days after the last occurred. 883, or nearly three fourths of the whole, were discharged from the third to the sixth day; 226 were seven and eight days in hospital; and 88 for a longer period, chiefly from the disease being attended with severe complications, or inducing debility from its violence. The seasons did not appear to exercise much influence on the duration of the stay in hospital.

On the diagnosis of paroxysmal fever our author observes—

"It is not, in general, attended with much difficulty, when the symptoms are accurately recorded at two periods of the day, and due attention is paid to the nature and history of any attendant local affection. There are three classes of disease, however, for which it is liable to be mistaken: one, where a febrile state is the predominating complaint; another, where any of the common topical accompaniments of paroxysmal fever exist, and are attended by pyrexia; and the third, where a fever is present, putting on the paroxysmal form, but having its origin in some organic visceral disorder. In the first class are comprehended those ephemeral attacks arising from excitement by heat, drinking, and the like, or from disorders of the chylopoietic organs, or exposure to cold and moisture; in the second are comprised certain cases of cephalic or splenitic inflammation, or rheumatism, and cholera—all of which, when paroxysmal fevers are numerous, may put on so much the appearance of these fevers as to be confounded with them; and in the third are included cases of hectic fever, arising from hepatic abscess, phthisis, or other extensive organic lesions, producing constitutional irritation. In distinguishing such disorders from remittent or intermittent fevers, the medical attendant will be chiefly guarded by the prominence or presence of peculiar symptoms, or their more or less continued nature or duration; and it may be here remarked that nothing will be found to assist him more in arriving at a correct knowledge of the nature of the disease

under treatment than accurate and minute records of the symptoms of the case, combined with the information which a series of such reports will afford respecting the previous history of the patient." (p. 149.)

Of the 1210 cases which have formed the subject of these observations, one only proved fatal. To illustrate, therefore, the manner in which paroxysmal fever terminates fatally, our author refers to the results of his experience on two different occasions when in charge of bodies of native troops. On the first of these, at Seringapatam, in 1823-4, of 1503 cases 23 died; and on the second, at Cuddapah, in 1826-7, of 955 cases 20 died. A tabular statement is given, showing the strength of the corps at Cuddapah, with details regarding the native countries of the men, the forms of fever, the average duration of the cases, the proportion of relapses, and the general plan of treatment. Into these particulars we shall not enter, but confine our remarks to the immediate cause of death in the fatal cases. In 14 out of the 20, repeated violent paroxysms of fever terminated in delirium or insensibility. In 8 this became superadded to the other symptoms in from two to five days after admission to hospital; in three, from six to nine days; and in three, after the ninth day from reporting themselves sick. "Among the first of these, death took place in three on the day following the occurrence of delirium or insensibility; in the same number the patient died on the second day afterwards; and in two, on the sixth and seventh days after. In the second class, the deaths were on the fifth and sixth days; and in the third, one took place respectively on the second, third, and fourth day after either of these symptoms had occurred. The cerebral affection was, for the most part, at first observed during an exacerbation of the disease." In three other cases death ensued from the violence of the febrile exacerbations, without the occurrence of stupor or delirium; in two, on the sixth day of the illness, and in the other, on the ninth. In another case, the patient, after two exacerbations, attended with slight delirium, and a tendency to vomiting and diarrhœa, died from "a sudden failure of the *vis vitæ* about the period of accession of the cold fit." In another case, dysentery supervened, and in the remaining one, an abscess formed in the site of an old injury of the chest, emptied itself through the bronchiæ, and the patient died of hectic fever. Of the 23 fatal cases at Seringapatam, 3 died in a state of delirium or stupor; 1 from a sloughing ulcer on the loins; 2 from the violence of the fever; 5 by the supervention of dysentery; 1 from beri-beri; and 10 from extreme debility, resulting from a chronic state of fever or of frequent relapses, with a tendency to œdema, or a loose state of the bowels. In one case the particulars are unknown.

We pass over our author's remarks on the causes of paroxysmal fever, and proceed to the consideration of the treatment. The indications which have directed the use of remedies in these cases were—"1st, to lessen the violence of the febrile paroxysm; 2d, to assist the tendency to an apyrexial state; 3d, to prevent the succeeding fit; and, 4th, to relieve local symptoms." In the cold stage increased covering, warm drinks, and a dose of opium, combined with calomel or antimonial powder, were employed; in the hot stage, bleeding, general or local according to symptoms, and antimonials, were the chief remedies; a dose of opium was also given with calomel, in cases where it had not been administered in the

cold stage. When the fever began to decline, a purgative was administered if considered necessary. When the intermission took place, the medicine which was relied on to fulfil the third indication, that of preventing the succeeding fit, was the sulphate of quinine. This our author looks upon as a most invaluable remedy in such fevers, and, accordingly, gave it in all the cases except seven. He has entered into a very full detail of the amount given and the results obtained; and "the conviction produced on his mind by his whole experience is, that wherever a certain quantity of quinine can be exhibited in that period of a fever in paroxysms wherein the disease has declined from its acme, the tendency to a succeeding exacerbation will become checked, and all the other phenomena of the disease will of themselves, or by the use of minor remedies, disappear." In support of this opinion he adduces strong numerical evidence, by showing the number of paroxysms in each case, before and after admission. In one instance these amounted to 30 before admission, but in none did more than four occur after; in very few more than one; and in above half of the cases not even one occurred.

Our author has also carefully examined into "the nature and extent of those evil consequences which have been occasionally referred to the use" of this medicine. After stating the cases in which any of the unpleasant symptoms arising from it occurred, and tracing the subsequent history of the individual who laboured under them, he concludes, "it is not believed that any morbid effect likely to ensue from the use of this medicine, either immediately or after a lengthened period, need prevent its being given to the extent required for the removal of paroxysmal fever; and it is certain that any evil consequences which could possibly occur from its use are far counterbalanced by its immediate effects in checking the tendency to a recurrence of paroxysms; which effect cannot be secured so readily by any other method of treatment."

Holding this decided opinion in favour of quinine, our author preferred it, in every case, to giving mercury to the extent of affecting the mouth, which, in the earlier stages of his practice in India, was the treatment recommended in remittent fever, under the idea that when the system came under its influence, the febrile phenomena would cease to recur. Having tried this method in 87 of the cases at Cuddapah, already noticed, he has given a tabular statement of the results, and compared them with those obtained in his subsequent practice. From this table it appears

"That, commencing from the fifth or sixth day before the mercury took effect, the fever ceased to recur in an increasing number of cases each day, until that preceding the one in which the mouth became affected, when 19 patients got rid of their fever; that on the day in which the influence of mercury was observed the paroxysms of 9 ceased to recur; and that thence the numbers declined until the fifth and sixth days afterwards, at which time the cessation of the febrile paroxysms took place in three instances. The number of those altogether in whom the disease stopped before the affection of the mouth by mercury, amounted to 48, and of those in whom this circumstance took place after such an event, to 28. From these facts there is reason to doubt whether the mouth becoming affected is not rather a consequence of the cessation of the fever than the latter a result of the system having come under the influence of mercury; but in some chronic cases, where the contrary appeared to occur, an increase of frequency of the pulse, and of feverish irritation in the remissions, have been observed to take place in a gradual

manner as the mercurial action showed itself, and this was considered to act by breaking in upon the habitual progress of the disease, which accordingly ceased to recur. In many instances, however, after a short interval of freedom from its attacks, these have returned before the affection of the mouth had entirely left the patient; and 37 of those who had been under the influence of mercury in the earlier months of the season had been seized with relapses before its expiration. From these circumstances, combined with a consideration of the occasional affection of the bowels, often amounting to a dysenteric state, produced by calomel, and of the lengthened sickness of the patient in consequence of his sore mouth, the reader will readily form an opinion of the relative value of mercury and quinine in putting a stop to that tendency to febrile exacerbations, which constitutes the main feature of the remittent and intermittent fevers of the East." (p. 189.)

The measures adopted to fulfil the fourth indication of treatment, that of relieving local symptoms, were, of course, various, depending upon the nature and severity of the complication. Great attention was paid to diet, both during the progress of the disease and in the stage of convalescence.

2. *Ephemeral and continued fevers.* These, in most instances, were obviously the result of the use, or rather the abuse, of intoxicating liquors. Occasionally they arose from exposure to the sun, or from errors in diet, and occurred chiefly among the young soldiers. The disease generally yielded to antiphlogistic treatment without much trouble.

Having thus placed before our readers some of the principal facts on the subject of paroxysmal fevers, we shall now proceed to examine briefly the observations on the other classes of diseases.

Diseases of the Head. In India these form a very important class of diseases, not as giving rise to a large amount of inefficiency from sickness, but on account of the very fatal character they assume, and their permanently disabling effects when death does not ensue. There is not much, however, in our author's observations demanding especial notice. The admissions from delirium tremens amounted to rather more than 7 per 1000 of the strength in the four years 1829 to 1833; which is a high proportion. This may, doubtless, be in some measure attributed to the pernicious system of the ration dram being still issued to the troops in India. A return of the quantity of arrack drawn on ration for the Madras European Regiment, from February 1830 to May 1833, inclusive, shows the quantity to have amounted to one twentieth of a gallon, or eight fluid ounces daily to each individual present with the regiment. The allowance of those in hospital, or of men disinclined to take it, was readily consumed by their comrades. In addition to this, there was a considerable quantity of spirits, arrack, wine, and beer sold in the canteen. "The quantity of intoxicating liquors thus used by the men would be considered great, if equally divided amongst them, and taken at divided intervals; but, in point of fact, while some soldiers observe great regularity in never exceeding their ration allowance, and a few restrict themselves to a less quantity, or even abstain from the use of arrack altogether for limited periods, generally under the sanctity of an oath, others, again, lose no opportunity of becoming intoxicated, and often retain themselves, more or less, in this condition for several days." With these facts before us, it is surprising that the disease did not prevail even to a greater extent; a result which

may, perhaps, have been influenced by the large proportion of young men in the regiment; for it is well established that the effects of intemperance are developed in a rapidly increasing ratio with the advance of years.

The only points in connexion with this disease to which we shall direct attention, are the period that elapsed between the cessation of the drinking and the occurrence of the delirium, and the duration of the delirium in the cases which ultimately recovered. In 18 cases our author had an opportunity of noting accurately the first of these circumstances with the following results: In one instance the interval appears to have been 18 hours; in two, 24 hours; in two, 36 hours; in seven, 48 hours; in two, 60 hours; in two, 72 hours; and in two, 96 hours. "The author is strongly impressed with the idea that some period of a similar nature will be found to occur in all cases where the peculiar delirium of this disease manifests itself; or that where intoxicating liquors have been used on till its supervention, they have been taken in less quantity than at first, or if to an equal extent, their habitual use has rendered them less efficient in producing excitement of the sensorium." On the second point it appears that the length of time from the manifestation of the first symptoms of derangement until its finally leaving the patient was, in six instances, 24 hours; in seven, from one to two days; in three, from two to three days; in two, four days; in one, five days; in two, seven days; in one, eight days; in two, 11 days; and in one, 12 days.

The next group of diseases discussed consists of cases of

Thoracic Inflammation, comprehending, under this term, only pleuritis and pneumonia. Of these the details of but 42 cases are available for analysis, whereof three proved fatal; one having terminated in hydrothorax of the right side of the chest, and the other two in empyema of the left side. The number of cases is too small to admit of legitimate deductions from them. We, therefore, pass on to the next subject—the important class of hepatic diseases. Dr. Geddes has reserved the consideration of the cases of catarrh and phthisis for another occasion, but we may remark that of the latter, during the five years, July 1828-33, only two cases, one of which died, appear in the returns, out of an aggregate strength of 2687 men.

Hepatic Inflammation. During the five years reported upon by our author 280 cases of hepatic disease occurred, of which 21 proved fatal, being in the ratio of 104 admissions, and 7·8 deaths per 1000 of the strength annually.

After making some corrections, which are specified, the number of cases available for analysis amounted to 268, occurring in 141 individuals. These have been arranged under four heads: 1st, cases of abscess ending fatally; 2d, cases of probable abscess; 3d, acute attacks terminating in resolution; 4th, mild attacks with a similar termination.

1. *Cases of hepatic abscess terminating fatally.* During the four years, May 1829-33, 21 of the soldiers died from this disease, who furnished, with their previous admissions, 63 cases. To these our author has added in his analysis 10 who died previous to that date, but of whose cases accurate records had been kept. He has given very complete tabular statements of the leading circumstances of each case, and the principal appearances on dissection. The existence of hepatic abscess seems, in most instances, to

have no connexion with the former illnesses of the patient. In 15 cases there had either been no admission previous to that indicating the formation of this disease, or their sickness had been very trifling, and quite distinct from it; in 9 the patients had been affected with dysentery; in 4, with rheumatism, 1 of whom had also splenitis; in 1, with jaundice; and 1 had, three years previously, been under treatment for catarrh.

"Upon the whole, with the exception of dysentery, and perhaps of rheumatism, it does not appear that abscess of the liver is often the consequence of any other disorder. Dysentery, too, is a very common symptom of hepatic abscess; and it is not improbable that in some of the cases registered thus, the hepatic affection may have already occurred, and proved in such cases a cause, and not a consequence, of the dysenteric disorder. The connexion, in short, between dysentery and hepatic abscess is such, that it is difficult in certain cases to say which is the original disease; and although due weight has been given to the attendant circumstances in forming a diagnosis, there has been a doubt—in arranging some of the dysenteric cases of patients where hepatic affection has afterwards become more decided—whether they should have been considered as idiopathic diseases or as symptomatic of the liver disorder." (p. 311.)

The nature and site of the abscesses in 29 of the fatal cases are thus briefly described:

"In 23 of the patients the disease consisted of one large abscess; in 3, of numerous small abscesses; in 2, of a combination of these, there being many small and one large abscess; and in 1 case there was one large abscess and another small one. The small abscesses have been found, generally, diffused through the whole substance of the liver; and when there has been a large abscess attending, this has been situated either in the middle or left lobe. In the case of two abscesses, the larger one was found in the right, and the smaller one in the left lobe. The greater proportion of the solitary abscesses was situated in the right lobe; there being twenty-one in this part of the liver, and only two in the left lobe. . . . . . Of those in the right lobe, 12 were seated in the upper part, near to the diaphragm, and in three of these the disease had communicated with the lungs, and been partly brought up by expectoration. In four the site of the abscess was more deep-seated, or in the posterior part of the liver. In one it was placed near its convex surface, and had been evacuated by an artificial opening in the right hypochondriac region; in another it was large and solitary, in the right lobe, but extending partly to the left; and in three the abscess was situated near the margin of the right lobe, which adhered in two of these cases to the colon. Of the solitary abscesses in the left lobe, one was situated near its concave surface, where it had burst into the cavity of the abdomen, and the other was placed in the upper part of the lobe." (p. 320.)

Our author divides the symptoms of abscesses of the liver into three classes: 1st, those more immediately resulting from the site of the disease, the chief of which is pain; 2d, those from sympathy with the affected organ, among which are dysentery, pain in the right shoulder, vomiting, and an altered or vitiated state of the bilious secretion; 3d, the effects of constitutional disturbance, generally in the form of hectic fever. From a a table, showing the relative predominance of these at the earliest notice of the disease, it appears that pain was the most marked in 13 cases, dysentery in 10, and pyrexia in 7; but the degree in which these are experienced varies greatly in the course of the disease.

The great variety in nature and degree of the symptoms of hepatic abscess renders the diagnosis extremely difficult and uncertain. Of 31

fatal cases, only 19 had been entered in the returns as hepatic disease when they were admitted; and of the 42 admissions of the same individuals previous to the fatal attack, only 5 had been so entered.

"The remainder presented symptoms assimilating them, in twenty instances, to fever, either paroxysmal or continued; in seven to dysentery or diarrhœa; in one to cholera; in two to thoracic, and one to abdominal inflammation; in one to catarrh; in two to rheumatism; and in two to indigestion. . . . . . From these facts it will be understood that the symptoms arising from the site of the disease are not always found in a very marked degree. In some instances, indeed, they have been entirely wanting, and in others are found varying greatly in form and intensity, as well as in the period of the disease at which they make their appearance, and their continued nature when they do occur." (p. 322.)

Our author gives an abstract of the chief peculiarities observed in these cases with regard to pain, which presented great diversity in its seat, character, duration, and permanent nature. From this he draws the conclusion that, "although frequently indicative of the locality of the disease, it has, in some instances, been wanting altogether, or during certain periods of the complaint; in others it has been felt at a distance from the situation of the abscess; and it has occasionally shifted its chief site during the progress of the disorder. This symptom has also varied greatly in degree, as well as in its extent, and this independently of the size of the abscess."

The most conspicuous of the second class of symptoms, those, namely, from sympathy with the affected organ, was dysentery; which prevailed to a greater or less extent in almost all the cases at some period of their progress.

"These attacks, although occasionally distinguished by their tendency to continue upon the patient, were more remarkable—like some other symptoms in the progress of an abscess in the liver—by their disposition to relapse, the irregularities in their severity, and the slight and apparently disproportionate means by which they occasionally became ameliorated." (p. 351.)

Another symptom in the same class is pain in the right shoulder. With three exceptions, this was present at some period of the disease, when the abscess was seated in the upper part of the right lobe of the liver; but in none of those when it was at the lower margin, or in the left lobe, or where there were numerous abscesses. This fully corroborates the remark of Annesley, "that pain at the top of the right shoulder is, when present, certainly characteristic of the disease in the right lobe."

Vomiting, another symptom of this class, was seldom present when there was dysentery, with ulceration of the bowels. It was impossible to trace, in any marked degree, its presence or absence to the site of the abscess. "The size of this, in some measure, seems to operate in producing this effect, as the tendency to vomiting has in general become more urgent as the disease advanced; but its presence otherwise would appear mainly to depend upon the constitutional predisposition of the patient."

The last symptom of this class to which we shall advert is an increase, diminution, or vitiation of the bilious secretion. The last appears the most frequent consequence of hepatic abscess, the stools being altered in consistence, and almost always of an unhealthy colour, varying from a blackish or muddy hue to a bright yellow or dark green. Occasionally

there has been a deficiency in the quantity of bile, while at other times it has been in excess. It does not appear that any of these peculiarities can be referred to the site of the abscess.

With regard to the constitutional symptoms, the fever, in most instances, assumes a paroxysmal character, although in a few it puts on more the appearance of inflammatory or continued fever. The pulse is generally recorded as thready or wiry, sometimes more or less sharp, irritable, or weak, and increasing in frequency as the case proceeds to a fatal termination. The tongue is generally furred; there is loss of appetite, with considerable thirst; and frequently want of sleep, or the sleep disturbed by unpleasant dreams. In one case only the abscess pointed externally, and was opened; in one, rupture took place internally, and the pus escaped into the cavity of the abdomen; and in three the abscess burst into the lungs.

Of cases of probable abscess of the liver. Under this head our author has included 34 individuals, who furnished 61 admissions from hepatic disease. From what has already been said of the difficulty of diagnosis, it is not surprising that we should entertain doubts whether abscesses really existed in some of these cases.

> "In the present section," says our author, "have been arranged those hepatic disorders which have exhibited similar symptoms [to those observed in the fatal cases comprised in the preceding section]; differing, however, from the former in the disease either becoming perfectly removed, or in its not proceeding to a fatal termination during the author's observation. Although varying in this respect, the similarity of symptoms, and in a few instances dissection, render it probable that they had owed their origin to the presence of the same cause; and that where the disease had disappeared entirely, this circumstance has been owing to the removal, in other words, to the absorption, or absence by some other means, of an abscess in the liver." (p. 365.)

Of the 34 persons furnishing the cases comprised under this head, seven died from other diseases, and the morbid appearances of the liver are stated in support of the accuracy of the classification; but we do not consider these to be satisfactory, at least in some of the cases. For instance, in one who died ten months subsequently to the hepatic attack, the appearances were, "in one spot, on the external surface of the right lobe, an indented mark, something like a scar, was observed, but not affecting the internal surface; the liver, generally, was somewhat smaller, paler, and softer than natural." In another, who died two years after being in hospital on this account, "the liver was irregularly indurated, of a pale colour; and on the outer surface of the right lobe there was a deep indentation, as of a scar, this penetrating the substance of the liver for some distance, in a tendinous structure." In another, "the liver was enlarged, and there was a spot on its convex surface resembling the cicatrix of an ulcer;" this man had survived the hepatic inflammation twelve months. In another, after nine months, "an adhesion was found of the liver to the ribs, with a white hardness on its surface, and the right lung had also formed adhesions to the pleura costalis." In a fifth, who died eighteen months afterwards of dysentery, "the state of disorganization was found to be such on dissection that it was difficult to say whether the adhesions of the colon to the concave surface of the liver, and otherwise, were owing to the recent disorder or to previous inflammation." In another case, sixteen months

afterwards "the surface of the liver presented, in the upper part of the right lobe, a scar of an ulcer without any adhesions there, and the substance immediately below it was hard and white. The same appearance was presented on the edge of the right lobe, where there was a slight adhesion to the colon, and the whole of the liver was somewhat paler and softer than what is natural." In one case, where the patient died of dysentery four months after his last discharge for hepatic disease, two small abscesses were found. We do not consider these appearances to justify the inference that in all, or even most of the cases, there had been abscesses. That they were the result of inflammation we admit, but we are inclined to believe it was inflammation terminating in effusion of lymph, and not in suppuration.

The symptoms were of the same general character in these as in the previous class of cases, but milder in degree. The bowels were not so generally deranged, and when disordered usually presented the symptoms of diarrhœa rather than dysentery.

Of acute hepatic inflammation ending in resolution. Under this head are comprised 81 admissions into hospital, occurring in 41 individuals. These cases were of a more acute nature, and less complicated and obscure in their symptoms than those already considered. The chief characteristic symptom was pain in the side, varying according to the portion of the liver affected, and the extent of the inflammation. In six of these patients an opportunity occurred of examining the state of the liver at subsequent periods, and in all were found unequivocal traces of inflammatory action.

The symptoms in these cases resembled those in which abscesses were found. The bowels, however, were not often affected, and vomiting and derangement of the biliary secretion were rarely met with. The constitutional affection was usually of a severe description. The duration of the stay in hospital was in 19 cases less than ten days; in 24, ten to fifteen; in 14, fifteen to twenty; in 8, twenty to twenty-five; in 3, twenty-five to thirty; in 7, thirty to forty; and in 6 cases, upwards of 40 days.

Mild cases of hepatic inflammation ending in resolution. These, amounting to 63 admissions among 45 persons, were of the same nature as the preceding, but milder in their symptoms, and shorter in their duration. There is nothing in the details of them which demands our particular consideration.

In treating of the diagnosis of these affections, our author states the diseases which may be mistaken for the more obscure form of hepatitis to be "chiefly affections of the head of the colon, either from a morbid irritability or disease in that situation, or from a collection of flatus or fæces there, with or without disease of the colon. The symptoms in such cases have been observed to become very similar to those of hepatic inflammation; the pain occasionally extending to the right hypochondrie, and being attended with a greater or less degree of uneasy sensation in the right shoulder. Such cases seem chiefly to be distinguished by the original and principal place of pain being observed by the effect of pressure there, the degree of constitutional disorder, and the speedy benefit derived from purgatives or local applications. Some forms of dyspepsia may also be mistaken for inflammation of the liver. Pain of the contiguous parts

produced by rheumatism, or by accident, or unusual exertion of the right arm, are likewise liable in India to be considered hepatic. In these and similar cases, or when the soldier feigns a pain in the region of the liver, the real nature of the complaint is most likely to be detected by a full consideration of the history and attendant symptoms, and by the effects of remedies."

In the treatment of hepatic inflammation the objects sought to be attained were—1st, to lessen the force of the circulation generally and locally; 2d, to excite another action more or less extensive; 3d, to remove peculiar symptoms; 4th, to restore the patient's strength after the disease had been subdued.

To fulfil the first indication venesection was employed, according to the urgency of the general disorder and the strength of the patient, followed by leeching, purgatives, and antimonial diaphoretics, and occasionally by poultices and fomentations to the part affected. For the second indication—the excitement of another action in the system generally—calomel, usually in combination with antimony or opium, was given as early as possible.

"It was believed, and this was confirmed by experience, that the specific action of mercury being excited in the constitution, the disease would give way to it, and the patient be left without any complaint but that produced by the effects of the medicine. In short, that simple inflammation and the action produced by mercury were incompatible in the same system; and hence that the sooner the latter took place, the more speedily the recovery of the patient would, in like manner, ensue." (p. 404.)

With the view of exciting a local action near the inflamed part, blisters were applied, and were found very useful in hastening the cure, and in saving depletion and consequent debility. The occasional symptoms demanding treatment were dysenteric or diarrhœal affection of the bowels, constipation, vomiting, and cough. With regard to these, the only peculiarity to be remarked was that the dysenteric affection occasionally produced by the use of calomel was almost invariably removed by a dose of castor oil, followed by an opiate. To accomplish the fourth object of treatment,—the restoration of the patient's strength,—nitrous acid, tonics, and, in protracted convalescence, wine, were given. When quinine was employed, its effects were closely watched, as it was supposed, where a tendency to inflammatory action remained, it might be renewed by a premature use of this remedy.

Such was the general line of treatment in the cases of inflammation, modified, of course, by the severity of the attack and the attendant complications, general bloodletting being less frequently necessary, and purgatives more trusted to, in proportion to the mildness of the disease. In cases of probable abscess, a local action was excited by means of blisters, which were kept open for some time, and counter-irritation by other means was also employed. Quinine was likewise given to check the disposition to fever, which, by producing debility, tended to diminish the powers of recovery. When suppuration was believed to have taken place, calomel was exhibited in smaller quantities than when the object was to remove inflammatory action. Our author believes the benefit in such cases to arise from the action of the mercury in exciting the absorbents to the

removal of the purulent matter, and has found more benefit from it where only slight soreness of the mouth was produced than where this became severely affected.

Throughout the course of the disease, it is hardly necessary to observe, great attention must be paid to the diet of the patient. In the early stages this must be as low as possible, and afterwards the transition to his usual mode of living must be very gradual. Our author notices the advantage, in chronic cases, to be derived from a sea voyage, or even from change of residence.

"Many officers of the Indian army are annually sent on ship-board, suffering under symptoms of this disease; and of these a large proportion, by a voyage to Europe, or by proceeding to the Cape of Good Hope, New South Wales, or elsewhere, and a sojourn in those climates for certain periods, are eventually enabled to return to their duties free from complaint. A land journey, performed by regular marches, has occasionally been attended with the same effects." (p. 415.)

The next subject discussed includes a series of cases classed under the head of

Abdominal Inflammation, comprehending "all the instances of simple inflammation of any of the contents of the abdomen, with the exception of those of the liver," and the majority of which consisted of cases of splenitis. There does not appear to be anything connected with them which demands particular notice. Only one fatal case is included in this section, arising from peritonitis produced by rupture of the stomach, and escape of its contents into the abdominal cavity. The man, having been previously in good health, was suddenly seized with pain of abdomen, attended with vomiting, thirst, restlessness, and general soreness. The pulse became frequent, small, and contracted, the face collapsed, the cheeks sunk, cold sweats supervened, and he died in seventeen hours from the commencement of the attack.

"On dissection, a greenish, muddy, watery liquid was found in the cavity of the abdomen, with a few particles of some oil that had been given him three hours previously, floating in it. The whole of the intestines were glued to the omentum by coagulable lymph, and all these parts showed much vascularity. The external surface of the stomach also presented the same appearances; and it was found, on separating it from the body, that a small hole, round, and with even edges, penetrated its substance near the pylorus, and thence the liquid had escaped into the abdomen. This opening had scarcely an ulcerated appearance; and the inner surface of the stomach, as well as the intestines, was otherwise in a healthy state. The man had been nearly six years in India, had not been in hospital during the previous six months, and in none of his former illnesses had exhibited any symptoms which could be connected with the cause of his death." (p. 435.)

Rheumatism. During the five years 215 per 1000 of the strength were admitted annually into hospital affected with this disease. The seasons do not seem to exert any marked influence on it.

"Generally, it is to be remarked that neither the occurrence of heat, rain, or cold appears to have any reference to the prevalence of this disease. The following is the order in which each month ranges in respect to this circumstance: viz. July, December, April, October, May, June, August, September, February, March, November, and January; presenting in the first six months, or those in which it was most prevalent, the same number [of months] belonging to each season as in the healthy half year." (p. 443.)

During the period our author was in charge of the regiment 457 cases were admitted, the details of 312 of which, occurring in 160 individuals, were recorded by him, and form the subject of his remarks. Our space will not permit us to do more than advert to one or two of the more striking points. With regard to the seat of the disease—

"On a general review of the whole, 47 individuals are discovered to have been subject, either in their primary or relapsed attacks, to rheumatism both in the muscles and ligaments; 43 had those structures affected along with that of the periosteum; 28 and 23 had respectively disorders of the ligaments, or of the muscles only; in five individuals the disease was confined to the periosteum; in eight this structure was attacked with that of the joints, and in three with that of the muscles only. In the remaining three, the disease was chiefly of a neuralgic character, attended in two with rheumatism of the ligaments and muscles." (p. 444.)

The different parts of the body were affected in the following order of frequency: "the shoulders, particularly where both are seized, the knees in like manner, the loins and back, the thighs, one elbow, one shin, the hips, both or singly, the arms, ankles, back of the neck, and one heel, or both heels."

Dr. Geddes goes into minute details relative to the presence of fever in these cases, and its character, the number of relapses, the duration of the cases, and the period between each attack, with the apparent influence of native country and previous occupation on their prevalence. Into these particulars we do not intend to enter, but shall proceed to the consideration of the predisposing causes. In 52 individuals, or nearly one third of the whole, furnishing 117 cases, the disease was believed to have a syphilitic origin.

"In 13 of these persons the venereal affection had been confined to ulcers on the penis; in a like number there had been both ulcers and bubo, but the latter had not advanced to suppuration; in 7 the bubo had proceeded to this termination. In 19 cases the disease had been confined to one or more buboes, without any ulcer having been observed or recorded during the patient's stay in hospital; and in 10 of such cases the glandular affection had gone on to suppuration. In 19 of these 52 patients no mercury had been exhibited during the existence of the venereal disorder; in 4 this medicine had been given, but there was no affection of the mouth; in 9 it had produced a slight soreness of the gums; in 8, a greater degree of this; and in 12 cases the salivation was considerable. It is scarcely to be observed that the total absence of mercury, or its various degrees of exhibition or impression upon the system, had any effect in bringing on the rheumatic pains at an early period, nor does the nature of the venereal symptoms seem to exercise any influence in producing such result." (p. 450.)

Although in these cases syphilis appeared to be the predisposing cause, our author does not consider rheumatism to be a very general consequence of the disease.

"Of 180 individuals who came under treatment for venereal, 109 suffering under the different varieties of the complaint above mentioned, and apparently presenting no decided peculiarity, either in symptoms or treatment, different from those having rheumatism afterwards, were quite free from any attack of this disease.......... The general results would appear to be that, from constitutional peculiarities, the syphilitic virus gives a tendency to the occurrence of rheumatic inflammation; and it is probable that this disposition is increased by the exhibition of mercury, when the effects of this medicine are not so severe as to induce the patient to avoid exposure during the period of its being given to him.......

The only peculiarity in these cases was a greater tendency to affections of the small ligaments, as those of the hands and feet, or of the os sacrum; and to disorder of single joints, as of one elbow or one knee, than in other cases; while rheumatism of muscular parts, or of the same joints in both sides of the body at one time, are less frequent." (p. 451.)

The other classes into which our author divides the cases of rheumatism are, first, those in which the rheumatic diathesis appears to have become excited by a previous state of indisposition—as fever, dysentery, hepatitis, and, in a less degree, diarrhœa, cholera, and scrofulous tumours—aided by the exhibition of mercury. This comprises 31 cases, occurring among 18 persons. 2d. Those referrible to previous disease, like the preceding, but in which no mercury had been exhibited; and in this class are included 20 admissions among 11 persons. In the other cases of this disease no predisposing cause could be traced.

"In all classes the frequent recurrence of the disease, or its lengthened duration upon any one occasion, renders the patient pale, emaciated, and cripple; and other disorders, in some individuals, become generated, which eventually eclipse the original disease. The most common and formidable of these are ulcers on the skin, and certain symptoms referred to hepatic disease. By either, or as is usual, by both of these combined, or succeeding each other, rheumatism has occasionally proved fatal." (p. 472.)

Dr. Geddes gives an abstract of seven cases which terminated thus, and in six others it became necessary to discharge the men as unfit for service. In no class of diseases are the benefits of change of climate more strongly remarked than in this, the health being greatly improved, and often entirely restored, by a short residence in England.

We must now bring to a conclusion our account of the volume before us, which, as already observed, is most creditable to the author. We have endeavoured to lay before our readers a general idea of the contents of the work, but have been obliged to pass over many interesting points discussed in it, because, from the adoption of numerical statements, Dr. Geddes has given his observations in a form that scarcely admits of condensation, and which want of space prevents our giving at length. To the medical officers in India, and especially to those about to proceed thither, this will be found a valuable book of reference, and well merits to be included in the list of works with which officers are required to provide themselves on joining the service.

The author states in his preface that, should his health permit, and the present volume be favorably received, it is his wish and intention to extend his researches over the remaining diseases of which he has records in his possession, comprising, among other subjects, the very important class of diseases of the bowels. We sincerely trust he may be enabled to fulfil his intentions, and to complete a work which cannot fail to be productive of benefit to those in whose welfare, from his long association with them, he must feel a warm interest; and which will supply "to the experienced practitioner the means of knowing the success of certain methods of treating the diseases of India, and to the student of medicine a full and minute description of those diseases which he is most likely to meet with in the exercise of his professional duties in the East."

Art. VII.

1. *Der Speichel in physiologischer, diagnostischer und therapeutischer Beziehung.* Eine mit Anmerkungen vermehrte Bearbeitung, nach S. Wright, m.d., &c.—*Wien*, 1844. 8vo, pp. 213.
2. *Het Speeksel uit een, physiologisch, diagnostisch, en therapeutisch oogpunt beschouwd,* door S. Wright, m.d. Naar de Hoogduitsche bearbeiding van Dr. S. Eckstein, vertaald en met aanteekeningen voorzien door F. Riienderhoff.—*Amersfoort,* 1846. 8vo, pp. 220.

The fact of a work being translated into two different languages, shortly after the publication of the original, suffices to show, at least, the interest attaching to the subject, and may be regarded as a fair indication of its importance, as well as of the satisfactory way in which the subject has been handled by the author. Admitting this, it may seem strange that such a work, coming out in our own country and in our own tongue, should have remained unnoticed by us until we received it in the shape into which it has been put by our German and Dutch brethren. The truth is, that Dr. Wright's treatise has never been published in this country in the form of a substantive book at all. It appeared originally in the 'Lancet' journal, as a series of papers; and it is from these that the translations now before us have been made.

As we doubt not that we shall have another opportunity of considering Dr. Wright's labours in a yet more perfect form, we shall, on the present occasion, confine ourselves to giving a mere analytical outline of the work, without any comments, critical or otherwise. And, in doing so, we shall—as well for our own case as for the sake of perfect accuracy—make use of the author's original diction, as given in the 'Lancet,' instead of retranslating the passages from the volumes on our table.

Dr. Wright commences by tracing the derivation of the term saliva. After quoting a variety of opinions upon it he concludes that, "the word is probably derived both from the Greek and Latin languages—the one being expressive of the manner in which the fluid is secreted and discharged; the other bearing an allusion to its saline constituents. Hence the Irish term *silim*, which signifies to drop or to distil; and the Welsh *haliw*, from *hâl*, salt." The author then passes on to consider the chemical history, properties, and composition of the saliva in its healthy state. Having quoted the authorities preceding him, he details his own process for the analysis of saliva. To obtain this fluid unmixed with buccal mucus is a matter of the greatest consequence, and not always easy of accomplishment.

"By moving the lower jaw and the tongue, as in the ordinary way of procuring saliva, this fluid necessarily becomes united with some mucus from the lining membrane of the mouth. For accurate investigation, it is necessary to obviate this intermixture, and it is best effected, according to my own observation, in the following way. The mouth should first be washed with cold water, and then, having depressed and fixed the lower jaw, the fauces are to be tickled with a feather, sufficiently to excite nausea, without the risk of vomiting. The saliva will sometimes run from the mouth in a stream, and generally it may be obtained in sufficient quantity for examination, and free from extraneous matter. A little

cayenne pepper, or any similar irritant, applied to the sublingual gland, will excite an abundant secretion; but, as I shall hereafter show, the saliva obtained by these means contains more than its natural proportion of free alkali."

Dr. Wright's analytical process has been repeated by Budge and Lehmann, and for the most part confirmed by them. (See Mr. Paget's Report on the Progress of Anatomy and Physiology for 1842-3. Brit. and For. Med. Rev. January 1844. p. 258, note.) He states the composition of healthy saliva to be, as follows—

Water	988·1
Ptyalin	1·8
Fatty acid	·5
Chlorides of sodium and potassium	1·4
Albumen, with soda	·9
Albuminate of soda	·8
Phosphate of lime	·6
Lactates of potass and soda	·7
Sulphocyanide of potassium	·9
Soda	·5
Mucus, with ptyalin	2·6

Whether there be such a principle as ptyalin, and what it is, are questions about which chemists have long been puzzling themselves. There can be no doubt that what Berzelius called ptyalin, was neither more nor less than albumen. (Dr. Golding Bird, Lond. Med. Gazette 1840-41. No. 43, pp. 643-4.) It is more than probable, also, from the manner in which Simon, Mialhe, and others separate their ptyaline, that it is either albumen or some other proteine-compound. Dr. Wright's ptyalin would appear to be different from that of other chemists, and is obtained in a totally different way, as we learn from the following detail.

"First, pass saliva through ordinary filtering paper, and, after filtration shall have been completed, secondly, exhaust the residue with sulphuric ether; the ethereal solution contains a fatty acid and ptyalin. It is to be allowed to evaporate spontaneously; and thirdly, the residue left by evaporation is to be placed upon a filter and acted upon by distilled water, which dissolves the ptyalin and leaves the fatty acid. If the aqueous solution be evaporated to near dryness, the "salivary matter" will be obtained in a pure state. Ptyalin, as thus prepared, is a yellowish-white, adhesive, and nearly solid matter; neither acid nor alkaline, readily soluble in ether, alcohol, and essential oils, and more sparingly soluble in water. It alone possesses the characteristic odour of saliva; it is unaffected by galvanism, and by most of the reagents which coagulate albumen; it is abundantly precipitated by sub-acetate of lead and by nitrate of silver; feebly so by acetate and nitrate of lead and tincture of galls; uninfluenced by bichloride of mercury and strong acids: the latter considerably heighten its proper odour and impair its solubility, whilst alkalies render it more soluble and give it the smell of mucus. Moderate heat, and oxygen gas also increase its odour; but intense heat or cold diminishes or entirely destroys it. At a suitable temperature, ptyalin may be preserved for any length of time without risk of decomposition. The salivary fluid from which ptyalin has been removed by filtration possesses a sickly mucous smell, decomposes much sooner than ordinary saliva, and in the process of decay invariably evolves ammonia. If this fluid be heated, the mucous smell will be increased until the evaporation shall have been continued nearly to dryness; when a slight salivary odour may be recognized, due to a portion of ptyalin being liberated from the mucus with which it was previously in combination."

The ptyalin thus obtained, and thus described, by Dr. Wright, bears no resemblance to the proteine-compounds of other chemists: its possessing

the characteristic odour of saliva, an odour which no other solid or fluid has in common with this secretion, strengthens greatly its claim to be considered as valid: still the question is yet *sub judice,* and we hope that the investigations to decide it are not likely to be much longer delayed.

The physiological and medical history of saliva are next treated of. After quoting divers authorities from Theophrastus downwards, in illustration of the superstitious and other applications of saliva, Dr. Wright enumerates a variety of experiments of his own, in proof of the physiological activity of this secretion. Of fourteen experiments, we select the two following as types of the whole. The quotation is long; but the great importance of the subject justifies it; and we are sure our readers will not think it tedious.

"Four drachms of slightly alkaline saliva, sp. gr. 1·010, were injected into the right external jugular of an old mongrel dog. Immediately after the fluid had passed, the animal uttered a loud yell and struggled violently; the heart palpitated with vehemence, and respiration became very hurried and irregular. When six minutes had elapsed, and the severe effects had subsided, other four drachms of saliva were injected. The heart's action again became so quickened that I was unable to number its beats; the pupil was contracted; the abdominal muscles underwent a strong spasm; and there was slight convulsion of the whole frame. At the expiration of ten minutes, the injection was repeated; it had the effect of increasing, but not remarkably, the action of the heart and lungs; the spasm of the abdominal muscles returned, and a quantity of bile and frothy mucus was ejected by vomiting. When thirteen minutes and a half had elapsed, an abundance of turbid urine, and fæces mixed with blood, were passed: severe tenesmus succeeded, accompanied also by slight priapism. At the expiration of twenty-five minutes, when the system was comparatively calm, the pupil a little dilated, but sensible to light, and the heart beating seventy-two strokes per minute, I injected the remaining four drachms of saliva into the vein. The symptoms which attended the first injection instantly recurred, but with increased violence, and continued, with trifling remissions, for nearly four minutes, after which time their severity subsided. At the end of forty minutes, there was slight convulsion of the whole frame; an offensive slimy dejection was passed, to all appearance involuntarily; and shortly afterwards, about half a pint of bloody urine escaped in a similar manner. When three hours had elapsed, the animal seemed to be tolerably calm and comfortable; he ate a little meat, and lapped milk and water: he was then left for the night.

It was observed on the following morning that he had made a great qnantity of water, and that he had been purged and vomited several times. He now looked drowsy and stupid; his eye was dull, watery, and injected; he was disinclined for sport and exercise; he ate little, but drank abundantly; respiration natural; pulse 86.

In three or four days the animal recovered his usual hearty and lively habits, and little notice was taken of him until the morning of the fifteenth day succeeding the experiment, when he was observed to look drowsy and dejected; his eyes were peculiarly downcast and inflamed; he refused to stir when called, and when approached, he uttered a growl expressive of anxiety and anger; his nose was dry and warm; paws cold; respiration irregular and quick; pulse 94. He lapped water or milk, but refused solid food. He continued in this state through out the day, passing one very offensive, dark, slimy, stool, and voiding at several efforts a great quantity of turbid bilious urine.

On the following morning, the symptoms of the previous day were much aggravated; the dog growled and snapped at everything, living and lifeless, that approached him. My assistant in alarm ran away, and the other attendants being

terrified at the dog's madness, deserted me also, and I was consequently left to manage him alone. By unobservedly seizing him at the back of the neck with one hand, and grasping him tightly, I was enabled to raise his lips with the other, when I observed that his mouth was filled with foam, and that his gums and cheeks were much swollen and inflamed; his nose was very dry and hot; paws cold and tender; respiration interrupted with frequent sighing; pulse 106. So long as I held him in my grasp, he seemed to be quite docile and contented, but the moment I loosed my hold, he ran furiously at me, and but for a strong chain which secured him to the wall, I have no doubt I should have suffered from his bites. In a few hours, the froth began to distil from his mouth; he was tortured with thirst, and lapped water eagerly, and without dread; indeed he plunged his mouth and face into the cold liquid, as if to relieve the heat and inflammation which troubled him; with the same intention he licked the cold wall and floor, but he would not touch any warm body, nor would he lap tepid water. He was remarkably irritable and restless, and when not snapping at objects that approached him, was constantly turning about, or chewing the sand and straw that were near. He was seen in the afternoon of this day by Mr. Bowker, surgeon, and by several other scientific friends, all of whom were decidedly of opinion that he was the subject of madness. In the evening, his restlessness somewhat abated, and he lay moaning in a husky voice, occasionally altering the position of his head, as if anxious to sleep. Thirst and salivation were diminished; eye dull and glassy, pulse 64, nose dry, extremities cold. He continued in this state, with increasing weakness and somnolency, until about five o'clock on the following morning, when, after a few struggles and signs of suffocation, he died.

Sectio cadaveris, six hours after death. The limbs were remarkably rigid, but the blood was everywhere uncoagulated, and presented scarcely any distinction between arterial and venous. No ptyalin could be found in it. The right cavities of the heart were gorged with blood; the left auricle was also full, and the left ventricle empty. The lungs were moderately crepitous, and unusually vascular; the air-cells and bronchi contained an abundance of mucus. The lining membrane of the trachea was vascular, and the redness extended to the membrane of the mouth. The gullet was also unnaturally florid, but the stomach and intestines presented nothing unusual. The stomach contained a quantity of straw, sand, and coal. The other viscera were natural. The brain exhibited venous congestion upon its surface, and a little bloody serum at its base; but in other respects it was healthy."

The second case is of the same interesting character.

"Six drachms of neutral saliva, sp gr. 1·008, were introduced at three separate injections into the right common carotid of a mongrel dog. Each injection was followed by an extraordinary increase in the heart's action; hurried, irregular respiration, and general convulsion. The symptoms closely resembled those detailed in a similar experiment, especially in the inability of the animal to walk in a straight direction, and his consequent movement in a circle: the inclination was always towards the vessel which had received the injection. The stage of excitement lasted for five hours, during which time the animal passed an abundance of urine, vomited, and was purged several times. At the end of six hours, slight coma supervened; respiration was deep and stertorous; heart's action slow and laboured; sensibility very diminished.

On the following day there was considerable reaction, and the animal manifested strong signs of irritability and excitement. On the evening of this day, I was called from home, and was unavoidably absent for a week. On my return, I learnt that, on the second day succeeding the experiment, the dog became calm and docile, ate and drank very well, and appeared not to suffer either pain or inconvenience of any kind In this state he continued until the morning of the eighth day, when, on being visited with his breakfast, he flew at the servant, who

narrowly escaped him. The door of the place in which he was kept was divided horizontally in two, so that by shutting the bottom half, he could be conveniently watched over it. He was described to me as frothing considerably at the mouth, and appearing very fierce in the eyes, which were deeply reddened. He wandered about incessantly, chewing straw or sand, or lapping a little water; but he refused all kinds of solid food. He was shortly left to himself, when he began to gnaw the bottom of the door, which he finally demolished to an extent sufficient for his escape. On my return home in the evening of this day, I discovered him in the middle of a field contiguous to the house, surrounded by half a dozen men, who, with sticks, forks, and spades, were variously endeavouring to get him back into the stable. It was sufficiently ludicrous to see such an amount of human srength and ingenuity successfully combated by a single brute; but the men were in thorough trepidation from the manifest signs of madness which the dog exhibited. He snapped furiously at everything that approached him, and would occasionally pursue one of his opponents, until, tired by the effort, he was compelled to stop for breath. When I saw him he was staring wildly, and a quantity of frothy saliva was distilling from his mouth: the anterior part of his body was covered with this foam. I put a strong glove upon my right hand, and whilst the dog was engaged in snapping at a stick held before him, I caught him by the back of the neck. He struggled violently at first, and seemed to be choking, but finding resistance useless, he became perfectly quiet and composed. From an experience of the treachery of the animal in the eighth experiment, I did not venture upon loosing my grasp until having examined the state of the eyes and mouth; both which I found to be unusually vascular, and the pulsations of the heart were 140 per minute, when I had a collar with a strong chain fastened round the dog's neck, after which he was reconducted to his stable.

"He was visited again in an hour, but there was no observable decrease of irritability or restlessness; he snapped at anything that came in his way, and was incessantly changing his position. He lapped a little water, but the only solid matter he would chew were fragments of sand and coal. On the following morning he was much in the same state, but less inclined to bite: his mouth was still very frothy, and his eye deeply reddened; respiration rather stertorous; pulse 98. The irritability and restlessness increased towards evening; he would allow nothing to approach him without snapping at it; he was constantly engaged in licking the cold wall, or chewing straw, sand, or coal; or dragging himself upon his belly over the rough ground. On the morning of the tenth day he was somewhat improved; he ate a little meat, and did not snap unless suddenly roused; the salivation was less, and the eye appeared to be brighter; there was no stertor, and the pulse was 84.

From this time the signs of madness diminished, and the dog seemed to be improving in health and strength until a fortnight had elapsed, when there occurred a most offensive discharge from his nose and ears; it was greenish-yellow in colour, excessively fetid, acrid and corrosive, for it excoriated the parts over which it trickled; and finally caused the entire of the nose and one ear to slough away. In a few days the dog became quite blind and deaf, though he did not diminish in strength, and he ate very heartily of meat which was plentifully supplied him. He did not appear to suffer any pain, and was seemingly very quiet and contented. He continued in this state for more than three weeks longer, at the end of which time I was compelled to leave home: I learnt, however, that ulcers subsequently appeared in different parts of his body, and were succeeded by gangrene of the extremities, of which he died. The body was in a state of putrefaction before death. This animal was several times seen by my friends Drs. Hutchinson and Taylor, and Mr. Massey and Mr. Thompson, surgeons of Nottingham."

To determine whether the mere animal matter of the saliva had any share in producing the above phenomena Dr. Wright performed the following experiments.

"I injected a drachm of isinglass dissolved in two ounces of water, into the carotid artery; a little temporary excitement was the consequence, but the animal suffered no further inconvenience.

"I injected a drachm of pure mucus, diffused through an ounce and a half of water, into the jugular vein; the heart was a little quickened, and respiration was correspondently hurried, but the effects completely subsided in twenty minutes.

"I injected the entire white of an egg, diffused through two ounces of water, into the left common carotid of a dog. It produced considerable cerebral excitement, which was succeeded by drowsiness and feebleness of the limbs that continued for several hours; but the symptoms were very mild, and their duration inconsiderable."

The bearing of these experiments on the momentous question of the etiology of hydrophobia will occur to every reader; and we think the author uses a due discretion in speaking doubtfully on the subject.

"Concerning the production of canine madness by the injection of saliva into the blood, it is not now my intention to speak. It will, however, be sufficiently evident from the experiments already cited, that saliva is capable of exerting a very marked influence upon the brain and nervous system. The spasm, convulsion, and coma which were consequent upon the introduction of saliva into the arteries and veins, are conclusive proofs of its activity; whilst the absence of all such symptoms on the injection of the other animal fluids into the circulatory system demonstrates that, not to any physical or mechanical influence, but to a peculiar property inherent in itself, is saliva indebted for the manifestation of its physiological action."

Dr. Wright next proceeds to consider the digestive properties of saliva. He finds that its alkalinity varies, in the healthy subject, according as the stomach may be excited by stimulants, or by the presence of acidity—proportionately to these things is the alkalinity of the saliva increased.

"I tested the saliva of a dog, and found it moderately alkaline. I then injected into his stomach, through an elastic catheter, an ounce of French brandy diluted with an equal quantity of water. In three minutes, the alkalinity of the saliva was doubled, and at the end of six minutes its strength was four or five times greater than at first. At the expiration of half an hour it had returned to its original state of alkalinity. I repeated this experiment several times upon other animals, and always with the same results.

"I was led to make an extensive series of observations upon this subject, from noticing in my own case the effect of spitting after a full meal. In such instances I never failed to have an abundance of acidity, with much pain in my stomach, *and a correspondent alkalinity in my saliva.* By taking a dose of carbonate of soda, which was immediately succeeded by the rapid disengagement of carbonic acid gas, *my saliva would return to feeble alkalinity in a few minutes, and often in a few seconds.* These observations, at a sad cost of health and strength, I made upon myself nearly one hundred times. Frequent attacks of dyspepsia, to which I was once happily a stranger, painfully remind me of the injury I sustained in the course of my investigations. I once spat two hundred and fifty drachms of saliva in one week, and from the nature of my experiments I was often compelled to spit directly after dinner: in that seven days I lost eleven pounds in weight, and was much weakened and emaciated."

After a variety of experiments, tending to show the digestive action of saliva upon starch and other substances, Dr. Wright enumerates the two following, amongst several others, as more directly proving the important part saliva serves in the common function of digestion.

"Eight ounces of lean, raw beef, and eight ounces of bread, were beaten to a

pulp with ten ounces of water, and the whole was then injected through the tube of a stomach-pump into the stomach of a healthy terrier dog weighing 14lbs. The animal had fasted for twenty hours previously. Immediately after the injection the gullet was tied. The saliva was tested at this time, and was observed to be *feebly alkaline. In half an hour its strength of alkalinity was at least doubled;* the *quantity* of secretion was also much augmented. *The alkaline reaction of the saliva increased, till, at the end of three hours, it contained* 3·14 *per cent of alkali.* At this time the animal was destroyed by the introduction of air into his jugular vein. The stomach smelt excessively sour, and its mucous membrane was deeply reddened; the shade was at least three or four times deeper than that which is observable in ordinary digestion. The food, especially the fibres of the meat, seemed to be little altered, and except being incorporated with an abundance of mucus, presented almost the identical appearance of the original mixture.

"Eight ounces of lean, raw beef, and eight ounces of bread, were beaten to a pulp with ten ounces of alkaline saliva, and the mass was subsequently injected into the stomach of a healthy bull-terrier dog weighing 13lbs. He had taken no food for twenty hours previously. The gullet was tied as before. On testing the animal's saliva, it was found to be moderately alkaline. *At the end of half an hour, the secretion was scarcely altered either in character or quantity.* When two hours had elapsed, the mouth was very frothy, but the saliva was little changed. At the end of three hours the mouth was still frothy, and *the saliva contained only* ·89 *per cent. of alkali.* The animal was now killed by the injection of air into his jugular vein. The contents of the stomach were not particularly acid, nor was the mucous membrane more vascular than in ordinary digestion. The food was reduced to a perfectly homogeneous pulp, and not a trace of fibre could be found in it."

The following are the "Inferences" deduced by Dr. Wright, from his experiments concerning the digestive properties of saliva.

"1. Saliva has the power of modifying, and to a certain extent, of digesting vegetable and animal substances.

2. It has a more powerful action upon vegetable than upon animal matters.

3. The healthy digestive action of saliva is always attended with the evolution of lactic acid.

4. Filtration, or boiling, diminishes the digestive powers of saliva, but does not destroy them.

5. Exposure of the saliva to atmospheric air, for a moderate length of time, does not materially weaken its digestive powers; but they are enfeebled in the ratio of the putrescency of the secretion.

6. Oxygen gas assists the digestive action of saliva, but is not essential to it. Carbonic acid gas impairs this action in a mild degree, and hydrogen and nitrogen gases weaken it very considerably.

7. Acids or alkalies added to saliva, diminish or destroy its digestive properties.

8. The presence of saliva in the stomach is essential to healthy digestion.

9. The digestive action of saliva is not possessed in any efficient degree by animal mucus, acids, alkalies, or alkaline salts."

The services performed by saliva in the animal economy he divides into active and passive. The former are—1st, to stimulate the stomach and excite it to activity by contact; 2d, to aid the digestion of food by a specific action upon the food itself; 3d, to neutralize any undue acidity in the stomach, by supplying a proportionate alkali. The latter, 1st, to assist the sense of taste; 2d, to favour the expression of the voice; 3d, to clear the mucous membrane of the mouth, and moderate thirst.

The pathology of saliva concludes the work, and constitutes, indeed, the greater part of it. Dr. Wright divides the pathological varieties of saliva into the following: "Deficient saliva; redundant saliva (α spontaneous,

β excited); fatty saliva; sweet saliva; albuminous saliva (α transparent, β white); bilious saliva; bloody saliva; acid saliva; alkaline saliva (α fixed alkali, β ammoniacal); calcareous saliva; saline saliva; puriform saliva; fetid saliva; acrid saliva (α per se, β from foreign matters); coloured saliva; frothy saliva; urinary saliva; gelatinous saliva." Each of these is treated at length, as regards its physical and chemical characters, its pathognomonic relatives, and its bearings upon diagnosis. Depraved states of the saliva, Dr. Wright thinks are often dependent upon idiopathic disordered function of the salivary glands, and that functional error of the gastro-intestinal apparatus occurs from the ingestion of this depraved fluid. In these cases, and in others in which the saliva is deficient, Dr. Wright lays great stress on the use of stimulant gargarisms, and epispastic or irritant applications in the neighbourhood of the salivary glands. In excessive secretion of the salivary fluid, he advises sedative and astringent gargarisms.

Without departing from our avowed intention of refraining at present from any formal criticism of Dr. Wright's production, we cannot be so ungracious as to dismiss it from our notice without one word expressive of our opinion of its value as a contribution to physiology. Even in the meagre outline of the treatise given above, the reader will discover its great interest and importance, and will receive it as conclusive evidence of the ingenuity, zeal, industry, and philosophical spirit of the author.

ART. VIII.

Anatomie de Texture, ou Histologie appliquée à la Physiologie et à la Pathologie. Par AD. BURGGRAEVE, Professeur à la Faculté de Médecine de l'Université de Gand, &c. Deuxième édition.—*Gand.* 1845.

Textural Anatomy, or Histology applied to Physiology and Pathology. By AD. BURGGRAEVE, Professor at Gand.—*Gand,* 1845. 8vo, pp. 700.

M. BURGGRAEVE'S work professes to be an exhibition of the present state of microscopic anatomy, and of its application to pathology. So wide a subject cannot be treated with great minuteness in a single volume, not closely printed, of seven hundred pages. The abundance of material in so small a compass must involve the necessity of a representation of *conclusions,* and the omission of the *grounds* from which they are derived. Hence, in the outset, an appearance of shallowness, from the state of the case: the conclusions of science are often simple, when the process by which they have been reached is most elaborate. A physiologist is under especial disadvantage in this respect; so many of his conclusions are mere facts, that, if he is confined to their enumeration, he will differ but little, externally, from those authors whose office it is to write for the million.

Invaluable as physiology is, simply as a science, one of its most important bearings is, obviously, on human pathology. The *symmetry of disease* is the fact which gives its value to textural anatomy, and the *exactness* of that symmetry constitutes the difficulty of its application. It is this symmetry which has recommended the study of the eye, physio-

logically and pathologically, as exhibiting lesions plainly visible, which must find their counterparts in other parts unseen.

Histology is a science, including at once fluids and solids, and, in its pathological application, realizes in M. Burggraeve's view the anticipation of the old physicians, and unites a rational humoralism with a modified solidism.*

M. Burggraeve carries us through the formation of the primary cell, its development into tissues, and *their* union into organs. There is little or no novelty in this, the introductory, part of his work, and so much doubt has of late been thrown on the nature of the evolution of cell-life, that our system, as at present recognized, may have to undergo an entire remodelling, and our part may still be merely to collect facts for some future and more successful theory.

Before proceeding to our selection from M. Burggraeve's work of such parts as may represent its character, a few words may be spent on the principle which professes to be the basis of the histological theory. It is the enunciation of Carus, that, granting the original development of every organized body from a primary type of formation, "every superior degree of evolution of an organized structure consists in the multiplication of this type, constantly repeated at different and gradually increasing powers." This is a mathematical statement of a physiological fact, and it is dangerous ground when we apply the terms of one science to another, particularly when the sciences are so dissimilar as the two here concerned.† Luckily, experiment is always at hand to correct the fallacies into which this axiom, standing by itself, might lead; such, for instance, as the doctrine, that in the grades of organized beings there is one uniform line of development from the lowest to the highest, a doctrine simply opposed to the fact that many animals in an inferior order are higher in organic development than the lower animals in one superior: such as, again, the notion that one species is capable of development into a higher species, in confutation of which our appeal lies to the analogy of nature, and which, as connected with the introductory part of M. Burggraeve's work, merits a few moments' consideration.

There is, indeed, no antecedent reason for supposing that a simple cell should be developed in one form more than another: the circumstances in which it is placed determine the direction of its development. It follows from this, that, in the lowest instances of organization in vegetables, the fungous germs, according to surrounding circumstances, may exhibit different genera in their perfect state. To say this, seems to be admitting a great deal; but all "circumstances" are not "separable accidents," and the *parentage of the cell* is a circumstance which *determines* the character

* The inclusion of *fluids* in histology renders the name somewhat inappropriate; but names get recognized before substitutes are found, and it is often no easy matter for an objector to propose a better substitute. Has the old technicality of Anaxagoras any recommendations? We are in no danger of applying it as *he* did: καὶ τὰς μὲν "ὁμοιομερείας" (ἀπεφήνατο) ὕλην, τὸ δὲ ποιοῦν αἴτιον τὸν νοῦν τὸν πάντα διαταξάμενον. Pseudo-Plutarch. de Placit. Phil., lib. i, p. 876, D. Vid. Lucret., i, 830. *Homœopathy* is too good a word to be wasted on empiricism; it might be used in pathology to express the *symmetry of disease*, while *homœomery* would express its analogue in physiology.

† Butler, Introduction of Analogy of Religion to the Constitution and Course of Nature. When will another Butler arise to apply the newly discovered facts of physiology to this, their highest end.

of its evolution. It is in those cases where fecundation is most obscure that this capacity of various development exists: in the highest organization it does not exist at all; and between the two extremes the capacity may vary inversely with the rank held by the parent in the organized world. From one plant a hybrid may be produced by applying the pollen of a different species to its ovarium; from one animal a hybrid may be produced by the fecundating influence of an individual of a different species, but in neither case can the stock be permanently continued. When we come to man, hybridity ceases to be possible: the ovum is potentially a *human* being from the circumstance of its parentage; it is capable of development, but only in one direction and from one source. In no case can a hybrid *species* be perpetuated: in the highest, the human organization, a hybrid *individual* is impossible. Pasiphae's offspring was not an idea derived from physiology, nor yet the marvel told us by Herodotus.* Indeed, our author does not use this axiom of Carus, and we may complain of an *affectation* of deep views, when a high-sounding apophthegm is propounded as the basement of a theory, without rhyme or reason.

We have now to accompany our author in the treatment of his subject. And in few words his scheme may be stated as follows: given the cell-germ, to trace its development into a cell; to trace the subsequent changes of the cell, in the production of blood, &c., of fibre, nerve, and vessel, as well as in the production of abnormal growths; to follow up these secondary formations into systems, whether cellular, nervous, vascular, parenchymatous, locomotive, tegumentary; to mark the functions of these systems and their lesions, the preservation of the one or the reparation of the other; and to define how art can lend its aid to the restorative energy of Nature. Such is the task undertaken by M. Burggraeve, and the *plan* is unquestionably good; it is very much what a system of histology ought to be. The manner in which M. Burggraeve has accomplished his task is of course another question, and our quotations from his work will supply the data for the judgment to be passed on its merits.

We will begin by letting our author speak for himself; we shall best represent him by almost literal extracts; and while this is our object, we shall neglect the correction or indication of errors, which will easily be detected by our readers. After this representation of M. Burggraeve's powers and opinions we will confine ourselves to such statements as may seem novel or worthy of consideration.

The cell, says our author, persists in its original state in the fluids, the blood-globules, chyle-globules, and lymph-globules; in the first, the red colouring matter fixes the oxygen of the inspired air. On the *surface* of solids, the cell exists modified by external causes in the epidermis; by both internal and external in the various forms of epithelium; in the *substance* of solids we have corpuscles of gray nervous matter, of fat (frequently with crystals of cholestearine interspersed), of pigment (which in their pathological development give rise to schirrous, fungous, and other tumours), of bone, of cartilage, and of glands, whether tubular (in various degrees of development) or aciniform. With respect to the glands, M. Burggraeve insists upon their centripetal development; they are not

* Herodot., vii, 57, ἵππος ἔτεκε λαγόν.

prolongations of mucous membrane in the first instance, but separate formations, and afterwards united with the mucous system. That such is the case with the nervous medullary fibre, is established by M. Longet at the beginning of his invaluable work, and M. Burggraeve asserts it as a law of formation without exception.

Fibres. The development of fibre is with M. Burggraeve that which is ordinarily given, viz. through the elongation and terminal fission of nucleated cells. It is known that Mr. Paget would regard it rather as produced "from a structureless or dimly granular substance, first marked and then broken up into fibres." However this may be, the cavity of the fibre is commonly filled with a deposit which may consist either of earthy salts or of proteiniform compounds; the former class will contain: 1. The yellow elastic fibre, which forms the middle or proper coat of the larger arteries, the yellow vertebral ligaments, &c. The ashes of this substance, when burnt, consist mainly of phosphate of lime; and hence the apparent ossification of arteries in old age, which results, in fact, from an increase in the quantity of this deposit. This fibre occurs in two forms: the one scroll-like and interlacing, the other reticulated, as in the yellow ligaments between the laminæ of the vertebræ. 2. The eburnated fibres of the teeth, which, says M. Burggraeve, differ from the former only in the diminished quantity of water and animal matter which they contain; *both* species are wavy in direction, and frequently bifurcate at their extremities. The latter class contains the cellular, sclerous or tendinous, and muscular fibres, as well as the dartoid, which, by containing fibrine no less than gelatine, form the transitional state from the first two to the last. We shall have occasion hereafter to speak again of the dartoid fibre, which occupies a prominent place in M. Burggraeve's Histology. His view of muscular fibre (in which he agrees with Dr. Mandl, p. 561) differs from that generally entertained in England; the primitive fasciculus of fibrillæ, he says, owes its transversely striated appearance to a spiral filament winding round it.

Nerves and vessels close the list of secondary formations; both are perforated, and perhaps both are lined with a ciliated epithelium (here we can hardly restrain our pen from adding a note of admiration); so far they are analogous, but they differ in their contents, which are, in the nerve, a secondary deposit in the form of a pulp; in the vessel a fluid in constant motion. The nervous pulp imparts its white colour to the containing cellulo-elastic sheath, which is composed of a double fibre, revolving in inverse spirals; (have M. Burggraeve and Dr. Martin Barry never met?)

Vessels in animals have their analogues in plants. It was Boerhaave who defined an animal to be a plant turned inside out (indeed, in the fœtal state, the placental nourishment brings into still closer apposition the animal and vegetable kingdoms), the spongioles of a plant are the same with the lymphatic system in the animal; both end in blind extremities, and by them absorb their nourishment. M. Burggraeve maintains that in a villus the lymphatics embrace the terminal artery and vein, and that, in consequence, these two last do *not* reach the surface of the mucous membrane. (Be it remembered that we are representing M. Burggraeve, and withholding any critical protestations.) The lymphatics are formed by the union of contiguous cells, blood-vessels by prolongations sent out

from the surfaces of primitive cells, and forming networks with greater or less interstices.

We may now, without fear of injustice to our author, cease to analyze, and adopt a more summary mode of proceeding with the rest of his work.

The nervous system. "Nerves," says M. Burggraeve, "bear the same relation to the solids of the body as the blood does to the fluids; they radiate like an atmosphere, the source of motion, sensation, and irritability." It is open to any one to propound a theory on the real constitution of the nervous system. The fact which is to be the keystone of the whole has not yet been discovered, or if it shall eventually prove to be a common one, like the fall of an apple, physiology has not yet had its Newton. M. Burggraeve has his theory; with him there are medullary fibres which form closed circles in the brain and spinal cord, and of them, the central part, the cerebro-spinal axis, consists: the peripheral part is represented by nervous cords, again forming continuous circuits, dipping centripetally into the gray matter of the cerebro-spinal mass. This hypothesis seems to our author more likely than any other to throw light on the nervous apparatus: it is not very apparent how it will tell so advantageously.

It is the opinion of M. Burggraeve (as it was of many old philosophers*) that sensation takes its birth properly in the tissues of the extremities. Here are found gray globules in the meshes of the nervous plexus and in the track of nerves of sensation; and sensation is conveyed by the nervous cords to the nervous centres for its development and its regular diffusion throughout the economy.

"These centres are to the dissemination of the nervous principle what the heart is to the distribution of the blood. The heart is the regulator not the principle of the circulation; the principle must be sought for in the capillary network of the extremities; even so, the organs are far from being passive media, traversed merely by the nervous currents; each of them is gifted with a certain power and spontaneity, whereby the emission of the nervous principle takes place constantly, in proportion to its needs. . . . . . . The terminal loops or plexuses correspond to the capillary network of blood-vessels. In organs of small extent, but requiring a large supply of nerves, as on the surface of the tongue, the papillary extremities of the nerves are formed into convoluted knots, presenting the greatest possible number of points of contact in the smallest possible space, representing in miniature the convolutions of the brain."

"Nowhere," says M. Burggraeve, "are the nervous cords continuous with the fibres of the central mass; the direction of the one class is for the most part contrary to that of the other." M. Burggraeve regards the central termination as *always* in loops, though he admits the opinion to be merely hypothetical. The researches of Kölliker have led to a modification of this theory. (See Paget's Report for 1844-5, p. 46.)

M. Burggraeve enters on the question whether the nerves of general sensation can supply the place of those of special sense. The question has been

* It *may* be, that they would have been of the contrary opinion had their acquaintance with anatomy been greater. For an instance, by way of illustrating our assertion, see Plotin., Ennead. iv, 4, c. 23, τὸ δὲ ὄργανον δεῖ ἢ πᾶν τὸ σῶμα ἢ μέρος τι πρὸς ἔργον τι ἀφωρισμένον εἶναι, οἷον ἐπὶ ἁφῆς καὶ ὄψεως.

handled by M. Longet* with his usual skill; and by him the question is decided in the negative. We do not think that M. Longet has received the attention from M. Burggraeve which he deserves; his name is *once* mentioned, just enough to show that his labours were not unknown (as indeed, how could they be?) to our author. M. Burggraeve ascribes the speciality of sense not to the essential character of the nerve, but to the modifying circumstances in which it is placed; and he meets the argument adduced by M. Longet, and derived from the fact that the irritation of one nerve of special sense never gives rise to the sensations proper to another, by observing, that the special character of the sensation was determined, in the first instance, by the *apparatus* in which the nerve was disposed (as the optic nerve, "in the most perfect and exact of dioptrical instruments"); and, that the nerve *retains* its *own* faculty of special sensation, thus stamped on it by habituation, in the same way as, by means of those nerves whose fibres had been distributed to an amputated limb, the sensation of pain is referred to the limb which has been lost. In some cases, a nerve of special sense is replaced by one of general sensation; for, in some animals, "the optic nerve, absent or atrophied, is supplied by the naso-ciliary branch of the ophthalmic of Willis." M. Burggraeve leaves undetermined how far the insertion of the nerve in the encephalon may contribute to the nature of the sensation, but remarks, that "if the corpora quadrigemina are the organs of luminous perceptions, the impressions which give rise to these perceptions may reach that part of the brain by *another* route than that of the optic nerve, as, by the trigeminus or the nerves of the medulla oblongata." How far M. Burggraeve would extend this power of substitution he does not distinctly affirm, but he throws in a hint that certain conditions "of magnetic somnambulism present equally striking cases of these singular transpositions." Generalization is a seductive thing; we do not *deny* that general sensation and special sense may be ultimately alike, but the argument which would reduce the nerves of special sense to the category of general sensation, modified by the organ, e. g. by a dioptrical instrument, would at least require the presence of a similar instrument for the conversion of a nerve of general sensation into one of special sense.

The vascular system, which takes its character in the higher animals from the centralization of respiration consists of the heart, the arteries, the capillaries, veins, and lymphatics. Its development is centripetal, and the heart is last in order of formation. The *capillaries,* the terminations of the vascular system, are—1, in the form of a *network,* where the tissues are to be nourished; 2, of a *plexus,* where they serve for diverticula, as in the pia mater, or for a protection against the effects of stagnation, as in the erectile tissues; and (3) of *villi,* where absorption is carried on, as in the intestines of the placenta.

The predominance of *veins* and *lymphatics* varies inversely; absorption by the former is lateral, by the latter terminal. This inverse proportion is particularly evident in the small number of lymphatics in the brain and pituitary membrane. The predominance of veins or lymphatics determines the bilious or the lymphatic temperament in different persons, or

* Tom. ii, p. 184.

of the same person at different times. In excessive excitement of the venous system, the lymphatic absorption is arrested, and the activity of the venous is increased; hence the fearful effects of puerperal peritonitis, resulting from the intensity of venous absorption, and the remedial effect of venesection, by restoring the lymphatics to the exercise of their functions.

Experiments have proved to M. Burggraeve with respect to the *absorption* of gas, that the air found in the stomach of a horse, and analysed immediately after death, had lost 20 per cent. of oxygen and 15 per cent. of nitrogen, but that the carbonic acid had increased in the proportion of 32 per cent. There is, proportionally, more of the oxygen of the air introduced into the stomach absorbed, than of the air inspired by the lungs.

Lymphatic ganglia. In abscesses formed in these ganglia their structure is not lost; their vessels are spread over the walls of the cellular cyst and easily return, on the evacuation of the abscess, to their normal state.

Lymphatics. The external coat of the lymphatics is dartoid, and is found muscular in the horse; still further, lymphatic hearts are developed in the course of the lymphatics in the inferior vertebrata, (reptiles), and, we may add, in birds, from the researches of Professor Stannius.

The Pacchionian corpuscles are little clusters ascertained to consist of the densest possible network of blood-vessels and lymphatics; they are, in fact, as their discoverer imagined, lymphatic glands. A similar arrangement is demonstrated by M. Fohmann in the choroid plexus. That the Malpighian bodies in the spleen also belong to the lymphatic system is rendered probable from the relation which they bear in number and size to the lymphatic vessels (which vessels, moreover, can, in all appearance, be traced to them), and from the increase in their size during the period of excitement of the whole abdominal lymphatic system, namely, during digestion. M. Burggraeve entertains the same opinion of the Malpighian bodies in the cortical substance of the kidney;* "they are hollowed," says our author, "and give rise immediately to lymphatics."

Diverticula. M. Burggraeve has some observations, which we will gather from various parts of his work under this head, as worthy of consideration. 1. The *brain* is protected from the rupture of its delicate vessels, not only by the course of the vertebral and carotid arteries, and the unyielding texture of its sinuses, as well as by the minute caliber of its capillaries, which requires an elongation of the red corpuscles for their transit, but by the existence of *diverticula.* These are the choroid plexuses, which receive the "bloody lymph," till they can be carried off by the veins. 2. In the *eye,* the choroid coat with its vasa vorticosa surrounds the nervous substance, as the pia mater surrounds the brain, and, like the choroid plexuses of the latter, the *ciliary processes,* are a *diverticulum* by which the sudden alternations of an erectile tissue like the iris, are prevented from being hurtful. This curious analogy of the eye to the brain may be extended by observing that the veins of the iris pour their contents into a sinus before emptying themselves into the ophthalmic veins, while these again pour *their* contents into the cavernous sinus: and, moreover, the

* So does Professor Hyrtl. See Paget's Report, 1844-5, p. 40. Mr. Paget rejects the opinion.

capillaries of the retina fall far short in diameter (as in the brain) of the blood-corpuscles, which are consequently retarded and obliged to elongate themselves. 3. In efforts, the return of the blood by the internal jugular vein is impeded; here again the brain is protected from venous congestion (not only by the sinuses) but by the *diverticulum* which the blood finds in the *thyroid body;* and the same protection is supplied during the less violent but more continuous effects of speaking. The veins of the thyroid body have, as might be anticipated, no valves. 4. The *spleen* is to the abdominal viscera what the thyroid body is to the cerebral; engorgement of the portal system is prevented by this office of the spleen in man, as the embarrassment of circulation is obviated in diving animals by a dilatation of the vena cava inferior. The pain felt in the left hypochondrium in running (*à fortiori,* after a full meal) is due to an excessive demand on the spleen as a diverticulum. It was this *pain* which led the ancients to the use of medicaments with a view to the extirpation of the spleen in foot-racers, a proceeding by no means in accordance with physiology.* 5. The *great omentum* too serves as a *diverticulum* to receive the superfluous blood of the stomach, in the intervals of digestion. And, lastly, 6. The *liver* has for one of *its* functions, that of a *diverticulum;* it varies in size inversely with the spleen, the extirpation of the latter being followed by a considerable enlargement of the former, and a pain in the *right* hypochondrium may be produced analogous to the pain above mentioned in the left.

After the vascular system, M. Burggraeve proceeds to the *parenchymatous.* We have already anticipated some of his observations, as we have preferred collecting those together which bore on one point, to following the exact order of his work. He divides the parenchymata into—1, the sanguineous ganglia, viz. the spleen, the thyroid body, the thymus, the supra-renal capsules; 2, the lymphatic ganglia; 3, the branchial and bronchial apparatus, viz. the branchia of the fœtus, the placenta, the lungs, and the liver; 4, the glands proper, viz. for secretion, digestion, reproduction, lubrifaction.

Our convenience leads us to introduce here the observations made by M. Burggraeve in various parts of his work on the *dartoid tissue.* This seems to be the same with the "contractile fibro-cellular, or connective, tissue" of Henle and others. The dartoid fibres belong to the fibrous class of secondary formations, with proteiniform deposit, and are intermediate between cellular and muscular fibres, When irritated, they have a tendency to contract, as is seen in the cutis anserina and in the effect of cold upon the scrotum; if stretched, they return to their normal length on the removal of the physical extension.

As fibrous tissue with *earthy* deposit forms the proper and middle arterial coat, so the dartoid or fibrous tissue with proteiniform deposit forms the external, and by its toughness continues entire after the laceration of the proper coat (as by ligature). Dartoid tissue forms the external coat of the *veins,* and becomes muscular in the pericardiac portion of the pulmonary veins and in the venæ cavæ of large animals (as in the ox): the external coat of the lymphatics is dartoid also, but of greater strength;

* Vid. Plin., Hist. Nat., xi, 80; xxvi, 83.

the skin is in different parts extensible, elastic, or contractile, in proportion to the predominance of cellular, elastic, or dartoid tissue in the derma. In the parenchymata the dartoid tissue plays an important part. The spleen is invested by a dartoid coat, which, at the hilus, is prolonged into the organ, inclosing its vessels, and following them in their ramifications, and the framework is completed by laminæ and threads of the same tissue proceeding from the inner surface of the coat and uniting with those which accompany the vessels. The distensibility of the tissue admits of an increased flow of blood into the organ, and its contractility restores the organ to its natural size, when its office as a diverticulum is no longer required. The kidneys are supplied with the same bridles; and in the liver they proceed from Glisson's capsule* to the proper fibro-cellular coat of the organ. This arrangement provides in a striking way against any pressure on the portal circulation, for the canal in which the portal vein and hepatic artery run is dilated rather than otherwise, while the dartoid filaments, by their contraction, diminish the bulk of the liver. On the other hand, the hepatic vein is channeled out in the parenchyma of the organ, and the contraction of its parietes can, says M. Burggraeve, only promote the centripetal circulation.

M. Burggraeve, who has not a few new notions on subjects generally agreed to, has of course one upon the spleen. We have already quoted his account of it as a diverticulum. With respect to the splenic juice or matter (*boue de la rate*), we learn, says M. Burggraeve, that the spleen is developed in inverse ratio to the liver, and that this splenic matter varies directly with the quantity of blood-corpuscles.† It consists of the detritus of the blood-corpuscles, of some of these corpuscles themselves *diminished in size*, of much albumen, of a little fibrine, and of various salts. These are submitted to the absorbents for redintegration into blood, and by their separation also (which is not, however, a *necessary* process), the blood is prepared to undergo the action of the liver, and the due proportion of red globules in the blood is preserved.

M. Burggraeve insists on the resemblance in structure and function between the liver and the lungs: the terminal vessels are arranged in the former precisely as in the latter, except that the terminal cell of the hepatic duct is interposed between the capillaries of the liver instead of the terminal air-vesicle. It will be seen that he differs in this from Mr. Kiernan, and he attempts to account for (as he conceives) Mr. Kiernan's error. It will require strong evidence for us to relinquish the results of our countryman's sagacity. The different theories on the structure of the liver and the lungs are in singular contrast. From the analogy of birds, in which the terminations of the bronchi are not closed sacs, but form a labyrinthine network, opening on the pleural surface, into large air-receptacles, which lie in the intervals of the viscera, and even of the bones. M. Bourgery makes the extremities of the bronchi in man and in mammalia communicate

* M. Burggraeve wishes to call this capsule by the name of his countryman, Van de Waele, as having preceded Glisson in describing it. It may be so; and, in that case, of course Glisson has no right to rob Van de Waele of his credit. But we doubt whether any individual's name should be foisted into the nomenclature of science. Witness the absurdities of our botanical nomenclature.

† So Pliny says, Lienem non esse constat iis quæ careant sanguine, xi, 80; and Aristotle, affirmatively, *σπλῆνα δὲ τὰ πλεῖστα ἔχει ὅσαπερ καὶ αἷμα*, Hist. Anim., ii, 15.

in capillary canals. As M. Bourgery thus assimilates the pulmonary structure to the hepatic (as we regard the latter in England), so M. Burggraeve reduces the hepatic to the recognized view of the pulmonary.

From the parenchymatous structures, M. Burggraeve proceeds to the fluids, and we find nothing which calls for special notice in his observations on them. We pass on then to the tissues connected with motion, and here, too, we have little or nothing to remark. He divides them into passive, as bones, cartilages, &c., and the active, which he distinguishes into—1, the vibratile cilia; 2, the cavernous or hæmomyary; and 3, the muscular or neuromyary. M. Burggraeve is not successful in his nomenclature: "*αἷμα, sang,*" and "*μύειν, mouvoir,*" will hardly reconcile us to such hard substitutes as the above for "erectile" and "muscular." Can it be that our author thinks he has discovered the root of the word "muscle" itself, and that the resemblance in form, which is supposed to have given occasion to the name, does not satisfy him? Whether this be so or not, it is most certain that our author, when he comes to Greek, *μύων βαδίζει.**

The division of muscles into those of animal and those of organic life, does not satisfy M. Burggraeve, as being an *imperfect* division; and he complains equally of the vagueness of the terms voluntary, involuntary, and semi-voluntary: he suggests, therefore, the division into "muscles with levers," and "muscles without levers;" but how this division is preferable to the others does not appear, since, manifestly, the latter class will place in the same category the visceral, vermicular muscles, and the *Platysma myoides* and *Palmaris brevis.* The fact is, few things are effected in nature *per saltum,* and where we find vagueness (if so be) in things, we must make allowance for vagueness in words.

From M. Burggraeve's account of the tegumentary system, it is only necessary to refer to his observations on the mucous membrane of the tongue. It is well known that the papillæ of this organ have recently been examined by Dr. Todd and Mr. Bowman. M. Burggraeve differs from them and from M. Cruveilhier in his opinion concerning the *Circumvallatæ papillæ.*

"Frequently," he says, "the mucous membrane lining a follicle is elevated by subjacent glandular corpuscules. This gives the appearance of a papilla standing up from a depression, and surrounded by a raised margin; by this means the name of papillæ has been applied to bodies, wholly unentitled to the name, as in the case of the lenticular papillæ distinguished by M. Cruveilhier under the denomination of perforated or calyciform. It is plain that neither their structure nor their function justifies us in identifying them with nervous papillæ: they are glands."

We conclude our review as we began it: M. Burggraeve has essayed a wide task in a small compass: and his space has been still more curtailed by mere catalogues of the facts of topographical anatomy. Such are his enumerations of the muscles, aponeuroses, blood-vessels, and nerves. If in some parts our extracts have been few, our readers are at liberty to infer that, as it has not been our object to epitomise the work, but to abstract from it views which lay claim to novelty, the portions thus lightly touched on contain a rapid but faithful conspectus of the con-

* We may take occasion here, too, to protest against the unscholarlike *look* of Greek words written without their proper accents.

clusions of human physiology up to the time of the composition of the work. We add this qualification because, though we have not the first edition, we have evidence which leads us to suspect that it has not been so *improved* in the second edition, as to keep pace with the rapid evolution of physiological research. For instance, M. Burggraeve ascribes to Dumas an opinion which he is well known to have revoked: analyses, again, of certain fluids given by M. Burggraeve have undergone considerable alteration, and theories which he admits have been laid aside. It may be said, that the multitude of modifications of opinion which each year produces may well excuse an author's ignorance of some of them, and we admit this to the full. We even think that the very endeavours to throw into a consistent whole the facts which are daily brought to light in physiology is laudable, and the more so, because, when physiology, now in its infancy, shall be a few years older, we may have to look back on our present position from a distance far in advance, and books which are now text-books will have become obsolete:

Tam venerabile erit præcedere quatuor annis.

Art. IX.

Remarks on the Dysentery and Hepatitis of India. By E. A. Parkes, m.b., late Assistant Surgeon H. M. 84th Regiment.—*London*, 1846. 8vo, pp. 271.

Of the diseases incident to Europeans in tropical climates, there is perhaps none of more importance than dysentery, whether we consider the amount of mortality arising from it, or the permanently impaired health produced by alteration of structure in those who have laboured under it. In the army this is even more marked than in civil life, for the soldier, often necessarily exposed to the causes of disease, and frequently unwilling to submit to the necessary restraint when convalescent, suffers repeated relapses until the disease terminates either in death or in organic alterations of such a character as to render him permanently unfit for military service. Dysentery has consequently attracted much of the attention of the medical officers of the army, and many excellent works have been written upon it. There are still, however, many disputed points in regard to its pathology and treatment, especially in the frequent case of complication with other diseases; and much patient investigation and accurate observation will be requisite ere these can be settled on a satisfactory basis. The volume before us is a valuable contribution to this end, and is most creditable to the industry and talents of the author. It contains the result of his observations while serving in the 84th regiment in India, and professes less to be a systematic work on the diseases treated of, than an endeavour to elucidate various points connected with them about which differences of opinion prevail, and to draw attention to the composite nature of all chronic abdominal diseases. "Before long," the author remarks, "a different mode of describing the allied abdominal diseases will be necessitated by increasing knowledge. Then it will be found, that each disease, when fully formed, is but a developed and prominent part of a

more general but partially latent affection. I am fully prepared to say, that a chronic affection of an abdominal organ never remains simple."

DYSENTERY. *Pathology.* Our author agrees with those who maintain the inflammatory nature of dysentery, but considers it to be peculiar in this respect "that ulceration of the large intestines occurs with great rapidity, and, except in one rare form, a case never presents true dysenteric symptoms without ulceration being present."—This does not arise from the intensity of the inflammation, for in slight cases where the patient has died suddenly from some other cause, "the proofs of inflammation, apart from ulceration, are often only just visible on post-mortem examination;" and in severe cases where extensive ulceration has been found, it frequently happens there has been during life very little constitutional disturbance. Dr. Parkes attributes the rapidity with which ulceration occurs, "to the glands of the mucous membrane being particularly implicated in the inflammatory action."

We shall endeavour to give, as concisely as possible, the statements advanced by him in support of his views, referring our readers to the work itself for the corroborative evidence contained in the post-mortem examinations. "1. There exists on the inner coat of the large intestines, a set of solitary glands peculiar to that particular mucous membrane." These are very different from the common mucous follicles, are hardly visible when the mucous membrane is healthy, but are enlarged and very evident at the commencement of dysentery. They have been noticed by various writers, but their relation to the dysenteric ulceration has not been pointed out, and they have been described as pustules.

"I have considered them not to be large mucous crypts for the following reasons. They present the appearance of round opaque bodies, without apparent orifice, imbedded in the mucous membrane, and even apparently attached to the submucous cellular tissue. In the early stage of dysentery, their contents are white, yellowish, and apparently thickened and starchy. They are sometimes streaked or striated on the surface, and bear on the summit, in some cases, a small black point, which looks like an orifice closed up. This is not, however, general or even common. Under the microscope, the mucous membrane around them presents the usual appearance of honeycomb cells. In a dysenteric case which has lasted two or three days, they are still more obvious. A minute vascular ring surrounds them, and they become prominent and a little hardened to the touch. In distribution, these glands appear equally numerous in the sigmoid flexure as in the cæcum; and on this account, I am disposed to regard them as perhaps the excreting organs of the colon." (p. 4.)

"2. Inflammation and ulceration of these glands constitute the earliest morbid change in tropical dysentery, and the process from the small ulcered gland to the irregular spreading ulcer, may be traced in every stage. The first alteration in the glands is an enlargement of them and a change in their contents. The contained substance becomes thicker, and now resembles flour and water in appearance and consistence. In all probability, this condition occurs every day, and giving rise to slight diarrhœa, relieves itself, and the glands return to their normal condition. . . . . . If, instead of thus relieving themselves by secretion, the glands continue enlarged for some time without being acutely inflamed, that appearance is presented which has been incorrectly compared by Ballingall and others to a variolous eruption. I have seen this several times, and the resemblance is about as great as might have been anticipated from the loose nature of the statement. . . If a greater degree of inflammation be present, the vessels around the gland become enlarged and conspicuous, and form a ring or halo spreading a short

distance into the mucous membrane. This condition presents the earliest symptoms of dysentery, viz. slimy stools, increased in number without blood, causing perhaps slight griping and tenesmus when passed, and generally unattended by pain on pressure. Immediately after this, and in severe cases during the very first days, ulceration begins and is always denoted by slimy and gelatinous stools, streaked with blood, and attended by tormina, tenesmus, and pain on pressure, varying according to the *seat* of the disease and its *intensity*." (p. 6.)

Such are the views entertained by our author of the nature and seat of dysentery; and on these he founds his division of it, when uncomplicated, into four stages: 1. That of enlargement and commencing ulceration of solitary glands: this is the condition described in the preceding quotations. 2. Of complete and spreading ulceration. 3. Of cicatrization. 4. Of abortive cicatrization, commonly called chronic dysentery; a disease which is a resultant of continued subacute inflammation, and ulceration, combined with ineffectual efforts to produce the cicatrizing process.

"The second stage is characterized by the existence of ulcers, more or less numerous, of various shapes, sizes, and degrees of development, round, oblong, or irregular; if small and round, often with raised edges; if irregular, with flat and levelled edges. In the same case every form may be seen, from the commencing punctiform ulcer, to the complete large spreading ulcer with lymph on its surface in nodules or layers. This period is attended with various kinds of stools: first, these are slimy and gelatinous, becoming more and more bloody; then the stools become scanty, lymphy and shreddy, streaked with blood, or watery, muddy, and with sanious discharges. At a later period the stools become like the washings of meat, dark, and perhaps offensive. If the ulcers heal, the stools become generally, first, like lymph floating in an albuminous fluid; then yellow feculence, streaked with blood, is mixed with this, and then the stools recover gradually their healthy appearance." (p. 11.)

In describing the third stage, or that of cicatrization, Dr. Parkes combats the opinion, held by many writers, that ulceration exists only in the advanced stages of the disease, and refers to dissections of several cases, the subjects of which were cut off by coup de soleil at an early period of dysentery. The process of cicatrization of the ulcers is thus described by him:

"After a certain time, in dysentery, when the inflammation has diminished, lymph begins to be effused over the surface of the ulcer, and between the muscular fibres, if these form its floor. In an ulcer disposed to heal, the lymph is regularly diffused over the surface, forming a gelatinous-looking coating, which becomes gradually darker in colour, rises to a level with the edges of the ulcer and the surrounding membrane, and then slowly contracts, puckering to a greater or less extent the adjacent mucous membrane. After an uncertain length of time, varying from one to four months, the only marks by which it can be distinguished from normal mucous membrane are by its greater and darker vascularity, its greater smoothness and peculiar slightly glistening appearance, and by the slight contraction round it. In the majority of instances, however, the process is less regular than this; from some cause or other, greater quantities of lymph are deposited on some parts of the ulcer than on others, and hence results a granular or nodular appearance, which after a time disappears, and the false membrane becomes levelled and uniform. In some cases the lymph is deposited between the muscular fibres, apparently compressing these; the ulcer is then healed, that is to say, it will not spread, and no blood escapes from it. Afterwards on this compressed muscular floor lymph is slowly deposited." (p. 17.)

After recording a number of dissections (which appear to have been very carefully made) illustrative of the different stages of the disease, our author gives a table of the principal changes found in the other abdominal organs, in twenty-five cases which proved fatal in the garrison at Moulmein. That most frequently diseased was the liver, which in seven cases contained abscesses. These were found in the same number out of 39 cases treated by Dr. Innes, 84th regiment. Mr. Marshall states that they occurred in about the same proportion among the troops in Ceylon.

Hepatic abscess. Abscess of the liver has, by some writers, been considered to be always the primary affection, and the dysentery in these cases to be consecutive to, and arising from it. This, however, is held by some of the best authorities to be erroneous. Agreeing, in this respect, with Annesley, our author divides the cases of suppuration in the liver, into those in which the disease is primary, or antecedent to the dysentery, and those in which it is secondary, or consecutive to it; subdividing the latter into (*a*) declared, and (*b*) latent.

The connexion between hepatitis and dysentery, although generally recognized, has never been satisfactorily explained. In primary hepatitis it has been supposed by Annesley and others, that the dysentery is caused by the morbid state of the biliary secretion. Dr. Parkes dissents from this opinion, and hazards the conjecture that it is caused by the absence of secretion altogether. In support of this, he adduces the fact that in some cases "when hepatitis has terminated in partial suppuration, and bile is still secreted, although altered in appearance, there is no dysentery; whereas when, from extent or peculiar situation of abscess, no bile is secreted, dysentery appears to supervene."

Passing over the description of the symptoms of secondary hepatic abscess, with the illustrative cases and dissections, we come to the subject of diagnosis. This is universally admitted to be extremely difficult, from the obscurity of the symptoms, and their close resemblance to those of chronic dysentery, especially when that is complicated with functional disorder of the liver. We agree with our author that "it is only by a study of the phenomena, from day to day, that a correct diagnosis can be given;" but we do not think he has stated sufficiently clearly, or fully, the various points of difference which may assist in arriving at a correct conclusion. One point on which he differs from many writers deserves to be noticed:

"Cases in which pus has been absorbed and discharged with the urine have never been observed by me. I have seen thick, apparently purulent, deposits in the urine, and have heard them called 'decidedly purulent;' but these are mere collections of vesical mucus, of a particular kind; and exactly similar appearances are seen in pyelitis and catarrhal inflammation of the bladder, where there is no suspicion of pus being formed anywhere. These deposits are soluble with effervescence in acetic and nitric acids. No coagulation was ever observed from heat or nitric acid." (p. 98.)

This opinion corroborates that of Dr. Budd:

"It has been supposed by some medical men in India, that the pus in an abscess of the liver may be absorbed and eliminated *as pus* in the urine. But this notion is evidently erroneous. Pus-globules, from their large size, cannot directly enter the blood-vessels, or escape from them. The matter in the urine, supposed to be pus, was probably a deposit of phosphates. During the severe constitu-

tional disorder that attends purulent phlebitis, there is often a sediment of this kind in the urine, having, to the naked eye, much the appearance of pus, but under the microscope, showing instead of pus-globules, beautiful phosphatic crystals." (Diseases of the Liver, p. 89.)

In endeavouring to account for the production of secondary hepatic abscess, our author disputes the opinion that it arises from the absorption of pus from the intestinal ulcers, or that it is a result of venous inflammation, or of an immense secretion of vitiated bile, and advances the hypothesis that it is caused by "a passage with the blood of those substances which, under ordinary circumstances, are excreted by the colon."

"When I examined," he remarks, "cases of consecutive hepatic abscess, I observed that the dysentery was general, though perhaps not very far advanced or very acute. The ulcers were sometimes small, and had healed early, but they were numerous and distributed universally over the mucous membrane of the large intestines; or if not everywhere ulcerated, all the glands were very large and hard to the touch. In other cases of dysentery, without hepatic abscess, the ulcers were perhaps very much larger, gangrenous, and altogether the colon may have appeared more diseased, but still there were clear spaces of undiseased mucous membrane. I therefore at length came to the conclusion, that the type of dysentery generally associated with the consecutive abscess is one in which there is universality of affection, with or without a high degree of intensity of inflammation. . . . . Is it not an allowable hypothesis that the normal action of part of the mucous membrane will prevent abscess, by excreting some undetermined ingredient, which in the other case (where the affection of the membrane is universal) must be circulated with the blood, and then by its effect on the liver produces suppuration in that allied organ? I state this as an hypothesis, that is, as an imaginary arrangement of facts which is to be tested by experience. The facts are, the intimate connexion of dysentery and abscess, which is undoubted, and the universality of affection of the colonic solitary glands, in secondary hepatic abscess—a fact which requires further observation to confirm it." (p. 117.)

Adverting to the great rarity of hepatic abscess among the natives of India, which our author considers to afford very strong evidence against the existing theories, he observes:

"According to the view I have taken, the difference between Asiatics and Europeans may be attributed to the difference in food, and consequent difference in the composition of blood and excretions, and to the difference of the skin, which in the former nations excretes more oily and carbonaceous perspiration." (p. 119.)

Complications. Dysentery is occasionally complicated with scurvy or purpura, and proves a most formidable disease. The most important differences between this and the simple form are, that the "ileum participates in the disease, and is sometimes more affected than the large intestine; the cæcal and colonic solitary glands ulcerate in the usual way, but the lymph thrown out does not circumscribe the ulcers; perforations are common; the intermediate mucous membrane is darkly vascular, and often softened, and appears to effuse blood even when unulcerated." This form of the disease will not bear depletion or active purging, and a few grains of calomel will often produce severe ptyalism. Dr. Parkes has found creosote, in combination with opium, very useful in such cases, after the acute stage has passed.

Causes. The exciting causes of dysentery are divided by our author into four classes: 1. All acrid agents, whether produced by irritating in-

gesta or secretions. 2. Suppression of secretions rapidly accomplished. 3. Epidemic states of the atmosphere. 4. Alterations in the blood, effected by some peculiar and at present unknown changes in the process of assimilation (scurvy, purpura, &c.)

Treatment. The indications of treatment are: 1. To subdue the inflammation of the solitary glands, and of the ulcers, when these are formed, as is always the case after the first few days. This comprises: *a*, the removal of the causes; *b*, the removal of morbid secretions; *c*, the restoration of the functions of the, liver, skin, and kidneys. 2. To assist the healing of the ulcers, when the ulceration has been arrested. The measures recommended to fulfil the first indication are briefly summed up as follows:

"1. Very free depletion, to arrest the progress of an acute inflammation. 2. Oleaginous purgatives, with opium, to remove secretions. 3. Opium, to allay tormina, and diminish the excess of nervous sensibility, which is one link in the inflammatory process; the combination of blue pill and ipecacuan with the opium seems to increase its powers. 4. Occasional production of salivation." (p. 146.)

With regard to the treatment of dysentery by salivation, formerly the almost universal practice, Dr. Parkes, after stating his objections in full, thus sums up with, we think, a less forcible protest against the meddlesome mischief than he was entitled to make:

"Considering that recovery is certainly slower with this treatment than with the depletion and alterative plan, that, in India, a scorbutic taint or an adynamic habit of body often accompanies dysentery in European soldiers, and remembering the great difficulty of limiting the effects of mercury, when rapidly administered, to a moderate action, it must be confessed that the utility in dysentery of this very powerful remedy has been rated too highly by some of its supporters." (p. 144.)

In discussing the means to be employed "to assist the healing of the ulcers" in chronic dysentery, Dr. Parkes divides the cases into four classes:

"1. Immediately on ulceration being partly checked, the reparative process begins, and vast quantities of lymph are thrown out upon the ulcers and between the intestinal tunics. If to this we have superadded, from time to time, attempts at fresh ulceration, followed as before by effusion of lymph, we get a bad form of chronic dysentery, in which the intestine becomes immensely thickened, and, as a consequence, partially lessened in caliber in different parts of its course, and enormous masses of nodular or granular lymph are effused on the mucous membrane, and the ulcers, wholly or partially concealing these parts. 2. Or, subacute dysentery being unchecked and becoming chronic, we get a state of parts in which all the ulcers may be healed, and no fresh ulceration going on, but the coats being densely thickened, and the functions of the mucous membrane completely interrupted, we have a long continued and exceedingly intractable form of lienteric dysentery; in the latter stage of which, if the thickening be universal, hepatic abscess may probably supervene." (p. 149.)

The treatment in these two forms should consist in local depletion, with a strict farinaceous diet, followed by the cautious administration of mercury till a slight action on the gums is produced; counter-irritation; and afterwards, if necessary, the nitrate of silver, or the nitro-muriatic acid.

"3. Another form of chronic dysentery, and a very common one, is that in which the original attack has been almost chronic from the first, or at any rate

not severe, and in which the glands get hypertrophied and slightly ulcerated, and a small quantity of lymph thickens at intervals the coats of the large intestine."

In this form the stools, instead of being feculent, loose, lienteric, or occasionally serous, are lymphy, fatty, dark, viscid, or variegated; or these varieties alternate with the former.

"4. Another form of chronic dysentery is more passive, following colonitis or erythematous dysentery, and consisting of pale ulcers, with the muscular fibres for their floors, prevented from spreading by effusion of lymph, which is yet not effused in sufficient quantity to heal them." (p. 150.)

In these cases metallic astringents are to be given, as the sulphates of copper, zinc, and iron, followed up by tonics combined with alteratives. In all cases the diet should be carefully attended to, being as much as possible restricted to farinaceous food, while beer, wine, and other stimulants should be interdicted.

HEPATITIS. Having discussed the subject of dysentery and secondary hepatic abscess, Dr. Parkes proceeds to the consideration of hepatitis. This disease is one of great importance to the military surgeon in India, as giving rise to a large amount of ultimate inefficiency. Of 41* cases of true hepatitis, which our author has tabulated, 6 died, 15 were invalided, 3 relieved, but not cured, 12 cured, and 5 remained under treatment when the table was drawn up. Thus it appears that upwards of one half of those attacked were ultimately lost to the service by death or invaliding.

Causes. Passing over the symptoms of hepatitis, and the various forms it assumes, we give a portion from our author's most important observations on its causes. We feel well satisfied of their justice, and are convinced that a little attention by the authorities to the improvement of the diet of the soldier would, especially in tropical climates, be attended with most beneficial results. Referring to the most common form of liver disease in India, "gastro-duodenal hepatitis," Dr. Parkes remarks:

"The diet of European soldiers in India, varying necessarily at different places, is, as a general rule, far too rich and stimulating. Hot curries, carelessly made by native cooks, are used several times every week for dinner; and vegetables are in many places scarce or of indifferent quality. Soldiers often refer the origin of their complaint at once to their diet, and, to my own knowledge, many men have supplied the place of the curries by rations purchased out of their own scanty funds. It often happens that an European regiment quartered with one or two companies of English artillery will show a much greater per centage of sickness: the habits of both corps are the same with one exception; artillerymen, living in small bodies, are easily looked after by their officers, and they are generally more careful about their diet. Again, married men who are not in a mess are always more exempt from both dysentery and hepatitis than single. If this is not attributable to their food being better cooked, the circumstance is inexplicable. It is an extraordinary thing that out of 150 married men in the 84th regiment, only two died during a tropical service of 30 months, while in the same period, the mortality among the single men was above nine per cent. The two deaths referred to were from phthisis and from delirium tremens. Some influence may be given to the habits of married men being more regular than those of single men, but in a small station where little debauchery goes on, the influence cannot be great.

* The number of cases is stated to be 42, but there are only 41 given in the table.

"A supervision of the whole system of diet among European troops—not as regards commissariat supplies, which are generally excellent, but as respects the cooking of these, and the time of meals, the encouragement of teetotal societies by every allowable means, and the formation of day and night guards, differently clothed to prevent the effects of the great daily thermometrical range of some Indian stations—are measures which I am convinced would at once reduce the list of duodenal hepatitis, and would probably even diminish the number of cases of dysenteric, febrile, and primary hepatitis." (p. 227.)

After discussing at some length the various exciting causes which have been assigned by different writers on this subject, our author thus briefly sums them up:

"1. Hepatitis, in many cases, is clearly caused by dysentery and remittent fever. 2. The influence of bad or improper food, spirituous liquors, &c., in causing hepatitis is probably to be explained by their effect on the gastro-duodenal mucous membrane. 3. Heat alone has not been proved to be a cause of primary hepatitis. The hot stations in the Madras Presidency are the healthiest; for instance, Bellary and Trichinopoly. 4. But as a collateral agent, heat has a great effect; it increases secretions, as of the skin, or alters them, as of the kidneys. To these increases and alterations by themselves the system seems to accommodate itself, but not to rapid transitions in them. There appears ground for believing that secretions increased by great temperature, and then suddenly suppressed or lessened by the abrupt supervention of another atmospheric condition, really have an influence, unexplained but decided, upon the liver in particular......5. The doctrine of pulmonary and hepatic antagonism by which the liver is supposed to be called upon, in hot climates, to excrete carbon, which in cold countries the lungs give off, has not been proved, as far as my knowledge goes, and at present is merely an ingenious conjecture." (p. 234.)

We do not intend to follow our author in his remarks on the pathology and treatment of hepatitis, agreeing with him that the former is very imperfectly known, "and that many of the most important means of studying it are at present little understood; when chemical organic analysis can be more easily performed, several points which can now only be guessed at will be cleared up. Till this be the case, the treatment will remain, as it is now, vague and empirical, and the direct opposite of the certain and decisive method of treating acute dysentery, to which a correct knowledge of the morbid anatomy of this disease has led us." (p. 248.) We shall, however, submit our author's conclusions as to the existence of urea in the urine of patients labouring under hepatitis, a question which is still disputed:

"In diseases in which the secretion of bile is stopped, as in great primary or consecutive abscesses, where there is no jaundice, no bile in the stools, and none in the gall-bladder after death, we find the secretion of urea to be also arrested or nearly so. I draw this conclusion—first, from the lowness of the specific gravity; secondly, from the pale colour; and, thirdly, from the impossibility of getting nitrate of urea in the usual way.

"In that universal chronic dysentery where there are yellow liquid stools without bile, and in which there is no jaundice, the urea is also diminished, as in the former case. The liver seems not to secrete bile sometimes for days together, or even weeks. Abscess often supervenes. In duodenal hepatitis, without jaundice, and where bile is secreted, though perhaps in less quantity than natural, the urine is high coloured, and of usual or high specific gravity, and the

urea is apparently in normal proportion, or, at any rate, not lessened. In acute gastro-duodenal dyspepsia, with the liver acting more than usual, judging from the copious and dark or orange-coloured stools, and, judging from percussion, also enlarged, probably from biliary as well as venous congestion, the urine is of great specific gravity, so that the urea is probably secreted in undue proportion. In duodenitis with jaundice, where abundance of bile is secreted, the urine is loaded with urates, and probably with urea, as well as with the colouring matter of the bile. So that there really does seem to be a connexion between the secretion of the kidneys and liver, by means of which we may hope in time to acquire some knowledge of the condition of the latter organ. Whenever the secretion of bile is stopped there appears to be a diminution in the quantity of urea separated by the kidneys." (p. 252.)

This is a subject well deserving of further investigation. Twenty-five years have elapsed since the non-existence of urea in hepatitis was called in question by Dr. Prout and Dr. Davy, and yet nothing has been done to settle the point!

In concluding our remarks on the volume before us, we may repeat that we consider it very creditable to the author's talents and industry. It is a work adapted for the practitioner rather than the student, and should be suggestive to him of many useful reflections. We trust those who have the opportunity will test the facts adduced by Dr. Parkes in support of his hypotheses, and will follow up his observations to verify or overthrow them. Much also remains to be done by the aid of microscopic investigation, and still more of chemistry. To these we would specially recommend increased attention, and at the same time caution our readers not to neglect the means of investigation at present employed.

"I am not of the opinion of those," our author judiciously observes, "who believe that the old paths of investigation—the study of symptoms and of post-mortem appearances—are now exhausted. If this be the case, how is it that the morbid anatomy of dysentery has not, to my knowledge, hitherto been fully described? How is it that in cholera, in spite of ample opportunities, every new writer discovers something that his predecessors have overlooked? How is it that the most important renal diseases have only been described within the last few years? Because new modes of investigation have been opened to us, why should we give up the old ones?"

We take our leave of this volume with sentiments of great respect for its author. We feel assured that we shall again meet him in the field of medical literature, which the present work proves him so well qualified to cultivate and enrich. We see here combined quickness of observation, clearness of conception, ingenuity of speculation, and a calmness and soundness of judgment, unusual in a young writer, and not too common in those of maturer years. The medical service of the army has cause to regret the early departure from its ranks of an officer so well qualified to add to its already high reputation; but we hope the new sphere of duties on which Dr. Parkes has entered, may afford to himself and to the profession sufficient compensation for the loss.

ART. X.

Malgaigne's Operative Surgery. Translated from the French by F. BRITTAN, A.B., M.D., &c. &c.—*London,* 1846. 12mo, pp. 586.

THE Manual of Operative Surgery by Malgaigne is so well known to the profession as one of the most comprehensive works on the subject of which it treats, and enjoys so extensively diffused a reputation amongst the surgeons of the Continent,—having gone through four editions in the original, and been translated into five continental languages,—that it will be unnecessary for us to do more than to name to such of our surgical readers as are unacquainted with them the principal merits of the work. We are surprised that it has not sooner made its appearance in an English dress.

The chief value of M. Malgaigne's work consists, first, in its very comprehensive and compact character, embracing as it does in a small compass the whole range of operative surgery; and, secondly, in the extreme clearness and precision with which details, whether of surgical anatomy or of operative manipulation, are described. In these respects—the especial desiderata to be met with in a *manual*—it stands unrivalled by any similar treatise on the same subject in any language.

The work is divided into three parts. In the first, the author treats of what he terms the "Elements of operations," such as the management of the knife, of cauteries, and of ligatures or sutures; with some short remarks on the plans that have been suggested for lessening pain during operations. In the second part, general operations, from bloodletting and plugging teeth to the ligature of arteries and the resection of joints, are fully and clearly described. In the third part, we have a very complete account of special operations.

In describing an operation, the author first gives, in a very concise manner, the surgical anatomy of the part concerned; he then describes minutely, step by step, the ordinary method of operating; this is followed by an account of the principal modifications that have been, or are, adopted by different operators; and he concludes with a comparison between the different methods, a general appreciation of the operation as a whole, and with cautions as to the dangers that may occur during its performance, or by which it may be followed.

The following extract will convey to our readers a good idea of the plan adopted in describing an operation, as well as of the style of the translation.

"PHYMOSIS. The surgical treatment of phymosis comprises three methods: incision, excision, and circumcision.

"1. INCISION. *Ordinary proceeding.* The patient is seated on a chair, with his back applied against a wall so that he cannot start back; or laid on the right side of his bed. The surgeon pinches up the right side of the opening of the prepuce, and draws it a little forwards; he then insinuates a director between the glans and prepuce in the median line, and on the superior surface, as far as the cul-de-sac of the mucous membrane. An assistant supports the penis and draws back the skin, so that the incision shall not go too far. The surgeon himself holds the director in his left hand, and with his right passes a thin, narrow-bladed, sharp-pointed, straight bistoury along its groove. When he reaches the end of the

director he depresses his right hand, and brings out the point of his knife through the skin; and then sharply drawing it towards himself, he divides the prepuce from before backwards in one cut.

"The skin is usually divided further than the mucous membrane, and the latter is subject to form a small cul-de-sac beyond the incision: it should be divided with scissors.

"Some surgeons advise placing a small ball of wax on the point of a bistoury, and introducing it flat without a director between the glans and prepuce; and then, turning its edge up, finishing the incision as usual. For this the blade must be very narrow, or it will wound the glans or prepuce in its passage. We prefer the ordinary proceeding.

"*Proceeding of Cloquet.* It only differs from the former in being performed on the inferior part of the prepuce at one side of the frænum. It is described by Celsus.

"*Proceeding of Cullerier.* He divides the mucous membrane of the prepuce, only beginning at its edge as if to free the opening.

"*Proceeding of Carter.* This consists in three superficial incisions on the free edge of the prepuce, and on its dorsal and lateral surfaces.

"M. Malapert modified this proceeding by making two incisions at the prepuce, and the third at the frænum. These incisions, made at equal intervals, should be of proportionate extent to the constriction of the orifice—from a quarter to half an inch but never more. . . . . . .

"*Appreciation.* Incision by the proceedings of Cullerier or Carter may well be used in cases where an accidental swelling has constricted a prepuce that is otherwise large enough. But in natural phymosis, or when the edge of the prepuce is ulcerated or indurated, more powerful means must be adopted. Incision by the ordinary means leaves two long loose flaps hanging about, more or less tumefied; by that of Cloquet, a long, thick, deformed, flap: excision, usually by the proceeding of Lisfranc, is then indispensable. When all the free edge of the prepuce should be sacrificed, recourse must be had to circumcision, for which the proceeding of Ricord seems the easiest.

"Whatever proceeding is preferred you should bear in mind that the prepuce is not united to the glans by a circular commissure; but that this commissure follows the corona glandis; and that as it is oblique from above downwards, and from before backwards, in this direction the incision should be made.

"Whatever proceeding is adopted, you may apply to it a modification proposed by Mr. Hawkins, of St. George's Hospital, London. It consists in reuniting the skin and mucous membrane by means of four or five points of suture. In this way union by the first intention is often obtained; whilst, by the old method, there was often a nasty ulceration of the wound for a long time." (pp. 471-5.)

In giving an English version of Malgaigne's Manual, Mr. Brittan has filled a hiatus that existed in the surgical literature of this country. With the exception of Hargrave's Operative Surgery, which,—in the clearness of its details, and the comprehensiveness of the information it contains, resembles Malgaigne's work more nearly than any other in our language, but which is now out of date, and we believe out of print,—English surgeons do not possess a complete treatise on the operations of surgery. The works of Liston and of Fergusson, excellent though they be, are incomplete, in not containing the details of many important operations, and rather represent the peculiar practice of their respective authors, or describe that which is most generally adopted in this country, than give a general view of the whole range of operative surgery. To these works, therefore, the volume before us will be a useful adjunct; containing as it does a vast deal of information, especially relating to the practice of

continental surgeons, that we shall seek for in vain in any other treatise in our language.

Mr. Brittan has executed the task he had imposed upon himself in a very judicious and praiseworthy manner. The translation is extremely readable, and, although a close and faithful version of the original, is rendered in an agreeable and easy style, being totally devoid of Gallicisms. The value of the original, has likewise been much increased by the addition by Mr. Brittan of notes containing the details of a few operations omitted by the author, and of some that have been introduced into practice since the publication of the last French edition in 1842; of this kind are those of Fergusson and Dieffenbach on Staphyloraphy, of Clay and others on Ovariotomy, &c., and giving the opinions of some of the more celebrated English surgeons where they are at variance with the doctrines advocated by Malgaigne. The illustration of the work by some very well-executed woodcuts, representing the more important surgical regions, has added much to its utility, and we can strongly recommend it both to practitioners and students, not only as a safe guide in the dissecting-room or operating theatre, but also as a concise work of reference for all that relates to operative surgery.

ART. XI.

1. *Recherches Expérimentales sur le Développement de la Graisse pendant l'Alimentation des Animaux.* Par M. BOUSSINGAULT. (Annales de Chimie, tom. xiv, 1845.)
2. *Der Chemische Process der Respiration.* Von JUSTUS VON LIEBIG. (Annalen der Chemie, Band lviii, 1846.)
3. *Experimental Researches on the Food of Animals and Fattening of Cattle, with Remarks on the Food of Man.* By ROBERT DUNDAS THOMSON, M.D.—1846. 8vo.

THERE are no terms which have suffered more abuse in the hands of those who are imperfectly educated in the science of observation than those of *practice* and *experience.* An example illustrative of this affirmation will place it in a clearer light than any abstract reasoning could accomplish. It has been found by many practical and experienced men, that a very large proportion of patients treated by them while labouring under Indian cholera has terminated fatally, while to another class, equally entitled to the same appellation, the issue of their experience has been diametrically opposite. Instead of disaster they have met with recovery, and have without delay announced their success as the result of their superior method of treatment. Now here we have presented to our attention two common illustrations of the results of practical experience, each of which is incompatible with the other. If no other considerations are taken into account than the simple facts that such has been the experience of two practitioners, the unfortunate position, the *opprobrium medicorum,* the impossibility of arriving at a decision among disagreeing doctors is attained. We believe we do not exaggerate, when we affirm that this mode of drawing conclusions respecting the efficacy of medical treatment,

is a very common method of procedure, and that a large proportion of the cures which are daily recorded in respect to the treatment of fevers, and other diseases characterized by a distinct type, are not, in reality, errors of fact, but simple statements of the cases which have occurred, and of their termination, without reference to any important element which can enable us to decide as to the natural law of the disease, and, by consequence, as to the possible influence of medicaments administered to remove any natural symptom or excess of natural action. By this mode of viewing the question, then it is at once obvious that we admit the possible, nay, probable accuracy of two practitioners treating the same disease; the one with the most fatal results, the other with the most successful issue. But we do not concede that by practical experience or common sense alone, in the ordinary use of these terms, these two incongruities can be reconciled. Mere common sense, or the general opinion of mankind, although absolutely requisite to the philosopher, is, as Sir John Leslie has well observed, "a very suspicious standard of appeal in matters of science." We believe medicine, if followed by a philosophical course, to be one of the noblest of the sciences, and therefore subject, as much as any other, to suffer by a bare appeal to so limited a source of judgment. The philosopher in other sciences would not be contented with a mere determination of the facts, as in the case cited, and an inference; but he would investigate all the connecting circumstances, and eliminate every element which would appear to lead to an erroneous conclusion. He would thus endeavour to ascertain what are the usual phases of the disease, and in what respect they differ from those which it is concluded have been subjugated by medical appliances. He would thus be under the necessity of making an extensive series of observations, that is, in medicine, of collecting a large number of cases, carefully bearing upon certain distinguishing features of the disease. In the example of cholera, which we have cited, he would find that out of 100 persons affected with this disease, one fourth die on the first day of infection, or 25·219, while only 0·3 recover; on the second day 21·7 die and 1·25 recover; on the third day 10·5 die and 4·23 recover; on the fourth day 6·3 die and 4·9 recover; while on the nineteenth day only 1 dies and 12·5 recover; and after the 25th day no death occurs, and all who are still sick recover. (Farr, Medical Annual, 1839.) The anomalous and contradictory position in which the two practitioners stood, who were previously noticed, after the knowledge of this law, is now explicable. It would now be quite legitimate to infer, as the data were not supplied by the practitioners themselves, that the cases which all perished, were those of persons newly affected, while those which recovered, with few or no exceptions, were old cases, which had survived the dangers of the first stages of the disease. This example is sufficient to impress upon us the importance of an acquaintance with the laws of the animal economy, not only in health, but also in disease, before we can consider ourselves competent to decide upon the amount of influence which we can exercise over disease, or judge of its weak and impregnable points under the administration of curative means.

The same mode of reasoning is applicable to the means requisite for preserving the systems of animals in a healthy condition. Mere experience, after the fashion in which that expression is commonly employed, has

utterly failed to enable us to lay down strict rules for the regulation of diet. The glimpses of truth which, in this respect, peer out in the distance, have been derived from other than men of mere practical experience. The foundation-stone of a true theory of diet was laid by a religious father (Beccaria), and the more modern advocate of a philosophical view of nutrition was a physician (Dr. Prout), who rose to fame and improved our knowledge of the healing art by a thorough acquaintance and masterly application of the *principles* of chemical science. Mere experience declares that many practices are perfectly wholesome which it is difficult to find any other arguments to support. The use of alcohol among human beings, in moderation, as it is termed, is one of these, and even in towns it has become customary to stimulate the systems of cattle by means of pot ale, under the idea of increasing the amount of milk. Now, if this pot ale have any stimulating influence, it must derive such power from the presence of alcohol, as the fluid contains no casein, or, if so, but an insignificant portion, and hence is not endowed with any nutritive energy. (Experimental Researches, p. 12.) There cannot be a doubt that in general we are far more merciful in the treatment of the brute than of the rational creation. What would be the public expression, if the owner of a horse were to sentence his dumb assistant to receive five dozen lashes with the apparatus usually employed to castigate the rational soldier, and carry the sentence of his court-brutal into execution in a locality to which the people had access? or what would be the indignant criticisms of the press? and would not the very dregs of the population rush to the rescue, if houses should be licensed for the purpose of legally disposing of fermented fluids for the consumption of the inferior animals? Would one solitary being exist who should advocate such a palpable specimen of criminality? Yet, is it not obvious to the least experienced, that the brute creation possesses more physical endurance, and more power to resist offensive reaction, than the strongest of the intellectual class of beings? What then has been the result of the use of such stimulating fluids when administered for any considerable period to cattle? Mr. Harley, who kept the celebrated dairy at Glasgow, and has published an interesting description of his establishment, informs us that as the season advanced he was in the habit of feeding his cows with grains and distiller's wash, in addition to a certain portion of succulent food; "but they were apt to make the cattle grain sick, as it is termed, and to prove injurious to the stomach of the animal. It has been ascertained that if cows are fed upon grains or distiller's wash, their constitution will quickly be destroyed. Cattle thus fed should not be kept longer than eight or ten months." We believe this opinion of a man of great experience in the feeding of cattle to be borne out by that of others who have acquired knowledge in a similar practical school. If such, then, is the result of the administration of moderate doses of fermented fluids to the cow, an animal whose physical constitution is proverbially of no peculiar delicacy, it is natural to ask if it be not equally or at least somewhat noxious to the weaker physical condition of man. It is no argument in favour of any such experiment upon human life that existence does not terminate on its adoption, or that the symptoms of some frightful disease are not instantly ushered in; the seeds of future mischief may be slowly and gradually sown by even one or succeeding experiments, and

may only lie dormant until other repetitions shall cause them to spring forth into living activity. (Exp. Res., p. 7.) Our argument, then, is that it is impossible in the present state of our knowledge to declare that even moderate quantities of certain substances, usually admitted as wholesome articles of diet, do not tend to impair the integrity of the human organism. We have seen, in reference to cattle, that a fluid considered healthy for the system of man, is unequivocally pronounced to shorten their earthly span. Is it probable then, we should ask, that to man the same diet should be innoxious, and that no elements of future mischief have been thereby constituted. At present we are unacquainted with the secondary results of the application of fermented fluids to the interior of the system. We know that it stimulates the nervous system, and that its influence decays with tolerable rapidity; but with what chemical decompositions this subsidence is accompanied, has not yet been ascertained; and hence we are not in a condition to predicate whether the products of the secondary changes in alcohol may not be calculated to produce even a worse influence on the system than that which is effected on the nervous system by the primary stimulating and the resulting depressive action of this powerful agent.

There is a speculation with regard to the possible formation of alcohol from the sugar of the food in the vascular system, the investigation of which would lead to much important exposition of fact. Mitscherlich has given it as his opinion, that the sugar swallowed by animals may be converted into alcohol in the intestines or vessels into which the absorbed food passes. The possibility of this change, it is true, cannot be denied, but there is one objection to the probability of the alcohol remaining for any length of period in this form in the body, that the temperature of an animal would immediately tend to convert it into acetic acid. There is, however, a curious fact which we have met with, and which would appear to corroborate the suspicion of Mitscherlich; it is a circumstance related in a note by the late Dr. Oudney, in his African travels: "Several of our camels are drunk to-day; their eyes are heavy and want animation, gait staggering, and every now and then falling as a man in a state of intoxication. It arose from eating dates after drinking water; these probably pass into the spirituous fermentation in the stomach."

Mere experience, in the ordinary sense of the expression, is not calculated any more than in the case which we have previously cited to inform us as to the wholesomeness or noxiousness of the practice of consuming food in a state of partial decomposition. On the contrary, if we can judge by the extent to which the use of decayed cheese and putrid game is employed, we should be inclined to infer that experience is decidedly in favour of their consumption. It is necessary before we can pass a faithful judgment upon a question so deeply based as this, to take an extensive view of the constitution, of the animal system, and of the mode in which it is preserved in all its integrity from waste and decay. One of the strongest proofs of the inability of common experience to give a valuable opinion upon such a point as this, is the remarkable fact that we do not remember to have seen in any work of original merit upon dietetics the question discussed as to the propriety of permitting or withholding from human beings food, such as we have described to be in a rapid state of decomposi-

tion. Yet it is well known that cases of death have frequently occurred from the use of decayed cheese. The deteriorating action in these cases was produced by the influence of a poison, which communicated its baneful state of decomposition to the human system. The body becomes dry and emaciated, and death closes the scene, leaving a mummy-like wreck behind. In such an example, decaying cheese is discovered to be unwholesome, because its action has been allowed to proceed to excess; in short, because a telling experiment, to use a familiar phrase, has been performed. But to argue that it is only when such striking impressions are made upon the senses that we are to suspect disease, would be as fallacious as the conduct of the philosopher who should insist that such phenomena as the showy combustion of gases exhibited on the lecture-table, or in the laboratory of the chemist, exercised a more important influence in the domain of science than the long-continued, secret, and almost insensible actions of a weak battery, which are known to be analogous to the most important electrical operations of nature; far more important indeed, and much more extensive in their consequences, than the most awe-striking lightning or the most powerful reverberations of a thunderstorm. It is, therefore, we conclude with some show of reason, that the man of science advises the physician to direct his attention to principles, as well as to ocular demonstration, guided by mere common sense. It becomes the duty of the practitioner to inquire into the changes which the curd of cheese undergoes in such instances, and to institute a comparison between its condition when altered by an excess of decomposing action and the more limited change in its particles. When affected by common decay, the researches of chemistry show us that in the decay of cheese certain volatile products arise from the comparatively fixed curd, and new bodies are formed and introduced into the system of whose mode of action we are supremely ignorant, unless when an overdose should happen to be followed by a fatal termination. Anatomy would lead us to infer that it is in some alteration in the natural constitution of the albuminous element of the food and blood, giving rise to an organic volatile produce, that we may probably look for the cause of those diseases which are characterized by a regular type, and constitute at present the *opprobrium medicorum.* It is not at all likely that what we term malaria—an expression analogous to instinct in relation to the actions of animals—is in its nature allied to mere gases, since the results which we can trace as produced by the action of such elastic fluids upon the animal system are irregular and reconcilable in general with simpler chemical action on the tissues or great systems of the organisms, and quite distinct from the regular phases of disease, as typified by such complaints as smallpox and measles. The circumstance that we have gained little or no ground for centuries in the investigation of the causes and consequent treatment of diseases which are distinguished by peculiar types, is a sufficient argument for reforming our methods of research, and for concluding that our scanty knowledge on such subjects may possibly have remained stationary in consequence of our having substituted in not a few instances words for ideas. In reference to the noxious influence of putrid game, and other decomposing forms of animal food, although little attention has been hitherto bestowed upon tracing evil consequences to such a source, it is scarcely necessary to say, that all will agree in the probable bad effects

of such diet. The facts that over-salted food exclusively employed, is apt to induce scurvy, a state of the system in which the blood and tissues are imperfectly formed—that bacon in particular stages of decomposition and sausages have been known to produce death—that fresh dead bodies when excised communicate by inoculation dangerous disease to the human system—supply us with examples of injury, induced by an over-dose of noxious matter, and would lead us to the suspicion from analogy that a smaller dose cannot fail also to occasion a deteriorating action upon the organism, which is not the less influential because it is slow and silent in its operation. One of the great obstacles in the way of improvement in the medical profession, and particularly in the investigation of such diseases as we have just noticed, is the unfortunate love of argumentation instead of research which long ago crept in, and still adheres to the members of our noble science. We are quite willing to admit that the practice of meeting and discussing medical questions, is calculated to confer benefit on those engaged in it; but the propriety of that discussion being confined in general to raw and comparatively unlearned heads, and, above all, the wisdom of publishing such immature opinions, may safely be pronounced as highly questionable, irrespective of the injury inflicted on the youthful debaters. It has been well said that it is a very bad and dangerous habit to permit one's self to express an opinion regarding subjects which one has not *considered.* In this way, leanings are often contracted which continue through life, for nothing is more easy to acquire, or more difficult to cast off, than a mental bias. Many persons also can hardly engage in an argument without persuading themselves at least that that side is the better one which they defend. It is related that Pope Sextus V was impracticable to all but the Venetian ambassador. Those who investigated the art of the courteous politician discovered that when he wished to gain any point with the sovereign pontiff he began by proposing the opposite measure to that which he desired. This is too frequently the condition of our younger members, and having placed themselves in a false position, they often continue to occupy it through life.

It is too often the description of practitioners who oppose the results of what they denominate their experience to the efforts of exact science, to generalize or connect the isolated experiments or facts of the observer of disease. This conduct is very much on a parallel with that of the maker of any minute portion of the mechanism of a watch, who should refuse to allow it to enter as an element into the composition of the entire machine, by adducing trivial objections instead of giving his willing assistance to enable the work of his hands to occupy suitably the original position for which it was designed. It is a very common occurrence to find well and long-considered experiments objected to by crude and superficial arguments, which must have occurred also to the experimenter on his first survey of the subject, but which have long previously been overcome in his own mind. Often, too, they are assailed by those who are incapacitated from want of previous knowledge to give a correct judgment on the subject. We lately met with such an example in physical science, in the diseased leaven disseminated by a cheap press. The object of the writer, a Manchester Chartist, was to refute the first chapter of Genesis, for what purpose it is difficult to conceive, and the peculiar weak point for assault he

considered to be the passages in which it is stated that light was created before the sun, just as if, added our wiseacre, "any one were ignorant that the sun is the source of light,"—thus exhibiting his want of acquaintance with the present theory of optical science, that the sun is not the source, but merely one cause of the development of the phenomena of light. It is this description of reasoners, who would have us to reject every good or sound idea, merely because it had been contaminated by a polluted source, as if gold were not all the purer from its oft-repeated subjection to the fiery ordeal.

It is in a conciliatory spirit, therefore, that we should address ourselves to such researches as have for their object the investigation of the very elements of our physiological knowledge. Such works as those we have cited at the commencement of this article are of this description. It may be that the conclusions contained in them are not always perfectly demonstrated; but if it be conceded that the authors have been labouring in the right direction, it cannot fail but that new light must be thrown on the subject of their investigations, by the number of facts which they have adduced and carefully scrutinised. It remains for the physiologist to exercise a critical eye upon such labours, not with a quibbling disposition, but with the judgment of one possessed of the spirit of generalization.

Art. XII.

The Military Miscellany; comprehending a History of the Recruiting of the Army, Military Punishments, &c. &c. By Henry Marshall, F.R.S.E., Deputy Inspector-General of Army Hospitals, &c. &c.—*London*, 1846. 8vo, pp. 375.

Although the volume before us is not, strictly speaking, a professional work, it has many claims to our notice as medical reviewers. While the object of the author is stated to be "to supply the reader with some information respecting the constitution, laws, and usages of the army, and to excite attention to the means which may meliorate the condition of soldiers, and exalt their moral and intellectual character," he has entered at some length into questions of great importance to the army surgeon, and particularly as relates to his duty with reference to military punishments, and to the effects of these on the health of the soldier. Mr. Marshall is already favorably known to the profession by his 'Notes on the Medical Topography of Ceylon,' and his valuable work 'On the Enlisting, Discharging, and Pensioning of Soldiers,' which is still the best in our language on feigned diseases. To him also we owe, in a great measure, the valuable Military Statistical Reports, as he drew up the plan on which they were framed, and for some time superintended the preparation of them. It is not very creditable to the authorities that they permitted the officer who was intrusted with, and satisfactorily performed, this laborious and responsible task to remain upon half-pay without the slightest acknowledgment of his valuable services.

In a motto prefixed to the Miscellany, our author, in the quaint words of Sir James Turner, assigns a reason for having compiled this work: "You ask me, what moved me to write these discourses? If I were put to the rack till I gave you my reason, I could give you no other than this,

that being out of employment and not accustomed to an idle life, I knew not how to pass away my solitary and retired hours with a more harmless divertisement." We would be well pleased to see many more members of the profession spend their leisure hours in so praiseworthy a manner.

Our author gives an interesting historical account of the various modes of recruiting the army, from the days when the justices were required "to raise as many men *by impress*, for soldiers, gunners, and *chirurgeons*, as might be appointed by His Majesty and both Houses of Parliament," till the present time, when an error in the mode of attesting a recruit has been deemed a sufficient reason to grant him his discharge. But we must pass over these details, interesting and amusing as they are, and turn to that section of the work which more immediately treats of the duty of the medical officer, viz. that on punishments in the army. These may be considered in two points of view: 1st, as regards the duty and responsibility of the military surgeon; and, 2dly, with reference to their effects upon the health and efficiency of the soldier.

I. *The duty and responsibility of the surgeon in cases of corporal punishment in the army.* It is a remarkable fact, that no instructions nor code of regulations of any kind have ever been promulgated by the military authorities for the guidance of the surgeon when called upon in the course of his duty to superintend a punishment. When a soldier is tried by a court-martial, the following certificate is laid before the court:

"I certify that No. ——, Private ——— ———, of the —— regiment, is in a good state of health, and fit to undergo corporal punishment or imprisonment, solitary or otherwise, and with or without hard labour."

(*Signature of the Surgeon or Assistant-Surgeon.*)

If he is sentenced to receive corporal punishment, it is necessary, according to the regulations for the army, that a medical officer should be present when it is inflicted. This rule appears to have been framed for a twofold purpose: 1st, to prevent the prisoner escaping any portion of the punishment if able to bear it; and, 2d, to prevent the infliction being carried to such an extent as to endanger life. This has been very clearly stated by Lord Chief Baron Macdonald, on the trial of Colonel Wall, in 1802:

"It is usual," said his lordship, "even in the infliction of ordinary punishments, that the assistance of surgeons should be called in, when the punishment is intended at the outset to be only such as experience shows us is never, without a very singularly unlucky accident, attended with death. The medical officer is, it would appear, to guard the life of a delinquent under punishment, so that the army may not lose the services of a man by death or by being permanently disabled. In the execution of this highly important duty he must be guided by a knowledge of the physiology and pathology of the human body, the habits and duties of soldiers, and an acquaintance with the regulations and usages of the army. A medical officer is presumed to divest himself of any opinion he may entertain in regard to the delinquency a man has committed, or the sentence which has been awarded him; his duty being, in the first place, to prevent the man from escaping punishment by feigning indisposition; and secondly, to see that he does not receive such a degree of injury as may endanger life or disable him permanently for the duties of a soldier. While the surgeon should invariably lean to the side of safety, duty requires that he ought to be scrupulously careful not to unnecessarily obstruct the course of military law—the rules and usages adopted to establish and sustain military discipline."

Our author advises medical officers to recommend that corporal punishments should not in any case be inflicted during the great prevalence of endemic or epidemic disease, or when symptoms of scurvy or ill-conditioned sores appear among the men. We would be inclined to extend this to periods when the weather is either unusually hot or very cold.

"The medical officer usually takes his station a few paces behind the man who is undergoing punishment; but, should symptoms of fainting come on, he sometimes moves towards the front of the sufferer, so as to see his face." (p. 273.) "Pain, but especially pain which is inflicted or imposed as a chastisement, frequently excites fainting, or *deliquium animi*, and when this takes place it becomes highly expedient to arrest the infliction of punishment. . . . . . But a man under punishment is liable to a partial *deliquium animi*, or fainting, during which it has been recommended (and it is, I suppose, usual) to permit the punishment to go on during some seconds of impaired sensibility. In the slighter cases, therefore, of *deliquium* the punishment need not be interrupted; indeed, the stimulus of flagellation frequently restores the sufferer to himself. If, on the other hand, the *deliquium* continues and a man cannot be roused in a few seconds, if he perspires much, and if the pulse at the temporal artery becomes weak or scarcely perceptible, he should be forthwith taken down. I never considered it expedient to examine the irritability of the iris, as is sometimes recommended in doubtful cases, being always satisfied with the conclusions which might be drawn from the above symptoms." (p. 285.) "When an unusual degree of tumefaction of the back takes place during punishment, a delinquent should be taken down, as this symptom is frequently followed by long protracted disease." (p. 290.)

When the surgeon deems a soldier incapable of bearing any further punishment without risk of danger, it is his duty to report this to the commanding officer and to recommend that the man be taken down. We are informed by the Adjutant-General (Report on Military Punishments), "that it is at the *peril* of a commanding officer to order the infliction of a single lash after a medical officer interferes for the purpose of suspending punishment." It has occasionally happened that commanding officers have refused to comply with the suggestion of the surgeon, and have ordered the punishment to proceed; but this is a responsibility which few or none would be inclined to incur in the present day.

It would appear to be the opinion of military officers, that when death ensues, consequent upon flogging, the surgeon who superintends the punishment would be the responsible party. Thus, Major James (Regimental Companion) says, "we cannot omit mentioning in this place, that the instant a military culprit receives a lash, the surgeon becomes responsible for his life." And Sir Charles Napier (On Military Law) observes, "The fact is, that the medical officers are placed in a most unfair and perilous position. The danger to which the life of the culprit and *the life of the surgeon* are exposed, appears to be a powerful objection to this punishment. As to making the surgeon responsible, it is unjust to do so; the law places a man *by force* in a certain position, and orders him to act according to the best of his judgment. He does so, and there is an end of the matter, whatever may be the consequences, unless it can be shown that he was drunk or mad!" With the gallant officer's opinion on the equity of the case we cordially agree, but we differ from him with regard to the legal responsibility. The only recorded opinion of a judge upon this point, so far as we are aware, is that of Lord Chief Baron Macdonald, on the trial of Governor Wall, already alluded to. Referring to the non-interference of the surgeon, he observed: "I think it necessary

to tell you that, if a punishment is inflicted unusual in its circumstances, either as to quantity or the instrument with which that punishment is inflicted, it will not take off from those who inflict that punishment a great deal of responsibility. . . . . . Notwithstanding the surgeon attends, and notwithstanding he does not interfere and make representations upon it, they who inflict the punishment, if it should be most inordinate in its quantity, or in the manner of inflicting it, by the nature of the instrument or otherwise, may, under certain circumstances, not exculpate themselves." (p. 275.) Paris and Fonblanque state, that "it is generally supposed the surgeon who is present at a military execution is responsible for its consequences; *this is not legally true,* and it is physiologically impossible: the punishment is too uncertain in its operation to allow of any medical officer ascertaining the boundaries of danger."

We do not know what view the authorities at the Horse Guards might take in such a case, but we believe that, unless culpable negligence were proved, he would be acquitted by a civil tribunal.

II. *Influence of punishment on the health and efficiency of the soldier.* In discussing the different kinds of military punishments, Mr. Marshall proclaims himself an uncompromising enemy to flogging. We unhesitatingly rank ourselves by his side, and denounce it as a degrading and demoralizing practice, which *ought to be* unjustifiable in the case of a person in the position of a soldier. The great difficulty, however, is to find an adequate substitute for it, as no one can deny that, in the actual state of society, and with the amount of intellectual and moral cultivation possessed by men in the rank of our soldiery, some kind of punishment is absolutely necessary for the support of military discipline and efficiency. At present we have only to judge between the practice of *flogging* and the practice of *solitary imprisonment;* and looking at them both impartially, we are forced to come to the conclusion, that of the two, *as at present inflicted,* the former is preferable to the latter—that is to say, that a slight flogging is less injurious, both physically and morally, than a severe imprisonment. Surely the time is not far distant when both will be dispensed with, and some other mode of enforcing the requisite degree of order be discovered, less injurious to both body and mind. But assuming, for the present, that punishment is necessary, and that we have only to decide between the two in question, let us briefly consider their respective advantages and disadvantages. In the following remarks, it must, of course, be borne in mind that we do not at all advert to the system which was pursued when our author entered the service, whereby a court-martial had the power to sentence the prisoner to receive an unlimited number of lashes: one instance is on record of 1900 having been adjudged, and several of 1000 having been inflicted. In the present day the utmost number permitted by the Mutiny Act is 200, and even that has been further limited by a recent order of the Commander-in-chief, to 50.

The objections of our author to corporal punishment may, we think, be fairly summed up under the following heads: 1, it is cruel and inhumane; 2, it is very unequal; 3, it is ineffectual as an example to deter from crime; 4, it has no tendency to reform the culprit; 5, it leaves an indelible mark. All punishment, it must be admitted, is an evil; but it is, or ought to be, the infliction of a less evil for the prevention of a greater. For the prevention of crime and the maintenance of discipline in the army, we fear

some sort of punishment will be found necessary till that peaceful period arrives when nations shall study war no more. The question, therefore, appears to be not as to the existence but the nature of the punishment.

For the more serious offences in the army, not involving sentence of death or transportation, there are, as we have said, only two kinds of punishment—flogging and imprisonment. Let us briefly examine whether the objections of our author to the former are not equally, or even more applicable to the latter. 1. On the ground of cruelty and inhumanity. We have often seen soldiers discharged from prison on the expiry of their sentence, broken down in health and spirits, and scarcely fit for duty afterwards. Now we maintain there is less cruelty or inhumanity in flogging a man, especially under the restrictions now in force, than in inflicting a punishment, the result of which may very possibly be to shorten his life, or induce such a state of health as to necessitate his discharge from the army without a pension, without health to work for his daily bread, and with the prospect of lingering out a miserable existence as the inmate of a union workhouse. 2. The objection that it is unequal in its effects applies also to imprisonment. This is fully corroborated by a quotation from the late Dr. Malcomson (Letter to Sir H. Hardinge), whose opinion is entitled to great weight:

"Many men, particularly those of indolent habits, endure a confinement of four or six weeks on bread and water without injury to their health; but in some instances, a shorter period is sufficient to cause a total loss of appetite; the bread is hardly touched, and no other food being allowed, the patient is unable to eat or to digest it." (p. 300.)

3. That it is ineffectual as an example to deter from crime is, in our opinion, much more true as applied to imprisonment than flogging. In the one case, the infliction of the punishment is witnessed by the man's comrades, in the other, it is undergone at a distance, and the severity of its character is matter of speculation. 4. Flogging has no tendency to reform the culprit. We freely admit this; but we must add that we never yet saw a soldier morally improved by any length of imprisonment. Indeed, under the old system, when they were incarcerated along with civil criminals, the reverse was too often the case, and the acquaintances formed in prison were of a nature which tended to anything but improvement. During the last twelve months military prisons have been established throughout the kingdom, and this source of moral contamination has been removed. *Omne ignotum pro magnifico* is a principle so universal in its application, that we confidently anticipate hearing much of the beneficial effects of these new establishments, but we much fear time will prove their reformatory influence to be vastly overrated. 5. The objection of its leaving an indelible brand will not, we believe, apply to the present reduced amount of punishment.

It would thus appear that the objections to flogging, except the last, are equally, or more applicable to imprisonment; but the latter is liable to two very serious ones from which the former is free. 1st. Its deteriorating effect on the health, especially when of any duration; and, 2d, its injustice to the well-conducted soldier, who is obliged to do extra duty during the period his comrade is in prison. This last is a very serious objection, which, we think, has not received due consideration from the advocates for the abolition of flogging; it is, in fact, extending a spe-

cies of indulgence to the man of bad character at the expense of the well-conducted soldier. But the fact is even more forcible, as it involves a permanent injury to the individual and an ultimate loss to the service. The effect of imprisonment on the bodily and mental powers does not appear to have obtained the consideration it deserves; indeed, the only work on the subject deserving of mention is the admirable paper by Dr. Baly, in the 28th vol. of the 'Medico-Chirurgical Transactions.' Mr. Marshall has given several extracts from the Reports of the Inspectors of Prisons which fully bear out our observations (p. 300). These evils may perhaps be attributable, in some measure, to the reduced diet on which prisoners are placed as part of their punishment. As regards the present system, our author remarks:

"By the rules for the District Military Prisons, the diet is ordered to be for prisoners not in solitary confinement, 12 oz. of oatmeal or bread with half a pint of milk for breakfast, and 5 lbs. of potatoes with a pint of milk for dinner; if in solitary confinement, by sentence of a court-martial, 10 oz. of oatmeal or bread with half a pint of milk, and 4 lbs. of potatoes with a pint of milk; and if in solitary confinement for a prison offence, 1 lb. of bread, daily, with water for drinking *ad libitum;* but this punishment must not continue longer than seventy-two hours at a time for the reasons already stated. I do not consider this an adequate diet, and it seems doubtful whether the Secretary at War consulted any medical officer having much experience of *military* prisoners, before adopting this scale. It would be out of place here to dilate further upon the evils of an insufficient and inadequately varied diet, added to confinement, want of exercise, and depressing passions. It is sufficient to state that they are calculated greatly to injure the constitution, and to excite the most formidable diseases, although from their anomalous character, these often escape detection until too late to be remedied by art. When the health becomes impaired by scanty nourishment, the subsequent addition to the diet may fail to restore it."

From a full consideration of the question we are of opinion that corporal punishment, as at present restricted, is preferable to imprisonment in the army, because it is less injurious to the individual, it is more efficacious in deterring from subsequent crime, and as a warning to others, and it prevents a great injustice towards the well-behaved soldier. If the condition of the soldier could be so much improved as to render dismissal from the army a severe punishment, then might flogging and imprisonment both be abolished, but this is a desirable event which we fear it will never be our good luck to see accomplished.

The commissioners appointed to inquire into the subject of punishments in the army, and on whose report the system of military prisons was adopted, put the following question to the witnesses: "Have you observed the habits of the released men to be materially altered or *their health essentially affected?*" The answers to this exhibit a considerable difference of opinion, but it is worthy of remark, that not a single medical officer in the army was called before the commissioners. The report, therefore, contains ths *opinions* of military officers from the rank of Major-General to that of Captain, but the regimental surgeons, who from their position and their duty, must have the best means of judging on this point, were not called upon for the result of their experience! We feel confident their information on this subject would have been found much more accurate and trustworthy.

While we differ thus materially from Mr. Marshall in our estimate of

the relative effects of corporal punishment and imprisonment, we most cordially agree with him in his desire to see the prevention of crime the great object of attention. The increase of education in quality as well as quantity, the introduction of healthy amusements to counterbalance the attractions of the canteen, the improvement of the barrack-rooms, thereby affording the soldier a comfortable home in the evening, and a general increased attention to his wants and comfort, would tend more to reduce the necessity for punishment than any coercive measures. The army owes a debt of gratitude to our author for having exposed many abuses, and called attention to the means of ameliorating the soldier's lot. We strongly recommend the work to the perusal of our readers as instructive and amusing, containing much interesting matter on hygiene, and characterized throughout by a kindly feeling towards the soldier.

On one point connected with this subject we desiderate more accurate information, the influence of imprisonment on the soldier's health; and we sincerely trust the Head of the Army Medical Department will call for this from the surgeons in charge of the military prisons, and, should the result be as we anticipate, fearlessly bring the matter under the notice of the authorities.

Art. XIII.

Lectures on Subjects connected with Clinical Medicine, comprising Diseases of the Heart. By P. M. Latham, m.d., &c. In Two Volumes. Vol. II.—*London*, 1846. Small 8vo, pp. 419.

Some time since it became our pleasing duty to place before the readers of this Journal* an outline of the facts, doctrines, and opinions which Dr. Latham had put forth in a first volume of Lectures, clinical in their main character, on Diseases of the Heart. In the article then devoted to the subject, we examined somewhat in detail such of the author's general principles of medical faith as we deemed necessary; we shall consequently be enabled to devote the present sketch altogether to particulars.

Lecture XVIII.—Here Dr. Latham speaks concerning that period in the course of endocardial and of pericardial inflammations which intervenes between the cessation of all acute evidences of disease and the restitution of the organ to a state compatible with existing safety. Of ninety cases of acute rheumatism in which the endocardium or pericardium or both were inflamed, three (in two both membranes were implicated) terminated by death. In seventeen instances only could the observer "feel anything like an assurance of perfect recovery." And the remaining seventy patients? what of them? Why they left the hospital more or less completely free from functional disturbance of the heart—such disturbance as directly and distinctly affected their comfort and was perceptible to themselves—but carrying away with them that significant murmur, the sure index to imperfectly repaired mischief within the heart. But they were safe for the present; a process of reparation of some kind had been established, and upon this process or this condition Dr. Latham wisely comments as follows:

* See Brit. and For. Med. Rev., July, 1845.

"But what is reparation? It is neither health nor disease; but it stands midway between them, and partakes of the nature of both. Now it is nearer to one, and now to the other; now ready to fall back into disease, and now to go forward into health. Truly this reparation demands as much of the physician's care as either of the other two; for it has its aids and its hinderances, which it is our business to study and to interpret; its aids, that we may apply them and cherish them, and in every way make the most of them; its hinderances, that we may intercept them, or lessen them, or counteract or annul them altogether." (p. 5.)

Seldom to endocarditis, oftener to pericarditis, and oftener to endo-pericarditis there may succeed a time—days or weeks—during which it is yet matter of question whether the individual will live or die. The most intelligible cause of this is, Dr. Latham suggests, the variable amount of injury which has accrued to the heart by the inflammation that has passed. When there is a loud endocardial murmur, for instance, and no other condition implying disorder of the heart, no palpitation, no præcordial anxiety or pain, no œdema of the ankles, "surely *there is reason to believe* that the damage is small, consisting in those little beads of lymph, deposited upon the free edge of a valve, which dissection discloses as the effect of acute inflammation." But an endocardial murmur may be attended with the functional disturbances, which we have just supposed to be absent, and with others; and here Dr. Latham would *believe* that a greater amount of anatomical change has occurred. For acute endocarditis

"Can do more than deposit specks of lymph upon the edges of a valve; it can spread a layer of lymph over a great superficial extent, even (as I have seen) throughout an entire auricle; it can accumulate masses of lymph as large as a pea or a cherry-stone, or larger, and leave them pendulous into the cavity of the ventricle; or it can destroy half a valve by ulceration, or carry away a long strip of the membrane, and lay bare the muscular substance; or (as I have known) it can penetrate from one auricle to the other, and lay them both together. Here are some forms of injury too destructive to admit any such degree of reparation as will allow life to go on at all; and here are others not destructive enough to make such reparation altogether impossible, but only capable of it slowly, and likely to keep life in jeopardy until it is accomplished." (p. 8.)

Now the doctrine taught by this antithesis may or may not be correct in respect of individual cases of cardiac disease; but it is certainly incorrect as a general doctrine of pathology (the pathological anatomists know the fact, and strive, but ineffectually, to weaken its force). There is, in truth, no constant or tolerably constant ratio between the amount of anatomical change in organs and the extent to which their functions are perverted; the lungs, the brain, the kidneys, the intestine, furnish daily examples of the total deficiency of harmony of (what may be termed) the anatomico-physiological kind. Hence it is that although, in distinguishing the nature of a disease, the most important point is the due cognizance of anatomical alteration, in prognosticating its issue and its course, the general condition of the individual plus the local imperfection of function furnishes the most pertinent and faithful clue. But Dr. Latham spoke of the heart; and of the heart our proposition is equally true. However we did not quote the passage just set down, because of its pathological doctrine alone, but because also of the inkling it gives of Dr. Latham's experience in the morbid changes produced by acute endocarditis; certain of these changes are assuredly remarkable, and as assuredly infinitely rare.

But here is a passage, striking in the manner of Dr. Latham, and oc-

curring a few pages on, which corroborates, to our very heart's content, the justness of the criticism now ventured upon:

"Look to the mere matter and bulk of things, and think only of what is visible, tangible, and audible in parts, and you will come to strange conclusions: you will see people die of too little to kill them, and see people survive what is enough to kill them twenty times over. But if, in such events, you would know what it is that *mainly* kills, and what that *mainly* saves, you must look out of the part into the constitution at large: you must do so especially in diseases of the heart." (p. 14.)

The lecture closes with an allusion—it is a mere allusion—to certain cases in which life is jeopardized, not by the cardiac disease in itself, not by the general constitutional pravity of the individual, but by certain disordered conditions of the brain and spinal cord (maniacal delirium, epileptic or tetanic convulsions, chorea, coma, fatuity) superinduced by the cardiac suffering. And hereupon Dr. Latham takes just occasion to observe that disease is "a great physiological teacher,"—he has "never laid bare a living brain, a living spinal marrow, or a living heart,"—"he has read of experiments which he has never performed, and *never could bear to see*;"—but of little importance is the loss or the deficiency in the present case. For, "have all the experiments that were ever done or perpetrated upon living animals given intimation of an influence like this, proceeding from the heart to the brain, and from the heart to the spinal marrow? Has not disease here been our teacher?" True in science, true in morality, true in religion! Yes, disease is *the* physiological teacher. How admirably, for instance, has disease of the brain demonstrated the fallacy of the localizations of various mental and affective faculties, as set down by the reckless perpetrators of aimless and barbarous vivisections! How emphatically were we told by the hewers of living flesh, that the anterior lobes preside over the function of speech; that the force regulating movements held its seat in the cerebellum; that loss of motive power in the upper extremity would follow implication of the optic thalamus, of the lower extremity injury to the corpus striatum;—and how much of this has clinical experience proved to be real? Not one jot, not one tittle! Far from bearing out the phantasies of these pseudo-philosophers (whose wisdom consists in the power to invent new methods of torture, and the stoicism to survey with indifference the writhings of their victims), Nature, watched clinically, is seen, as it were, to tax her ingenuity in overturning them—so various, so unmistakeable, and so perpetual are the contradictions she furnishes to the inferences founded on the results of vivisection. There is no science, and no true benefit to humanity in these practices, which simply confer notoriety on those who pursue them, and apparently—but not really—save our generation the labour of clinical observation. We are glad to find Dr. Latham has the courage to confess he can feel for the sufferings of the lower animals, and the honesty to admit his want of sympathy with the *hoc genus omne* of nerve-twitchers and brain-cutters; he "*may* have learned something from them; something, how dearly purchased!"

Lecture XIX. "It is a general truth," observes Dr. Latham, "never formally declared, perhaps, but well worth our notice, and of great practical importance, that organs must be previously sound to show clearly the nature of the injury or malady which they suffer, and that in proportion as they are unsound, they are spoiled for giving expression to the ills which

afterwards befall them........A broken instrument is ever out of tune; whatever key you touch you can never bring out the right note corresponding with it." (p. 32.) This is perfectly true, and it is true not only in respect of functional disturbances, but also of physical signs. The history of the physical signs of the membranous inflammations of the heart proves this; but that history does not, in our experience, allow the clinical observer to acquiesce in the dictum of Dr. Latham: "In the first inflammation of the sound heart they were everything; in all after inflammations of the unsound heart *they are nothing*." And then proceeding to particulars, Dr. Latham affirms that in after inflammations of the pericardium there is no pericardial murmur, "and none there can be if its surfaces adhere completely. And if they adhere partially, and there be a murmur, it will not have the proper attrition in it, and so will want the exocardial (pericardial) character." Now we cannot help suspecting that Dr. Latham's devotion to antithesis has led him to state his doctrine of the inutility of physical signs in a second inflammation of the pericardium both more broadly and more uncompromisingly than his own observation, had he accurately analysed *all* its results, would warrant him in doing. It is no doubt true, that *if* the pericardium be totally adherent to the heart from previous inflammation, new inflammation will set up no friction-sound on its surface; but such total adhesion is not the common result of pericarditis. Imperfect adhesion is the rule; hence Dr. Latham must turn for defence of his general proposition to the deficiency of "the proper attrition" in the new-made sound by the new-made pericarditis. But admitting (which is not always, we say, admissible, except *argumenti gratiâ*) that the attrition character is wanting—the position in which the murmur is heard, its instability in character as in precise locality, its non-transmission beyond the præcordial region, its evident superficial seat, all these conditions will point to the pericardium as the seat of the new-made sound. A case which we have at present under observation illustrates and justifies these remarks; but we must confess we know not (nor does any one know, nor can any one now guess) in what *proportion* of cases similar conditions of murmur may be verified and *new* pericarditis be diagnosticated.

Next Dr. Latham applies his general doctrine to endocarditis. It is well known to every one accustomed to pass round the wards of hospitals, that it is the general habit, on the admission of a patient with acute rheumatism, to pronounce that patient to be the subject of acute endocarditis, if an endocardial murmur exist. Yet observe how erroneous the conclusion; for the murmur audible may be either the result of old endocarditis, or of new endocarditis, or of new endocarditis superadded to old, and both conducing to the same modified sound. All this Dr. Latham puts with his usual point and terseness in the page before us; and the fact that he does so, supplies a very remarkable illustration of the indubitable truth in regard of the mechanism of mind, that multitudes of men will have a keen perception of, and will emphatically uphold, a certain given general doctrine as such, and yet fail altogether to see its applicability, or, at least, to apply it, in particular cases. And the fact supplies the illustration in this wise: in Dr. Latham's first volume appear tabular views of the frequency of membranous inflammations of the heart in rheumatism, and in those tables, and in the commentary upon them, all

the cases of endocarditis are referred to as examples of *acute* inflammation. This matter has been better elucidated by Dr. Taylor, in his most valuable paper on the 'Causes of Pericarditis,'* than by any other writer, though within certain limits his observations have been anticipated by the statements of M. Chomel.

But the great practical question is this,—are there any means whereby the recent can be distinguished from the old endocarditis, and the compound of both diseases from either singly? So long as all cases of rheumatic endocarditis, were habitually, when observed, set down as recent, it was not to be expected that any clue to the problem should be discovered,—its necessity was not dreamed of, why then should it be looked for? Yet the point is a peculiarly interesting one; a man lies before you, his joints the seat of migratory rheumatism, his heart excited, an endocardial murmur audible. Are you to forget his rheumatism, comparatively speaking, and treat his heart, or give yourself no special present anxiety concerning the state of that organ? The answer must depend upon your power to diagnosticate the age of the endocarditis. Now, how far goes that power? Dr. Taylor has a claim to be heard on this question, for his observations upon it are not only true and just, but stamped (like his entire paper) with a character of philosophical caution and severity, which is as striking as it is unfortunately rare in medical writings. He says:

"I have been enabled to discover the existence of *recent* endocarditis in a few cases, chiefly by the following circumstances: 1. By the appearance of a bellows-murmur, which could not be ascribed to rheumatism, or to any other obvious cause; and in a patient who was found to be free from it on admission. 2. By some considerable change in the rhythm of the heart, in cases in which it was at first found to be natural. I have observed the pulse to become *decidedly less frequent*, and each individual beat to be more slowly performed; in other words, both *slow* and *tardy*. The action of the heart becomes irregular, both as to its force and rapidity, and the duration of the periods of rest and often also, intermitting. With these signs, I have generally observed the bellows-murmur to persist after the heart's action has again become regular. We might not perhaps *a priori* have expected the contractions of the heart to become less frequent and tardy in a disease which must increase the sensibility of its lining membrane. Perhaps the cause is the same as that of suspension of the peristaltic action of the intestines in peritonitis, and as that of the paralysis of the intercostal muscles, which some physicians believe to occur in pleuritis. Be the explanation what it may, however, I am quite sure, both from the careful examination of such cases during life, and from an examination of the heart after death, that the symptoms I have described are produced by endocarditis. But then they are not present in all cases of endocarditis, as I have also ascertained. Were I to trust to the impression on my own mind, unchecked by figures, I should say that I have found the symptoms in question, in cases of endocarditis, unaccompanied by pericarditis; and that in cases of endo-pericarditis, I have found the frequency and perhaps the quickness of the pulse increased, with or without intermittent or irregular action. Cases of pericarditis and of endo-pericarditis, with increased frequency and quickness of pulse, would appear to be unfavorable to the explanation of the slowness and tardiness of the pulse already adverted to." (loc. cit., p. 496.)

But it will be perceived that, whatever be the accuracy of these observations, they have no reference to the solution of the problem most frequently presented in actual practice: they refer to cases in which an

* Med.-Chir. Transactions, vol. xxviii, p. 493.

endocardial murmur is developed *after* the patient has come under observation, and not to cases in which it is already existent when the physician first sees him.

Lecture XX. But, as is well known, organs which have once suffered inflammation are prone to suffer afresh under the influence of exciting causes. When a man has had his cardiac membranes once affected, and is subsequently seized with rheumatism, pneumonia, pleurisy, or fever, and exhibits increase of habitual palpitation and pain, then the existence of fresh inflammation should, according to Dr. Latham, "be assumed as a fact." He goes on to say, that, for admitting the existence of this new inflammation, and for our guidance in treating it, we "have only the warrant of conjecture. . . . . But there is such a thing as sober conjecture, as well as sober certainty. And diseases are treated, and cures are achieved, and lives are saved, *as often under the guidance of one as the other*." This seems a startling article of therapeutical faith, but it nevertheless conforms with fact, and will be found to lose much of its paradoxical character in the commentary of its promulgator.

Lecture XXI. The lecturer proceeds to the consideration of the unrepaired effects of endocarditis and pericarditis as constituting permanent unsoundness of the heart in themselves, and becoming the possible elements of further unsoundness beyond themselves. The morbid appearances left by endocarditis he finds to be "opacity and thickening of the membrane, marks of perfect and imperfect cicatrization, and breach of surface or solution of continuity." Now, concerning the real existence of the first kind of change, no doubt can be entertained, but of the second and third we are not so sure. Hear, however, the author's description:

> "Beside such general opacity and thickening a particular valve sometimes presents a hard elevated line or ridge where it is especially thickened, or a small spot where it is indented or depressed, looking like a complete cicatrization in one case, and an incomplete cicatrization in the other. Sometimes a valve is perforated or cribriform or it wants a portion at its edge, or a tendinous cord is snapt in two, and its ends are hanging loose within the cavity of the ventricle." (p. 79.)

Now, we altogether question the fact of the appearances here referred to signifying a cicatrization-process, either perfect or imperfect: we know the appearances, and we cannot conceive them to mean anything but irregularity in the deposition of the plastic matter exuded by the inflammation which is gone. Why, if these be signs of cicatrization, ulceration and healing of the valves are among the commonest of phenomena,—and yet, who is in the habit of stumbling upon many unquestionable valvular ulcers? And, further, the perforations and cribriform appearances of the valves, which are doubtless far from uncommon, cannot, with any colour of plausibility, as far as we know, be traced to inflammatory action. What is the coexistent change in the anatomy of the parts, what the feature or features in the bygone clinical history of the owner of those parts, which enables Dr. Latham to ascribe, thus unhesitatingly, the cribriform condition of valves to inflammation? We know of none, and shall be glad to learn what they are.

Lecture XXII. Here are considered the consequences to life and health produced by the permanent unsoundness entailed by endocarditis. There are three kinds of cases observed, and the sum of Dr. Latham's ob-

servations may be given in the following quotation, wherein well-known facts are impressively and pointedly put:

"Taking then the three descriptions of cases in their order, I believe it to be the tendency of each to pass progressively onward into the others. The endocardial murmur left by acute endocarditis may be simple and alone, and so it may remain for years, but it is ever apt to have a palpitation added to it. The palpitation accompanying the murmur may be occasional only, and so it may continue for years; but in the mean time, it is ever ready to become permanent. The permanent palpitation may remain for a while moderate in degree, but it is always tending to become greater and greater. Of these three conditions, then, the best that experience allows us to hope is, that each may remain stationary: for their changes are never retrograde, but always progressive, and always for the worse. Each condition becomes worse as it is converted into the other, and the condition of permanent palpitation passes on to new results, and to the final and fatal event." (pp. 96-7.)

Lecture XXIII. In this discourse the permanent unsoundness derived from pericarditis is the subject. Once pericarditis has ceased, auscultatory signs cannot be appealed to for information concerning the state of the pericardium. There is, in the first place, no such thing known, Dr. Latham "believes," as permanent continuance of a pericardial murmur. In this belief Dr. Latham is certainly supported by all recorded experience, so far as we know; but we have our *suspicions* (and why suspicions may long continue suspicions only is readily intelligible in a case like this), that there are *rare* instances in which the friction-sound of pericarditis permanently remains in a modified form.

There may be, as results of pericarditis—1, universal adhesion of the pericardium, and complete obliteration of its cavity by hardly any apparent medium of adventitious substance; or, 2, instead of the adhesion of the pericardium being total and the obliteration of the cavity complete, both may be partial only; and, 3, it is not only the *extent* of adhesion that varies, but the *quantity* of uniting medium also. There may be naught but thready filamentous texture holding the two surfaces together, or there may be a solid mass, more than an inch in thickness, and this solid mass is well described as follows:

"Its texture sometimes laminated like the coagulum of an aneurismal sac, red or tawny near the heart, and pale or white more remote from it, sometimes of a mixed consistence, in part almost liquid and purulent, and in part solid or tuberculous. Or the adventitious substance has been of one uniform texture, either so like muscle as to be at first mistaken for the fleshy substance of the heart itself, or so far firmer than muscle as to resemble flesh hardened in brine, either much paler than the heart, or much redder from being deeply injected with blood. This tough flesh-like substance may occupy a portion only of the surface of the heart or the whole of it. I have seen it opposite the right auricle, while everywhere else the pericardium has closely adhered with little intervening medium, and I have seen it enveloping the entire organ and forming round it (as it were) another case of muscle. And then, if (what often happens) the muscular substance of the heart itself be augmented, a strange spectacle is disclosed on dissection. There is an enormous mass displacing the lungs and leaving nothing visible in the entire front of the chest but itself." (pp. 115-16.)

Lecture XXIV. "Permanent unsoundness of the endocardium from diseases of a specific and malignant nature, especially from analogous formations," forms the subject of this lecture,—one of the least satisfac-

tory of the set. We do not see how "analogous formations" (e.g. ossiform, cartilaginiform matter, &c.) can be classed as malignant. And we believe that Dr. Latham is not justified in calling the cartilaginiform-looking productions which occur in connexion with the membranes "cartilaginous depositions,"—they have not the intimate structure of cartilage. The diseases giving rise to these depositions, Dr. Latham assumes to be different from inflammation—whereby he means, different in their origin from that state. Now, this may be so,—it may be that ossiform-deposition takes place where no inflammation has gone before, but Dr. Latham should have proved this, as there are persons who hold the opposite opinion. M. Bizot has clearly demonstrated the fact that in the arteries the formation of ossiform substance arises independently of inflammatory processes, but it does not thence follow as a necessary consequence that they shall have no connexion therewith in the heart.

LECTURE XXV. There is nothing novel in the matter of this lecture, which is devoted to the consideration of "acute, pervasive, pus-depositing inflammation" of the muscular substance of the heart. The cases referred to are those of Messrs. Stanley and Salter: they are said to be the only ones that have fallen within the lecturer's knowledge.

LECTURE XXVI. Chronic inflammation of the substance of the heart, terminating in ulceration, in partial dilatation, or in possible rupture, is the main subject of consideration in this lecture. Cardiac aneurism receives no addition to its pathology at the hands of the author; and, like his predecessors, he is obliged to admit that the diagnosis of the affection is unattainable during life. This admission he makes in the following passage, which gives a good notion of the existing deficiency of our knowledge on the subject:

"Now our clinical acquaintance with these diseases during life has not kept pace with the knowledge which anatomical investigation has procured us of them after death. Sometimes they have had their beginning and their progress without awakening in the patient the least suspicion of anything wrong within the heart. He has had no consciousness of ailment or suffering, and the fatal consummation has been an awful surprise. Sometimes they have been attended with suffering enough to alarm the patient, and by symptoms enough to enable the physician to infer damage of the heart, and even to anticipate its fatal event, but not to be sure of its nature; such as faltering and failure of the circulation and dyspnœa and anguish, either constant with occasional aggravations, or altogether occasional and in paroxysms, but, whether constant or occasional, never attended with any precise auscultatory signs. But sometimes they have had the accompaniment withal of precise auscultatory signs, and these have gone to the clear diagnosis of certain present conditions of disorganization within the heart. But then these conditions have been no essential part of *the* disease. Auscultation has told of hypertrophy and general dilatation of the ventricle with certainty enough, but it has left the partial aneurismal dilatation and the circumscribed progressive ulceration and the impending rupture entirely unsuspected." (pp. 148-9.)

Hereupon follow two cases in which the disease existed, but was not even dreamed of during life. The narratives of these cases are instructive, but their utility does not appear to us as much increased as the lecturer, from internal evidence, probably fancies, by the quaint and rather affected style in which they are set down. It strikes us that, if we heard a lec-

turer proceed to the description of a case of disease in the following manner (as Dr. Latham does in another part of his volume)—"There was a certain youth, David Aikin by name, and he was fifteen years of age; he was a poor puny lad, and first came under my care at St. Bartholomew's Hospital," &c.—we should feel disposed rather to smile in expectation of a jocular tale, than to put our wits in order for the reception of solid instruction. But this is, we confess, little more than a matter of taste.

And of the evidence of fatty degeneration of the heart Dr. Latham has little to say; but he appears to us to have said that little so well, that we extract the passage;—it contains the sum of knowledge on the matter:

"During the life of the patient, however, there is (as far as I know) no sure diagnosis of the fat heart, but a probable conjecture only. And even this probable conjecture can scarcely be made while the heart is *simply* fat, and nothing more, but must wait until it has reached that further disorganization to which it naturally tends, namely, dilatation. But, when dilatation is ascertained by its appropriate signs, if valvular unsoundness, as its cause, be excluded by the absence of endocardial murmur, and if a feeble fluttering movement of the heart be felt at every part of the præcordial region, or beyond it; and if, moreover, the constitutional habit of the man be such as to accumulate fat in all other parts, then it may be taken almost for certain that fat is especially deposited upon the heart at the expense or detriment of its muscular substance. Be it always remembered, nevertheless, that our inference, however correct it may turn out, is drawn, not directly from any express diagnostic signs, but indirectly from coincident circumstances. No murmur reaches the ear to tell us at once that the heart is fat. But we know that the heart is feebler and more capacious than natural. And we know that such, if life lasts long enough, is ultimately the condition of all fat hearts. Besides, we observe that the patient is *altogether* fat, and so we infer the probability that the heart has not escaped his constitutional peculiarity." (pp. 166-7.)

Lecture XXVI. Dr. Latham now comes to those affections of the heart which, he says, "may be usefully classed together under the name of unsoundness from disorganization." These affections consist of alterations of size and shape, and bulk and capacity—in fact, hypertrophy, atrophy, dilatation, and contraction. And Dr. Latham designates them "*as a class* by this name of 'unsoundness from disorganization,' that it may help us to keep in mind the important truth, that disease properly so called does not enter into the actual process of their formation." We confess the phraseology does not please us: we have, in the first place, no notion of *disorganization* without *disease* (unless, of course, in instances where destroying external agents act on the textures); and in the second, the application of the term *disorganization* to a state of *hypertrophy* seems to us (to use Dr. Latham's antithetic style) happily infelicitous. It is to be observed, that we do not contest the justness of the pathological notions the lecturer had in view: but object to the terms simply which he has fixed on to give signification of them. In his comments on the subject there is nothing new.

Lecture XXVIII. Here we have considerations on the fact that "unsoundness of the heart from disorganization is sometimes traceable to an accidental shock which it has sustained." Cases of three kinds are referred to rather than related;—in some, death had occurred after a certain lapse of time from the receipt of an injury in the region of the heart, or after a violent effort or a violent fit of passion, and rupture of a valve has been

discovered, with superadded hypertrophy; in others death had not occurred, but palpitation, arising under the same influences as causes, had remained more or less permanent and constant, and here a similar rupture may be supposed to have occurred in the valves, or possibly some injury to the muscular structure itself; in yet other cases the clinical history was the same as in those we have just spoken of: a post-mortem examination took place, hypertrophy was discovered, but no evidence of material injury inflicted on the heart at the time of the accident, to which the symptoms of heart-disease (palpitation, painful impulse, &c.) had been confidently and uniformly referred by the patient. Now, in these latter cases a natural question arises, which Dr. Latham puts and responds to in the following manner:

"But is it quite certain in these cases that the hypertrophy and dilatation really came from a material injury done to the heart at the time of the shock? In neither of them did the heart present the visible traces of any such injury as could be conceived to proceed immediately from violence. Still I do not know that anything short of absolute disruption must necessarily leave the characteristic marks of itself ever afterwards. It is conceivable that the injury itself might not be of a permanent nature, and yet abide long enough to lay the foundation of permanent disorganization. Further it is possible that, in these same cases, causes might have been found in other parts of the body (for such it will presently appear there often really are) entitled to a share in producing what was found in the heart. Nevertheless the shock, that had been suffered in both cases, was a remarkable part of their clinical history. The patients themselves constantly ascribed to it the origin of all their malady. We cannot therefore exclude it from our consideration, and may venture, without speculating further upon what cannot be proved, to regard it as in some manner powerfully conducive to the hypertrophy and dilatation of the heart, and to the fatal event." (pp. 207-8.)

This is an interesting lecture upon a subject which has attracted comparatively little attention. It would appear to follow, from the cases referred to by the writer, that life, in case of injury to the heart's structure, has been distinctly saved by full bleeding. That quietude, moderation in sexual intercourse, &c., are indicated subsequently, when the first danger has passed, is so plain, that the fact needs not to be insisted on.

LECTURE XXIX. Conditions conducive to "organic unsoundness" of the heart are found beyond the organ itself, in parts near and in parts remote, and in the constitution at large. Thus dilatation of the aorta, as well as unnatural narrowness of the aorta, will lead to hypertrophy of the heart—both for very obvious reasons. Cases no doubt occur in which the result is not witnessed, and then counteracting influences may sometimes be detected, sometimes be matter of conjecture, and sometimes no clue to them may exist. Dr. Latham believes that dilatation of the aorta is more apt to affect the heart when it occurs as a general enlargement of the vessel over a certain space, than as an abrupt expansion in the form of a sac; and also that the nearer the alteration of form of the vessel lies to its origin, the more capable is it of producing the ill influence in question. Of the general accuracy of both these propositions we have no doubt.

Among causes exterior to the heart capable of producing its "disorganization," are certain diseases of the lungs, offering impediment to the circulation through those organs; the right cavities undergo dilatation with or without hypertrophy. But pulmonary consumption, the disease which renders useless so vast an amount, it may be, of lung, does not pro-

duce, except in rare instances, such dilatation. How comes this? Simply from the fact that various agencies are at work in the constitution at large, which bring down the amount of blood in the individual—the amount of blood requiring to pass through the lung. All this is matter of familiar knowledge; but here is an application of this familiar knowledge which, if not actually new, is well and strikingly put:

"It is remarkable in this disease [phthisis], how those symptoms which are considered to be of the most fatal omen, seem to arise out of an express provision of nature for prolonging the duration of life. The hectic perspiration, the occasional diarrhœa, the sputa, the languid powers of nutrition, all tend to keep down the current of blood to that measure which can obtain an easy passage through the lungs. On any other terms the patient would die of suffocation suddenly, and at an early period of his disease." (p. 218.)

And it follows, as well from actual observation as from reasoning, that the influence of causes seated in the lungs in producing dilatation of the right side of the heart, is best to be seen in diseases which, while they cause great impediment to the circulation, yet do not much impair the general health or the powers of nutrition.

Upon impediments arising out of morbid actions in distant parts Dr. Latham says:—

"I cannot so easily accommodate my mind to an hypothesis as to believe all that is pretended concerning them. I find depositions of lymph in the cellular texture of a limb, constituting what is called a solid œdema; I find tubercular depositions in any organ, such as the liver; I find even simple inflammations of distant parts seriously insisted upon, as if they were well-authenticated causes of disorganization of the heart, when they have happened to exist together with it. And the *theory of mechanical obstruction* is brought in confirmation of the fact. For, say the theorists, where there is inflammation, there must be spasm of the extreme vessels, and spasm is tantamount to obstruction. And again, where there is effusion or deposition of any kind, there must be pressure upon the neighbouring blood-vessels, and pressure must produce obstruction, partial or complete, according to its degree. Now, by parity of reasoning, there is no conceivable sort of morbid action in any part of the body, which may not be construed into an obstruction of the blood-vessels, and thus conjured into a possible cause of disorganization of the heart." (pp. 221-2.)

Dr. Latham turns to the influence exercised by the renal disease attended with secretion of albuminous urine, in the generation of "organic unsoundness" of the heart. Incidentally he reviews the chief morbid states of various textures and organs, which experience has taught us to connect with that renal affection;*—and he presumes it probable that the source of those various states lies in a poisoned state of the blood, an opinion which has been held by many observers, and one which is unquestionably put forward upon very plausible grounds. But Dr. Latham's remarks upon this subject are the merest generalities.

* We cannot omit this opportunity of directing the reader's attention to the researches of Dr. Taylor on the influence of Bright's disease in producing pericarditis and various other internal inflammations. These researches, contained in the paper already referred to, exhibit, to a degree for which pathologists were unquestionably not prepared by the inquiries of any preceding observer, the active power of the renal affection in generating the cardiac inflammation. But, valuable as this point of knowledge is, we consider that its establishment is but one of the least merits of Dr. Taylor's paper. We admire that paper infinitely more for its general spirit than for any of its special results,—for the devoted zeal with which truth has been sought in its pages, for the sound logic of its deductions, and its uncompromising rejection of those loose speculations which pass for wisdom with the crowd, and are known as "foolishness" by the few.

LECTURE XXX, treating of the treatment of chronic valvular unsoundness, is principally interesting from the remarks it contains upon the existence of valvular murmur in children, first *accidentally* discovered, not traceable to any attack of acute diseases, remaining for years unaltered in character, not affecting the local or general health of the individuals, or preventing them from indulging with impunity (as far as their own feelings go) in violent and athletic exercises. What is the state of things in such cases? One of the kind we have ourselves seen,—the loudest systolic endocardial murmur we ever heard, musical and sonorous, pervading the chest before and behind of a female child, aged about five years (a very model of health), had all the negative characters in respect of its history and pathological influences we have above enumerated. Dr. Latham pleads ignorance of the nature of such cases; we must be content to follow in his track.

LECTURE XXXI.—Hypertrophy of the heart—is it curable or not? Dr. Hope says he has cases which afford him "reason to believe that *nearly the whole,* who are under the age of forty, may be *radically cured,* provided the hypertrophy is exempt from complication with valvular or aortic disease, adhesion of the pericardium, softening of the heart, or other organic obstacles to the circulation; and provided, also, that the constitution is sound, and the general health tolerably good." Dr. Latham, *per contra,* says, "I must confess that in the whole course of my experience I never yet met with a single instance in which I was perfectly satisfied that hypertrophy was cured." To which of the learned doctors shall we extend our faith? Collateral evidence may help us in deciding. In the first place, the marvellous Dr. Hope gives no proof of his marvels,—he has given no single specimen of his cases; the boastful allusion recorded above is all the information we get concerning them. In the second place, it was one of the failings of Dr. Hope's mind, and one of the weaknesses of his moral nature, to believe, or to feign that he believed, himself a man of wondrous dexterity in the cure of disease,—witness the special prowess he imagined he possessed in curing acute rheumatism, and warding off harm from the membranes of the heart; as likewise his posthumous paper, wherein he appears in the amiable and scientific guise of a very heaven-sent curer of empyema! On the other hand, Dr. Latham gives no sign of undue distrust in the powers of medicine generally, and yet he unhesitatingly proclaims the incurability of the disease we speak of. Hence we are prepared to believe Dr. Latham on the point at issue; more especially as (pardon the egotism, reader,) we have yet ourselves to behold the cure of an absolutely and truly hypertrophied heart. There is a *mock* hypertrophy, so like, in some of its characters, to the real, that Dr. Latham would "find no fault with his auditors for being taken in by the counterfeit." This *mock* hypertrophy he describes graphically in these words:

> "There may be violent impulse of the heart, felt not only in the præcordial region but in every part of the front of the chest upon which you lay your hand; and there may be pain in the heart, and pain and throbbing in the head; and all these may be never absent and often aggravated from time to time by accidental circumstances; and they may continue from first to last for several months or for several years, and produce in the mean while an incapacity of all useful exertion both mental and bodily: all these may be, and yet there be no hypertrophy." (pp. 248-9.)

And this morbid state is curable,—curable with variable amounts of facility in different cases, but still curable.

Dr. Latham's remarks on the necessity for caution in the employment of venesection in cases of hypertrophy (we mean the real) are good and sound. He believes that by leeching to a small extent on the præcordial region we shall be able sometimes "to overpower the most tumultuous conflict of pain and dyspnœa and nervous alarm which can be conceived," and that the plan "succeeds oftener in bringing relief than either venesection or cupping." Why this is so is not very clear, but so it is. "Beware," adds Dr. Latham, "in the management of hypertrophy of the heart, beware, above all things, of bleeding your patients into paleness and poverty of blood." And all who have witnessed the effects of that terrible combination—hypertrophy and anæmia—will echo the caution.

Lecture XXXII. Softening of the heart occurs as a part of certain general diseases,—of typhoid fever, (as shown by M. Louis, and insisted on by Dr. Stokes,) of scurvy, and of anæmia. Cure these affections—give respectively stimulants, lemon-juice, and iron—and you cure the softening also. But there is another species of softening, "secret in its beginning, and secret still in its stages and periods."

"The patients are in the decline of life, or they have forestalled the season of old age by intemperate habits. Their nervous system may be shattered: their arteries may have undergone extensive changes of structure; their livers may be enlarged, their kidneys may be granulated. All of these forms of disease may be, and some of them are sure to be, conjoined with softening of the heart, before it comes to be known and to be treated." (p. 268.)

And this softening may be accompanied with hypertrophy or atrophy, and in either case by dilatation of one or both ventricles. And all this is incurable, utterly.

Lecture XXXIII. The effects of unsoundness of the heart in its different forms upon the vascular system, venous, arterial, and capillary, form the subject of this lecture. The matter is illustrated and enforced in the usual pointed and emphatic style of the lecturer, and the whole deserves to be deeply considered by the practical physician,—but there is nothing actually new to be dwelt upon by us. The following passage, in which the author insists upon the advantages of hypertrophy of the left ventricle as a state superadded to valvular disease, seems, however, to require brief notice:

"Here it is the hypertrophy, which is the safety of the patient and enables life to go on as it does. Take away the hypertrophy and leave the injured valve, and the patient would be in a far worse state than he now is; worse with half his disease than he now is with the whole of it. The pulse would begin to flutter, the complexion would become dusky, and the lips blue, and the surface of the body mottled and patched in consequence of the blood being here and there unequally distributed or partially detained. The ventricle reduced to its common bulk would want the power needed to impel the blood steadily onwards against an extraordinary obstacle." (pp. 296-7.)

Now it appears to us that Dr. Latham's love of generalization and antithesis combined has betrayed him into the utterance of an opinion which, taken as it is uttered, stands in need of material qualification. He makes no distinction of the valve diseased, nor of the nature of the disease with which it is affected: the coexistence of systolic murmur and hypertrophy of the left ventricle is the condition spoken of. Now such a murmur may

be aortic obstructive or mitral regurgitant; and it is obviously only when it is of the first kind that the occurrence of hypertrophy can conduce to the patient's well-being in the manner spoken of by Dr. Latham. For the additional power acquired by the left ventricle can only have the effect of increasing the vigour and amount of regurgitation at each systole, and so increasing all the evils which follow in the wake of insufficiency of the mitral valve, when that is the valvular affection present.

LECTURE XXXIV. Here is presented a general view of the secondary diseases which proceed from an unsound heart, and an outline of their treatment. The vast extent of their pathological range is pointedly insisted on, and the conditions limiting and enlarging the expectations of medicine in different cases duly weighed. Congestions, effusions, hemorrhages, and inflammations, are in general terms the affections which arise, and which arise in various parts and textures, under the influence of cardiac disease. Now the author derives, from his examination of the curability of these various secondary affections, a certain number of propositions, and as these propositions comprise the marrow of the whole lecture, we cannot do better than reproduce them here:

"1st. That the fact of congestions, or effusions, or hæmorrhages, or inflammations having their actuating cause in an unsound heart prohibits the possibility of their cure in *the highest sense*, and limits the expectations of medicine to their suspension, their abatement or their temporary removal.

"2dly. That the form of unsoundness in the heart furnishes a measure of calculation, how much medicine will probably be able to effect in the individual case, whether it will go to suspend or to abate or even so far as to abolish them for a time.

"3dly. That the presence or absence of coincident disease in other organs always modifies those expectations of relief by medicine, which the mere form of unsoundness in the heart itself would lead us to entertain.

"4thly. That constitutional plethora and anæmia are grave contingences when they are found in coincidence with an unsound heart, having an important bearing upon its consequences and upon our modes of treating them and upon our expectations of giving relief.

"5thly. That, besides all these intrinsic conditions, the extrinsic accident of what may be the patient's circumstances in life throws a great weight into the balance for or against the probability of relief. The man, who, having an unsound heart, must traffic with his sinews, for his daily bread, has a poor chance of benefit from medicine.

"6thly. That treatment has never more need of being favoured by opportunity than it has now." (pp. 316-17.)

LECTURE XXXV. Dr. Latham proceeds to take a more particular view of the secondary diseases springing from unsoundness of the heart. And the lungs are the organs first considered,—the considerations presented exhibit no actual novelty, but important truths, which cannot be too often or too seriously urged on the attention of the physician, are put in that striking form that cannot fail to arrest and stimulate thought.

LECTURE XXXVI brings us to the subject of atrophy caused by cardiac disease,—the treatment of which will "be simplified by keeping in mind two points,—1st, the relief which is sought by the effusion of serum; yet, 2dly, the mischief which actually follows its accumulation. Rather a subtle refinement, however, perhaps you will say, thus to claim a character of good in its design for that which inevitably terminates in evil! It is like taking a *salutary* leap into perdition."

Dr. Latham sees antitheses in things as well as employs them in words; here is a remarkable and instructive illustration of the fact:

"Among the various ways in which the fortunes in life of those, who are the subjects of an unsound heart, can modify the expectations derived from the intrinsic conditions of their disease, there is one which is very remarkable. In those who are well-off in the world, dropsy seldom arrives until the unsoundness of the heart has reached the point, at which the circulation can endure its oppression no longer without seeking relief by effusion of serum. But in those who are ill-off, dropsy often appears long before the unsoundness has reached the point at which it would naturally and necessarily take place. In the first, the injurious operations proper to the heart's unsoundness are less accelerated by accidental circumstances, and so its evil consequences and dropsy among the rest are found to tarry, until it has itself become absolutely great enough to produce them. In the second, its proper injurious operations are unavoidably quickened day by day and every hour of the day by accidental circumstances; and so a smaller disease in one case is more felt than a larger disease in the other, and its evil consequences and dropsy among the rest arrive sooner. We may well be surprised at the complete success which occasionally attends our treatment of the vast dropsical accumulations, which accompany an unsound heart. But in such cases it will be found that, but for the patient's unfortunate circumstances, there would have been no dropsy at all, the affection of the heart being not yet ripe for it." (pp. 351-2.)

Hereupon Dr. Latham tells two clinical stories, concerning "a young man" and "a little boy," who had both cardiac disease, but of whom the one recovered and the other died. But we do not see that the point sought to be illustrated by the contrast of these two cases derives any very distinct elucidation from the faint outline given of their particulars.

Lectures XXXVII and XXXVIII close the book with an account of angina pectoris. Angina pectoris is the name for an affection known only by its symptoms, and which Dr. Latham would define thus: "Pain of extreme severity passing through the chest from the sternum to the spine, arising suddenly and ceasing suddenly, and accompanied, while it lasts, with a feeling of approaching death." Dr. Latham seems to have actually seen but two fatal cases of angina pectoris; in both these organic change was found in the heart. It is well known that the theory of ossification of the coronary arteries has been frequently (in proportion to the absolute frequency of angina pectoris), proved to fail utterly: angina has killed persons with sound coronary arteries, and individuals, whose coronary arteries were as unyielding as bone, and seriously obstructed, have died of other diseases, without ever having had an attack of angina. The victims of this cardiac anguish have been found to have dilatation of the aorta, valvular disease, hypertrophy or atrophy, softening or fatty degeneration. And it has killed where "no form of disease or disorganisation whatever has been found, either in the heart or in the blood-vessels nearest to it." Hence, then, in the author's words: "we are sure of what angina is as an assemblage of symptoms; we are not sure of what it is as a disease."

And Dr. Latham's notion of the intimate nature of the affection appears in the following statement, wherein it will be perceived that he adopts the original doctrine of Heberden.*

* By the way, how came Heberden to have seen *upwards of a hundred cases* of angina? Either he must have sometimes erred in the diagnosis of the affection, or the hearts of our population have wonderfully changed since his time. Is there a living man now who has ever seen *thirty true* such cases, no matter what his age, what the extent of his practice, or what the peculiarity of his opportunities for the observation of cardiac diseases?

"'Spasm,' it has been said, 'is a mode of action in muscular structures different from, or beyond, the natural and accustomed mode.' The natural actions in all muscles, voluntary and involuntary, are unaccompanied by any conscious sensation whatever. But spasm is always accompanied with pain. And pain and spasm, wherever they are, disable the parts which they befall. Colic stops the peristaltic movements of the bowels. Cramp forbids the hands to handle, and the feet to walk. But the heart is a muscle, and its functions flow from its attributes as a muscle. Now we are in search of something in the heart which, as the concomitant of pain, may be disabling to its natural functions, and capable, according to its degree, of hindering or abolishing them altogether. This we find in spasm. In its spasm of smaller degree the heart fails to close freely upon the blood and to impel it freely into the arteries. In its spasm of greater degree it fails to project it altogether. Herein we discern an adequate explanation of the chief phenomena of angina pectoris. It is a spasm of the heart." (pp. 385-6.)

In respect of treatment of the paroxysm there are two things to be considered, the sense of dissolution and the pain. The first is to be relieved by those stimulants known to be most rapid in their effects,—and Hoffman's æther and the spiritus ammoniæ, in a drachm dose, are those recommended by Dr. Latham. Now pain is not always to be treated at all, because it passes away with the spasm, and is so short-lived that no remedy would reach it in the time. But in cases when the pain continues for a longer period than usual, a quarter of an hour or more, then opium (a drachm of laudanum) must be given with the æther.

And now must we bring to a conclusion our notice of these Lectures; the series now reviewed amply sustains the reputation of the first; and to set down our general opinion of their merit, would be but to repeat the strong eulogia with which we conveyed our impression of the signal excellence of the former volume.

Before we finally lay down our pen, we must write a few more words on the literary character of this work. In our notice of the first volume, we gave to Dr. Latham's style the highest general commendation, while we made some exceptions to its antithetical quaintness and mannerism. The present volume is exactly like its predecessor in every respect,—indeed, too like; as it is impossible not to feel that, with all its excellencies, the style is rather artificial and monotonous, and therefore somewhat tiresome. At the same time, we must add, that it is only when regarded in relation to the general high cast of composition in these volumes, that Dr. Latham's style can be pronounced even partially defective. Compared with that of the great majority of medical books, the language appears almost perfect, and compared with the very best, it may fairly contend for the first place. The language is, indeed, throughout—not merely in every chapter or page, but in every sentence—faultlessly accurate, and at the same time, polished in the highest degree. In this respect, and, indeed, in other respects also, the volumes before us are well entitled to the name of classical; and they will, we doubt not, take a place among the very small number of medical books that will be prized and read by men of other generations.

Art. XIV.

Practical Observations and Suggestions in Medicine. By Marshall Hall, m.d., f.r.s., l. & e., &c. &c.—*London,* 1845-6. First series, 8vo, pp. 360; Second series, 8vo, pp. 360.

In our Number for April, 1845, we noticed a volume of 'Observations in Medicine,' by Dr. Hall. We *noticed* it merely, and out of kindness to the author, because we felt convinced that an impartial analytical and critical review of the volume, must necessarily detract from the reputation already acquired by Dr. Hall. The appearance of another and a similar volume recalls us to a juster sense of our duty,—reminding us that there is something due to the character of British medical literature and science, and that a tenderness for the faults of an able man ought not to render us disloyal to truth. We therefore now propose to review these 'Observations' more in detail.

In our previous notice, we said that a man with the reputation of Dr. Hall could not afford to bring out an inferior book, for his own sake: we now say that, for the sake of others, he must not be allowed to bring out one inferior book after another, unless a just estimate of them is laid before the profession at the same time. Example in high places is as potent for ill as for good; and the name of Dr. Hall may make that pass current which would be at once rejected under a lesser warranty. In criticising the volumes before us, we shall distinctly state the grounds of any opinion we may express regarding the author's merits; and, doing this, our readers will themselves be able to criticise the critic.

The author's objects in publishing these volumes would seem to be various; they have all, however, a reference either to the student or the practitioner. In the first place, Dr. Hall evidently desires to set himself up to both as an example and a teacher. This purpose discloses itself in the very beginning of the book. Among the "introductory remarks" of the first volume, in the section on "fertility in remedial resources," as a requisite in an able physician, we have the following:

> "If the following 'Observations and Suggestions' prove that I have not cultivated *this* subject entirely in vain, and lead other physicians into the same useful train of mental culture, I shall be amply rewarded for the pains which I have taken in embodying them in language and laying them before the profession.
>
> "I have published these 'Practical Observations and Suggestions' in an unpretending form: first, that they may appear in the modest manner becoming such a trifle; and, secondly, that they may be readily portable; for my ambition is, that they may become the companion of the student, and of the country practitioner, in some of his lonely drives, especially of those whose time is too much taken up by practice to admit of their reading larger and denser volumes.
>
> "I have also wished not to deter, by the form of this work, the general reader, who may be interested in medical matters generally, or in some particular medical subject." (Vol. i, p. 3.)

Another object appears to be, the promulgation of the author's therapeutical experience, or the communication of methods of treatment already found useful by him—such as the daily scarification of the gums in infancy, the alcoholic lotion in phthisis, &c.; or of hints or suggestions of

treatment derived from analogies, or from physiological ratiocination, as the cold douche to the loins in sterility, the cure of vascular nævus by puncture, &c.

A third object of the author is the republication of essays formerly written on various subjects. Of this kind are the chapters on "intestinal irritation," "exhaustion from loss of blood," "the sinking state from various causes," "on hybernation," &c.

A fourth object is to make the work a sort of receptacle for the lucubrations of friends, and for their hints, observations, and suggestions. When we consider how freely open the numerous weekly and monthly journals are to the reception of communications of this kind, we think this altogether unnecessary, and at variance with the other objects of publication. The observations we allude to are certainly *not* "Observations by Dr. Marshall Hall."

Dr. Hall's volumes are also "intended to be suggestive of new observations, as well as the record of some observations of his own;" and, in giving these suggestions for new observations, the author indicates special objects of inquiry, propounds the principles of observation that should be adopted, and lays down the plan that should be followed. The subjects indicated for observation are detailed in vol. ii, p. vii.

The following extract exhibits the principles propounded by Dr. Hall as the guide to students and practitioners in conducting the suggested observations:

"I am persuaded that physiology, or a knowledge of the healthy actions, is the only foundation for practical medicine,—and the only remedy against the hydra, quackery, now so prevalent, both in and out of the profession. This physiology should be at once experimental and clinical. The biblio-physiology of the day can issue in no good whatever. The medical mind wants discipline; we should study the works of Harvey and of M. Louis; experiment, and observation, and philosophy should go hand in hand....... When every member of our profession has a sound knowledge of physiology, derived from his own cautious observation, and not, at second and third hand, from books, medicine will take its just position, and quackery its departure. But whilst a medical person can be found to boast that he, forsooth, is a 'mere practical man,' quackery will continue. One kind of mere empiricism is as good as another." (Vol. ii, pp. v, vi.)

These principles of clinical observation we cannot object to, as they are almost the same as some which were advocated in the correspondence elicited by an article published in this Journal last January, and particularly in one of the letters addressed to the editor in the Twenty-first Volume. We are happy to find that these views, although omitted in the first volume, find so prominent a place in the second; and we are the more gratified, because we can bring Dr. Hall's "purely useful and practical" observations to the test of his own avowed principles without any demur on his part. It is rather pleasing, too, to find our author laying down a "plan of observation of diseases of the nervous system," prefaced by this declaration: "If we wish to pursue the subject of *clinical* observation in general, we have fortunately a *perfect* model and example in the writings of M. Louis."

The value of the new therapeutical experience recorded by Dr. Hall varies extremely. As an example, we will take the first practical point in the first volume, and we think it will not be difficult to show, that the

observations recorded in that chapter have not been made under the guidance of physiology, but that they are in absolute contrast to the "*perfect* model and example of M. Louis," whilst the whole chapter displays, as we shall subsequently point out, a spirit and aim very remotely related indeed to the lofty aspirations of the author. The chapter is headed, "On the use of the alcoholic lotion in phthisis pulmonalis."

"So many persons affected by incipient phthisis, marked by dulness of sound on percussion, and no doubtful pectoriloquy under the clavicle, hæmoptysis, and disposition to chills, heats, and early morning perspirations, &c. have been benefited and restored to apparent health by the remedy, or remedies, which I am about to mention, that I cannot but think they possess great efficacy. The first and the principal of these remedies is an alcoholic lotion constantly applied by means of six folds of linen over and across the upper lobes of the lungs. One part of pure alcohol is mixed with three parts of water. It is applied tepid at first, afterwards of the temperature of the atmosphere. It is applied, in *small* quantity at a time, every *five* minutes, so that the application may always consist of alcohol and water." (Vol. i, p. 25.)

On the next page we find the alcoholic lotion gets all the credit; but still Dr. Hall's practice is successful.

"It is by no means my wish to laud this remedy beyond its just value; but I have no hesitation in asserting that it possesses a power in checking the progress of the deposition and softening of tubercle in the lungs, beyond any other which I have ever tried. And the number of patients who have recovered from incipient phthisis under its use, and who, after many years, are still living, and in apparent health, induces me to express myself in strong terms in regard to its extreme value." (Vol. i, p. 26.)

Then follows the list of cases, much in the style of those which usually are found to clench quack advertisements: for example—

"One patient, who consulted me fifteen years ago, had dulness on percussion and pectoriloquy, and every other sign of incipient phthisis. He applied, and long wore, the alcoholic lotion, called it his 'breast-plate,' and is now a professor of ——— College.

"A lady, about 30 years of age, became affected with hæmoptysis, and displayed the physical signs and the usual symptoms of phthisis. She was enjoined the alcoholic lotion. It is fourteen years since it was first applied, and it is continued, or renewed, if ever suspended, to this day.

"I saw a young lady two years ago, one of a most consumptive family, affected with hæmoptysis, and with every threatening sign and symptom of incipient phthisis. I prescribed the alcoholic lotion, and the cough and hæmoptysis were removed, and every fear dispelled. It had already been proposed that this young lady should take a voyage to Madeira. She did so, continuing the lotion, and returned in apparent good health." (Vol. i, p 27.)

Now, if these general statements mean anything, they mean that certain individuals labouring under phthisis pulmonalis were *cured* by the use of this alcoholic lotion. They mean this, and nothing less. But an indistinct idea seems to have passed through the author's mind, that his *professional* reader might bring some common sense to bear on the subject, and demur to the facts. We therefore have him stating, "I do not imagine that the alcoholic lotion does more than *check* the morbid processes." Nay, he goes further; he even doubts whether it even effects this:

"In what the morbid processes of the deposition and the softening of tubercle

consist, I believe we do not know; but if these processes be really checked by the application of the alcoholic lotion, we have a *practical* fact which must excite the deepest interest. Some degree of this influence, in incipient cases, is, I believe, exerted by this remedy." (Vol. i, p. 28.)

What, then, is the therapeutical experience in this chapter? Firstly, Dr. Hall has no hesitation in *asserting,* that the alcoholic lotion possesses the power of checking the progress of the deposition and softening of tubercle; nay, "a number," and "so many," have been "restored" by it from what he calls "incipient phthisis," but which we must call confirmed phthisis, seeing there was "dulness of sound and no doubtful pectoriloquy under the clavicle"—that he cannot but "express himself in strong terms in regard to its extreme value;" and then, on the next page, all this is softened down into "*if* these processes be really checked by the alcoholic lotion," and "*some* degree of this influence is, I *believe,* exerted," &c. *I believe!—some!—if!*—are these the terms to use regarding a remedy for an incurable disorder after "*I have no hesitation in asserting?*"

We have scanned this chapter carefully, hoping to discover the slightest hint of the numerical method, or the smallest modicum of physiological science. We trusted that there might be at least one line which would indicate, however so obscurely, the physiological principles which had led Dr. Hall to the use of the remedy, and which would elucidate its *modus operandi.* But the result of our most careful analysis is, in the language of the chemist, *not a trace.* As for the "perfect model and example of Louis," we need only observe, that they are just as utterly wanting as the principles of physiology. Even the language is singularly loose and indefinite. For example, the folds of linen are ordered to be applied "over and across the upper *lobes of the lungs:* a mode of wording, moreover, as if it were meant to indicate the rationale of the practice. To say nothing of the next-to-impossible feat of applying the remedy "every five minutes,"—yet one lady seems to have done this for fourteen years!—not a hint is given as to how the influence of the alcohol reaches the morbid tissues, and when there, how it effects the beneficial change. There is not, we repeat, the slightest attempt at a physiological or numerical consideration of the nature of tubercle, or of the means by which the deposit may be prevented or removed. Yet, surely, if the "*perfect* model" and example afforded by Louis be ever worthy imitation, it is in investigating the nature and cure of phthisis pulmonalis.

There is a chapter "on the treatment of lateral curvature of the spine," and here we made sure of physiological observation. The pathology of spinal muscular action is so manifestly an important branch of the "true spinal" doctrines, that we were warranted in fully expecting from Dr. Hall an application of those doctrines to the pathology and therapeutics of lateral curvature. What Dr. Hall has really done shall be here stated:

"The plans for the treatment of curvature of the spine which I propose, have three objects in view:—The first is the restoration of the natural form. The second, the renutrition of the weak and emaciated muscles. The third, the restoration of the health and vigour of the general system. I propose to accomplish my first object by stays applied to the *curvatured* form, so constructed as to

give the most perfect support without inducing the least injurious pressure, either on parts too protuberant, or on others, as the axilla or the ilium, taken as points of support.

"Let the patient be artificially made to assume the straight or perfect form by means of posture, stretching, and pressure; under these circumstances, carefully preserved, let a cast of the bust be taken in plaster of Paris; from this cast let a mould in wax be taken; lastly, on this mould let stays of steel be accurately fitted (or let copper be deposited by means of the electrotype and sawn in two vertically). This is to be covered and lined with soft leather and slight wadding, and fixed and worn in the ordinary manner, drawn to the proper degree of tightness.

"It is obvious that, these stays being put on and fastened in the recumbent posture, the bust *must* retain the perfect form, although the patient may now resume the erect position; that no partial or injurious pressure will be made on any part; and that there is neither any constrained position, nor any obstacle to the due use of the muscles." (Vol. i, pp. 55-6.)

All this is apparently theoretical: if Dr. Hall can produce the cases in which he has so applied the stays, and successfully, we trust that he will do so. A reference to the general practitioners in attendance—whom he is always so careful to name—will suffice. The next step in the treatment is thus described:

"In order to accomplish the restoration of nutrition in these atrophied muscles, something more must be done; and this remark leads me to a second proposition for the treatment of these distressing cases. It is that of inducing, by means of rubbing, what I will designate *counter-muscular effort*.

"If, whilst the patient is sitting perfectly erect and unsupported, we press on any given point along the spine with the finger, every [?] muscle situated *below* the point of pressure is necessarily [?] called into a state of action, which (action and reaction being equal, but in contrary directions) is, in degree, commensurate with that of the pressure, unless, indeed, the patient *yields* to the pressure. In this manner, any one of the numerous muscles situated on either side of the spinal column may be called into action *at will*, [?] and this by means of the very friction which was formerly used in a form that may be termed *inert*, and in a degree proportionate to the *pressure* with which it is applied.

"The patient being placed unsupported in the erect position, the hand or hands are to be passed along the muscles of the spine, pressing, at first very moderately, then more and more firmly, whilst they are carried upwards and downwards alternately, in the ordinary manner of rubbers. At every successive instant a fresh set of muscles is called into action more particularly, whilst the whole system of the spinal muscles is made to contract together, or in their turn." (Vol. i, pp. 58-9.)

Dr. Hall would have his readers infer a therapeutical novelty here; it is, however, nothing more than the old method of shampooing, kneading, &c. combined with gentle friction: friction alone is rarely, if ever, recommended by professional men. The physiological novelty is in the explanation of the *modus operandi*, which is, in truth, a most improbable hypothesis, namely, that on pressure being made on any given point along the spine, action takes place in every muscle situate *below* the point of pressure in *a degree proportionate to the amount* of pressure. In the treatment of curvature of the spine it is necessary to anything like success, to ascertain the exact muscles implicated in the distortion; then the nature of the morbid muscular action; and then the causes. Distortions of the bones, joints, or muscles usually, if not always, take place from unequal muscular action, consequent upon either disease of

the bones to which the muscles are attached, or upon paralysis or spasm of the muscles themselves. The antagonism between the opposing muscles, either of the two halves of the body or of the two sides of a limb, is destroyed. We have obvious examples of this in distortion of the face, in wry-neck, in club-foot. Now, no one ought to know better than Dr. Hall, that this interruption of equilibrium of muscular action may arise from two opposite conditions of the muscles implicated. Firstly, there may be spasmodic action, as in spasmodic tic douloureux, and the distorting traction is in the direction of traction of the affected muscle. Secondly, there may be debility or paralysis, and then the healthy unparalysed muscles drag their movable attachment in the direction of their traction, as in facial paralysis. But there may be a third condition, namely, that in which there is spasm of the one set of muscles, and paralysis of the antagonizing set. The diagnosis in these several examples must often be difficult, and there could be no greater service rendered to practical medicine than an easy method of diagnosis, founded on just physiological data. When, for example, in strabismus, is there spasm, and when paralysis? or when the one condition or the other in facial distortion, or in spinal curvature? If we trace the different forms of spinal distortion downwards, we shall find the first form to be that in which the recti antici and longus colli seem to drag the occiput and atlas forwards, causing a corresponding depression in the nuchal region, and a projection in the lower cervical. In torticollis, sometimes one half of the trapezius is implicated, and always the sterno-cleido-mastoideus. Coming to the thoracic region—the more ordinary seat of scoliosis or lateral curvature—it is to be supposed that as the sterno-mastoid and trapezius were the distorting agents above, some of the same class—the respiratory class—of muscles will be the agents in the distortion below, either as healthy, unantagonized muscles, or as being spasmodically affected. It is maintained by some pathologists, that in scoliosis, or lateral curvature, the external respiratory muscles are those most usually affected, namely, the trapezius, sterno-cleido-mastoideus, levator anguli scapulæ, rhomboidei, scaleni, and the serratus magnus. Now if these be the muscles affected in ordinary cases of scoliosis, the pressure on the spinal muscles recommended by Dr. Hall, can be of no possible benefit, even if it be fully granted that shampooing, so performed, excites the sub-lying muscles proportionately to the pressure. But on what grounds does Dr. Hall assert that such a result takes place in the atrophied muscles of the spine? No doubt slight muscular contractions may and do occur; and we will give Dr. Hall the full benefit of this admission, and of his views as to the class of muscles affected, simply for the sake of asking him *which side,* in scoliosis, should be pressed and rubbed? for it is evident that if *both* sides be equally excited, the healthy, strong muscles will acquire increased bulk, at least, *pari passu* with the paralysed or enfeebled, and there will still be that unequal muscular action which originally caused the deviation of the spinal column. We will say nothing of the distortion which takes place from unequal action of the two latissimi dorsi, and of the muscular masses on each side the lumbar vertebræ, and attached to them and to the sacrum.

Dr. Hall finally asserts, respecting his plan of treatment—

"By the stays already described, the morbid extension is removed from the enfeebled, atrophied, and stretched muscles. By the friction and the counter-effort of these muscles they are nourished and strengthened. By both these means they are rendered efficient in their office of supporters and movers of the spinal column and the head." (Vol. i, p. 59.)

Now, we venture to affirm—1. That the stays, as recommended by him, will and must press injuriously, because, being inflexible, they cannot be adapted to the flexible spine ; being inelastic, they cannot be adapted to the varying tension of the muscles. 2. That in the majority of cases of lateral curvature, the deviation is caused by the morbid action of varying sets of spinal muscles : in some cases, the external respiratory, or thoracic ; in others, those of the dorso-lumbar region ; and, consequently, that the hap-hazard shampooing recommended by Dr. Hall is, theoretically, of doubtful value. 3. That as the method has been repeatedly tried with so little success, we shall be not a little surprised if Dr. Hall can prove that the results above described by him as consequent on his plan of treatment ever occurred, even in a single case.

We will take one other of these practical papers, and we select the chapter on aphoria, or sterility ; to which we will confess that we were attracted because it concludes with one of those random and ridiculous claims to discovery and novelty in which Dr. Hall indulges beyond any other man.

The pathology of aphoria is a subject on which a sound physiology only can throw light ; and Dr. Hall accordingly observes, that it is by a knowledge of the physiology of conception, and of the causes that most probably influence that function, that we can be led to a suitable method of cure.

We believe that the doctrine of *ovarian* influence and sympathy, as opposed to the old doctrine of *uterine*, is now well established. It is certain, moreover, that the relations of the ovaria to the uterus, and to distant organs in connexion with the reproductive system, as the mammæ, are much better understood, and much more strictly defined. It has been amply proved that the ovaria are the essential organs of repoduction, and the point of departure of the greater proportion of those sympathies which have been usually attributed to the uterus. Thus it has been found, that extirpation, or non-development, or atrophy of the ovaria, is followed by a corresponding change in the characteristics of the sex, inasmuch as the uterus becomes atrophied, menstruation ceases, the mammæ waste, the subcutaneous fat disappears, the skin becomes harsher, the voice hoarser, and in some examples of a peculiar condition of the ovaria, even the characteristics of the male are developed. But the uterus may be extirpated or non-developed, and none of these changes occur, provided the ovaria are unchanged in structure or function. Even a secretion analogous to the menstrual fluid will occur at the regular periods, apparently from the vagina. The phenomena of menstruation specially have so close a causal relation with the ovaria, that it is probable no functional change in that secretion takes place without the agency, direct or *indirect*, of the ovaria. Structural changes, as ulceration, fibrous tumour, or scirrhus, have of course their own proper influence.

Now, is it not most certainly to be expected that a physiological physician, with pretensions like those of Dr. Hall, should at least convince his readers that he was fully acquainted with the whole physiology of conception, in discussing the pathology and treatment of barrenness? But what is the fact? This: that the whole of his chapter on this subject is written as physicians wrote a century ago—or rather is not so well written.

"Sterility, doubtless, frequently depends on organic defect; but the fact that a first child has been born after many years of marriage is a sufficient proof that, in other cases, the defect has arisen from causes of a functional and less permanent nature. Of these, a too excited condition, and the opposite state of inertia in the immediate uterine system, appear to me to be the most frequent. The former case is illustrated by what is observed in dysmenorrhœa; the latter, by a condition of the uterus, frequently attended by leucorrhœa. They may be designated aphoria tonica, and aphoria atonica, respectively." (Vol. i, p. 150.)

The treatment is deduced from empirical conditions. The families of the laborious poor are numerous and tend to augment the population, whilst those of the indolent and luxurious rich continually tend to become extinct. The conclusion, as Dr. Hall observes, is obvious; "as a remedy for aphoria, much exercise should be taken, even to fatigue, whilst the diet should be moderate, not to say, spare."

But surely such mere empiricism as this, or such as the recommendation of extreme moderation in intercourse, required no preliminary flourish about the "physiology of conception." The treatment is deduced from experience alone, and would be deduced just as certainly if Dr. Hall were in profound ignorance of the very existence of the ovaria and uterus. The poor multiply; if the rich wish to multiply, let them live like the poor. If they take humble fare and work hard, they will then digest well, sleep well, and have neither time nor inclination to waste their hours in enervating dalliance. But the advice is just as applicable to the rich *man* as the rich woman; for impotence, we suspect, is almost as common as sterility.

"But I proceed to another and most interesting view of this subject. There is an extraordinary reciprocal sympathy between the mammæ and the uterus, which has always presented an object of deep interest to the physiologist. At every catamenial period, the mammæ sympathise with the uterus, become tumid, and tend to assume their office of organs for the secretion of milk. But the sympathy is not only seen in the affection of the mammæ, by the condition of the uterus, but, though somewhat less definitively, in the inverse order, and the state of the mammæ influences that of the more immediate uterine system. In general, though by no means invariably, the catamenia are suspended and conception is postponed by the process of lactation." (Vol. i, pp. 151-2.)

Dr. Hall must admit that this "extraordinary reciprocal sympathy" is, at least, the most *ordinary* every-day occurrence. It is, in fact, in one sense, only just as "extraordinary" a circumstance as that women conceive and bear children. But this is only one of a hundred examples of the inflated style in which Dr. Hall refers to common physiological facts. It is, however, something extraordinary that Dr. Hall could write the significant words, "the catamenia are suspended and conception postponed by the process of lactation," and yet be, as it would seem, unaware that the facts he mentions demonstratively prove the true sympathy to be between the mammæ and ovaria. After quoting several examples in which

this connexion between the two organs had been rendered available in checking uterine hemorrhage, and another in which a maid servant induced the lacteal secretion by putting an infant to her breasts, Dr. Hall infers as follows:

"I would propose, then, that the patient should sleep or one week before and during each catamenial period, with an infant on her bosom." (Vol. i, p. 157.)

Now this remedy *might* possibly be serviceable in certain cases of sterility dependent upon atony of the ovaria; but not because there is sympathy between the mammæ and uterus, but because the irritation of the nipple is usually accompanied with pleasurable feelings, and might, therefore—especially when we consider the means—an infant's lips—act favorably on that portion of the nervous system from which the ovaria derive their tone. But there is not one word said by Dr. Hall as to the *diagnosis* of the cases of aphoria in which it would be useful.

Another remedy need only be stated in the words of Dr. Hall himself; criticism is scarcely needed. As the cold douche relieves inertia of the uterus and so relieves uterine hemorrhage,

"May not a means, obviously of such efficacy against inertia of the uterus, be effectual in those other forms of inertia of the uterine system on which atonic sterility sometimes depends? I would suggest its due administration immediately after intercourse. The actions which lead to conception may be of the excited class. Contraction of the uterus may be excited, and be followed by relaxation and ingurgitation." (Ibid., pp. 157-8.)

And there *may* be people so simple as to try the remedy. A douche of cold water on the hypogastrium "immediately after intercourse!" Dr. Hall is safe here, at least, as to his physiology; the cowleech and stallion-keeper will certainly lay claim to "complete anticipation," as to practice. Here follows the *Io pæan:*

"I trust that it will be admitted that new applications of physiology to practice have been made in this paper; and, first, that the sympathy between the immediate uterine system and the mammæ is both nervous and vascular; and, secondly, that the principles of the reflex actions have been shown to have a practical bearing not previously noticed." (Ibid., pp. 158-9.)

We might multiply examples like the preceding, from the practical papers, as Dr. Hall wishes them to be considered, but we will only say *ex uno disce omnes.* We will therefore turn to his physiological philosophy, and try if we can find anything more satisfactory on his own favorite ground, the nervous system. And here we are sorry to have to make the preliminary remark, that in the numerous papers on this subject in the two volumes, we have almost always to complain of indefinite terms, obscure meaning, and cloudy views. For instance, often as he employs the terms "cerebrum," and "true spinal marrow," he never once defines them in these volumes. A reference (and a troublesome one too, as we know by experience) must be made to previous works for a fundamental proposition which might have been stated at most in two lines.

We have felt embarrassed in selecting a physiological chapter for examination. Many of them are simply repetitions of the ideas already promulgated in the 'Memoirs,' the 'New Memoir,' the 'Lectures,' and the

'Oration,' that Dr. Hall has writ or spoken from time to time; ideas expressed in words full of promise to the ear but frustrating the hope; always going to end in something grand; never soaring from the small circle within which Dr. Hall seems spell-bound. Perhaps some ideas on the nature of sleep will show as briefly as any the easy off-hand style with which Dr. Hall throws out wild suggestions and hints to the rising class of physiologists, and in the same breath adopts the hints as proven verities.

"Little or nothing is yet known of the immediate cause of sleep. I am of opinion, and I shall have to repeat the observation, that a state of contraction of certain muscles of the neck takes place, analogous to that of the orbicularis palpebræ, as sleep comes on; that certain veins are compressed; that congestion of the brain takes place; and, lastly, as a consequence of this last, *sleep*. A similar event takes place for a moment or two, in some cases, inducing that short oblivion, or epilepsy of which Heberden gives so just a picture." (Vol. ii, p. 27.)

It will readily occur to the reader to suppose that Dr. Hall undertakes just to name the muscles that contract and compress the veins, and indicates the veins that are compressed. But he does not. They are "*certain* veins" and "*certain* muscles of the neck." But perhaps he distinctly shows that contraction of muscles actually takes place previously to sleep or a fit of epilepsy? He does not. Then, at least, he proves that there *is* congestion of the brain previously to sleep? No. He stops not for such trifles, but shows that epilepsy, being a disease of congestion, is allied to sleep. He assumes the theory to be proven. Further, as he promised, he repeats the observation:—

"Certain muscles may be muscles principally of excito-motor action; and when volition is withdrawn from the other muscles of the neck, *they* may contract like the orbicularis, and gently compress the jugular veins, and so induce congestion of the brain and sleep—and, as we often observe, attacks of apoplexy and of epilepsy." (Ibid, p. 35.)

But we must not conceal, however, that with regard to this hypothesis, Dr. Hall expressly observes that it "is a mere conjecture, to be accepted for what it is worth, and a mere foil." Then, we say, it is worth just nothing.

But may not a man be allowed to publish a few conjectures? Unquestionably; nevertheless when a prince of philosophers, as Dr. Hall assumes to be, so deeply, too, "imbued with the Baconian philosophy" condescends to conjecture, we have a right to look for something different from this. Volition being withdrawn from certain muscles of the neck, other certain muscles are then allowed to contract and compress veins and cause cerebral congestion, and then sleep comes on. But had not sleep *already* come on when volition was withdrawn from the aforesaid muscles? or, if not, will Dr. Hall gratify his readers by stating from *which* muscles of the neck they must withdraw volition as a preliminary to a good night's rest? An elephant supports the earth, and a tortoise the elephant, but on what does the tortoise stand? Dr. Hall is a veritable Brahmin in his power "fingere hypotheses."

We might multiply examples of this kind; but those we have cited already sufficiently show that Dr. Hall is not able to carry into practice

the principles of research he lays down, nor to imitate the "*perfect* model and example" he holds out as a pattern to others.

We are gratified, however, in being able to allow that Dr. Hall has shown, in some of the practical papers, a certain degree of diagnostic and therapeutic acuteness. The statements respecting the uses of enemata of cold and warm water, although not new, are well put. The method described for the regulation of the temperature of a sick room is good, inasmuch as it recommends simple means for the purpose. One important objection, however, occurs to us, viz. that the ventilation of the apartment would be somewhat defective. The precautions necessary to be observed in the treatment of tetanus and hydrophobia have a sound physiological basis, and merit attention; for too much care cannot be given to the prevention of paroxysms, by the exclusion of irritants to the nervous system. We agree with Dr. Hall in thinking that cataplasms are useful in pleurisy—and we have often used them—but not because the atmospheric air is excluded from the skin (as Dr. Hall imagines it to be), but probably because of the action of heat and moisture; there can be, however, no analogy between the treatment of burns and that of pleurisy, with reference to the exclusion of atmospheric air; it is a quaint conceit to think there is. We also think the remarks on the causes and prevention of apoplexy have considerable practical value.

There are many more small practical hints and modes of treatment derived from empirical observation or instructive analogies, which we might go on enumerating. Physicians and practitioners of experience abound in such hints and suggestions, but they do not generally think it necessary to make a book of them, or trumpet them to the world as discoveries. Those of Dr. Hall may be allowed to prove that he is an acute, diligent, and thoughtful physician, but he will be surprised to be told that they do not prove that he is either a Harvey or a Sydenham.

We wish we could, in justice, here hold our pen; but there are two or three things so prominently conspicuous in the manner and style of the volumes before us, that our regard for the dignity of the profession forbids us entirely to overlook them. The first of these which we shall notice, has reference to the singularly dogmatical and boasting tone which the author constantly assumes. As illustrations of this we might refer the reader to numerous passages in the volumes: we shall here only extract the following:

"And here, in reference to my own labours, I trust my readers will allow me to make a very few brief remarks, without imputing to me any other wish than that of doing myself an act of mere justice.

"I may truly affirm that mine has been a really active professional life. In the midst of practice and of lectures, I have allowed few days to elapse without recorded observation. This habit I regard as the *test* of a physician's steadiness and industry. But, besides this, for ten years I devoted a part of my leisure hours—often snatched from hours which should have been devoted to repose—to physiology. My labours in this department have been rewarded by no less a discovery than that of *the Function of the Spinal Marrow, reflex* in its form, *excited* in its character, embracing *all* the *acts* of *ingestion* and of *expulsion* in the animal economy, and therefore all such acts in their relation to the preservation of the individual, and the continuation of the species—a function, therefore, of no mean or inconsiderable extent. To the physiology of the spinal marrow, too, must be added its pathology and therapeutics.......

"But of my various labours, in the course of medical practice, I look upon my researches in regard to the use and abuse of bloodletting, of all remedial measures the most powerful for good and evil, with the greatest satisfaction. I *know* that the Rule for the due administration of bloodletting, which I have adduced from those researches, is the daily means of saving valuable lives; of extricating the individual practitioner from many and most serious difficulties; and of protecting the patient from the dangers of the undue detraction of blood on the one hand, and the inefficient administration of an important remedy on the other.

"I think I may say, of the distinction between the cases of intestinal irritation, of the effects of loss of blood, of inflammation, and of the various *mixed* cases consisting of two or of all three of these, variously mingled together in the same case, pretty nearly what I have said of the Rule for the due and safe administration of bloodletting,—that it is one of great practical importance, and that it has proved the means of saving many dear and valuable lives. This is still more true when the diagnosis in question is confirmed or corrected, and the treatment regulated, by that very rule.

"I remember the time when sporadic puerperal diseases in general were regarded, without due discrimination, as inflammatory, and forthwith treated with lavish bloodletting, by which they were frequently rendered fatal.

"At an early part of my medical career, I published an Essay on this subject, of the practical value of which the late Dr. Baillie spoke in the highest terms. I believe this testimony to have been true. It has constantly been confirmed by those of my friends most engaged in the practice of midwifery, who have had at once the opportunity and the disposition to put my suggestions to the test of experience; and, after a lapse of twenty years, I have still the happiness of thinking that my labours have, in this instance too, had a highly useful result.

"After the rule proposed for the due administration of bloodletting, and the diagnosis established in regard to puerperal diseases, I would rank the observations I have made on that form of hydrocephaloid disease which arises from exhaustion, for their practical utility. I have seen, and do still see, little patients whose maladies are doomed to run a fatal course, under the erroneous idea that they are really hydrocephalus, rescued from their state of peril by a juster diagnosis.

"One other service which I think I have rendered the art of medicine, in its application to infants and children, is that which relates to the treatment of infantile convulsions." (Vol. i, pp. 3-7.)

We do not know what Dr. Hall's ideas may be as to the precise meaning of the words "modest" and "unpretending," by which he characterizes his own lucubrations; all we can say is, that there are many similar passages in the volumes from which we quote; and, in fact, the expressions used by the author, as to the value and merits of his own work, are, in our humble opinion, as far removed from the "modest and unpretending" as can well be conceived.

No doubt Dr. Hall would be shocked to find himself compared, in the remotest possible degree, to the detestable race of puffing charlatans, against whom he is so loudly and so justly indignant in many pages of the volumes before us: and heaven forbid that we should think of making the comparison—even if he had not told us, as he does in his preface, that "a physiologist could not, if he would, either be a quack, or disposed to quackery;"—yet it is painful to observe into what seeming approximation to the objects of his just detestation, morbid vanity and self-conceit can bring an honorable man like Dr. Hall. Like the veriest quack, he addresses to the "general reader," boasts of his industry, his knowledge, his discernment, his discoveries, his great cures. Dr. Hall's enemies —and he admits that he has many—might be disposed to attribute

the seeming polarization of meaning in these and similar passages to the desire to produce a reflex action, in a *practical* sense, on "the country practitioner in his lonely drives," or on "the general reader who may be interested in more particular medical subjects;" but we—whom probably, however unjustly, he may reckon among his enemies—are disposed to ascribe all such demonstrations entirely to the egregious conceit and vanity which constitute such a striking feature in Dr. Hall's mental constitution. Yet it must be admitted that it requires the exertion of some candour, and a constant reference to the author's solid merits and high position, to refrain from doubting whether many passages in these volumes were not indited with an aim not strictly scientific. On what may be called the hypothesis of reflex action just hinted at, many things seem readily explicable, which otherwise are puzzling; but with the key of conceit just adverted to in our hands, we find no difficulty with all the seeming allusions or real contradictions. In illustration, let us refer back to our extracts from the paper on Consumption (*supra*, p. 203). Here it might seem that the assertions and cases of *consumption cured* are made suitable to the "general reader;" the doubts as to the efficacy of the remedy and the reality of the cures, to "the lonely driving country practitioner;" but our key unlocks all the difficulties.

Another illustration of the painful peculiarities we are commenting on is to be found in the chapter on scarification of the gums during dentition. He recommends daily scarification of the gums, and even twice daily in urgent cases; "whilst there is fever, or restlessness, or *tendency to* spasm or convulsion." Fever *or* restlessness, or a tendency to !—such is the indefinite language in which Dr. Hall recommends this painful operation to the general reader, student, and "lonely driving country practitioner." The whole is concluded with a grand finale, an Io pæan of mighty success in treating infantile diseases:

"I do not pretend, in the above proposition, to have advanced anything new; but in the *locality* chosen for the operation, and in the *promptitude*, *repetition*, *perseverance*, and in the *energy and steadiness of purpose* with which I recommend the measure to be adopted,—if these be fully apprehended,—I believe I do propose something *new;* and when I repeat that since I adopted the plan of *effectually* removing *all* irritation in the gums, stomach, and intestines, in cases of crowing, and other convulsions of the same nature, early enough, I have not known or seen a fatal case, I am aware that I propose a plan of treatment at once new and *invaluable*." (Vol. i, pp. 33-4.)

The next chapter is an appendix to the preceding, and is on the nature and treatment of stridulous convulsion in infants. The pith of the chapter we found to be the tail-piece. It is a reference to a case published some four or five years ago. "No case could present a more marked diathesis of the convulsive character than this little patient." The results of treatment are stated in letters from the practitioner in attendance, whose name and place of residence are given in full (the two latter particulars we shall venture to omit):

"W—— R——, April 28th, 1842.

"Dear Sir,—I am sure you will be much gratified to hear that your plan of treating the disorder of Mrs. Grey's infant has been signally successful. ....

We have adhered rigidly to the rule prescribed since you saw him. . . . . . I continued the lancing of the gums twice a day until yesterday, when I only did it once, and the same to-day, as the gums, where divided, continue soft and open, and *slightly ulcerated*, as well as from the amelioration of the symptoms. . . . . I am anxious to have your advice by the bearer how I shall go on, &c.

"W—— R——, May 4th, 1842.

"Dear Sir,—Mrs. Grey's infant has continued to improve since my last report. I believe no spasm has been noticed; the different functions of the body go on in a healthy way. The treatment you suggested has been unremittingly carried out, excepting that, since I last wrote, the gums have been only lanced once a-day." (Vol. i, pp 44-5.)

We should like to hear what "the lonely driving country practitioner," with his strong common sense, would say to this rubbish; remarkably interesting it is to the lonely driver to learn that, with a healthy nurse, a good appetite, gentle laxatives, change of air, and "sea breezes," the different functions of the body of Mrs. Grey's infant went on in a healthy way; but more interesting we opine it will be to think that this agreeable ordering of the said functions of the body of Mrs. Grey's infant took place *in spite of* the lancing of the gums twice a day, until they were "slightly ulcerated!"

Next comes a chapter on the use of setons; and as usual, when ordered by Dr. Hall, never-failing success attends upon them. *Fiat setaceum,* uttered by him, is the password to health and ease. Example:

"In a variety of cases of acute or chronic, local or limited internal inflammation, I have had recourse to the seton, and uniformly with the most marked success; so that, I think, we may look upon the remedy as almost specific in such cases. It is unnecessary to enumerate them. But hepatitis and nephritis belong to them in an especial manner." (Ibid., p. 48.)

We beg leave to assure Dr. Hall that it *is* necessary to enumerate them; and more necessary still to substantiate his astounding assertion of the seton being used by him "*uniformly* with the most marked success," in local or limited internal inflammation. With the experience we have already acquired (e. g. *in re* the alcoholic lotion) as to the latitude of speech Dr. Halls allows himself to indulge in, we call for evidence.

We pass over one or two unimportant chapters to one headed, "On milk abscess and milk fever." It is, at the least, intended for the glorification of Dr. Hall.

"I am of opinion that what is designated milk-fever is frequently symptomatic of the condition of the mammæ. The remedy for this febrile state also is, therefore, *depletion* of the milk ducts. As a preventive of milk-abscess and milk-fever, and with other hygienic objects, the infant should be put to the mammæ at the moment it is born. If, in spite of this, the mammæ become in the slightest degree tumid, or febrile action be set up, another and a stronger infant should be applied *without delay.* This is *Nature's* mode of relief, and infinitely more efficacious than the application of leeches. The mammæ must be kept absolutely free from heat, tenderness, and tumidity; the patient must take barley-water as her sole nourishment, and the bowels must be freely purged.

"I do not pretend, in the above proposition, to have advanced anything new; but in *proposing such promptitude, perseverance, energy,* and *steadiness,* and *decision of purpose,* with which I recommend the measure to be adopted, if these be fully apprehended, I believe I do propose something *new.*" (Ibid., pp. 74-5.)

It will be observed that this is the identical Io Pæan uttered over the grand gum-lancing aforesaid (see page 197), and may be regarded as the patent formula stereotyped on the great No. X of Dr. Hall's organology. The italics are Dr. Hall's, and of course the qualities he italicizes are rare qualities, especially with the lonely drivers of the country. We imagine one of these—an effeminate creature—wending his way over hill and dale, along lanes and by-roads, suffering from the blast, but gathering strength from 'Observations in Medicine,' and receiving a flash of intellectual light from Dr. Hall's *new* proposition, to keep the bowels of a parturient female open, the mammæ cool, and apply the infant to them as soon as born, or subsequently, with "*promptitude, perseverance, energy, steadiness, and decision of purpose.*"

Another painful feature in the volumes before us, hardly ever seen in the writings of men in the position of Dr. Hall, and one which, like the boasting just animadverted on, savours even more strongly of the spirit of puffery and quackery, is the constant, careful, and, indeed, obtrusive mention of the names and addresses of the general practitioners who have consulted the author, and of the patients for whom he is consulted. We purposely and for obvious reasons omit quoting examples of this undignified proceeding—to give it no worse name; but we can assure our readers that there is hardly ever anything so remarkable or incredible in the cases quoted, or their accompanying circumstances, as to render all this flourish of name, address, and condition in life at all requisite; while in instances in which corroboration or verification would be really valuable, there is a total eclipse of any tangible testimony; as, for example, in the reported cures of phthisis, and of curvature of the spine.

Before closing our remarks on what Dr. Hall is pleased to call his '*Practical* Observations,' we must say a few words on the character of the reprints in the volumes before us. We have stated that one of the avowed objects of Dr. Hall, in bringing out these volumes, is the republication of some of his previous writings. And we may remark that this practice of constantly repeating himself, is a striking illustration as well of the irresistible tendency to self-glorification, so characteristic of Dr. Hall, as of the comparative barrenness or limited range of his intellect. Perhaps there never was an author who persisted so perseveringly and systematically in thrusting the same things — sometimes altered, sometimes unaltered—over and over again on the attention of the public. Every new publication so surely contains a greater or less portion of one or more that preceded it, that we may safely assert, that if all his volumes were simply weeded of repetitions, they would dwindle to less than half their present dimensions. In the volumes before us, this habitual custom of the author is, of course, not foregone. On the contrary, it is carried, as usual, to such excess, that he not only reprints what fairly comes within the compass of his professed subjects, but what has no other relation to these than the relation which is always all-sufficient with Dr. Hall, viz. that the things are his own, or that he thinks they are his own. Even when the subjects of the writings are related, who but Dr. Hall would think of re-reproducing them, well knowing that they have all been already issued and sold to the reader in another form? It is surely somewhat unfair to the purchaser of a book, advertised by

the title of 'Practical Observations,' and who bought it solely on the ground of its being what it professed to be, a work on practical medicine, to have forced on him chapter after chapter of old physiological memoirs and lectures, essays on hybernation, reports of policemen at coroner's inquests (vol. ii, p. 301), extracts from letters (ib., p. 308), reports of Dr. Hall's speeches at societies (p. 298), &c. &c. And it is not over-civil to take our money anew even for practical papers which we had already paid for, sometimes more than once, in Dr. Hall's previous publications, or in publications to which he contributed, without giving us previous warning of what we had to expect in our new purchase. For, instance, in the last of the two volumes now before us, we have nearly one third of the matter—actually six or seven of the largest chapters in it—transcribed, almost verbatim, from the 'Cyclopædia of Practical Medicine' (to which Dr. Hall contributed the article on puerperal diseases), only enlarged by the addition of a few cases—probably omitted by the editors in the printing of the original papers.

And here we are compelled still further to animadvert on the extremely careless manner in which many of these reprints have been made. So precious in Dr. Hall's eyes is everything that Dr. Hall fancies he can call his own, that he seems almost to lack the heart to destroy any part of what he has once fathered, though demonstrably bad or wrong. Accordingly, we find, in these volumes not a few of the old writings reproduced in their original form, although this may be one which harmonizes very badly with the new forms with which the writings are now surrounded. In fact, it often happens that the old and new matter directly contradict each other; or that the old matter is so literally old, as to have lost all relationship to the existing state of knowledge.

In illustration of what is just said, we need only turn to the chapters on puerperal diseases already referred to. These essays, we do not hesitate to denounce as being utterly behind our present state of knowledge of the subject. For example, at pp. 120, 128-29, vol. ii, Dr. Hall gives us three seats of puerperal inflammation in the abdomen, but seems innocently ignorant of its being most frequent in the uterine veins and lymphatics. It would actually seem that *uterine phlebitis* was unknown to him! At pp. 122 and 127 he speaks of pelvic abscess as "partial inflammation and suppuration of the *peritoneum*." Now, the truth is, as every one but Dr. Hall knows, that the seat of these abscesses is the cellular substance behind the peritoneum, and not the peritoneum itself; the disease is no more partial peritonitis than a malignant whitlow is inflammation of the skin of the finger. Various other instances of the same sort of antiquarian lore might be extracted from the same chapters; and we could lay our finger on as many direct contradictions in the old and new papers. Thus, at pp. 131-32, vol. ii, the "utility and necessity" of purgatives in peritonitis is laid down as "undoubted;" that is old. At p. 9, however, it is declared that this same treatment in this same disease is prescribed (says the author) "in vain, but not, I believe, with safety and impunity;" this is new.

As we have already hinted, a large portion of the volume has no reference whatever to *practical* medicine. For instance, the chapters on the "Inverse ratio of irritability," &c., and on "Hybernation." The

first of these, which occupies twenty-three pages, from page 223 to 245, is printed verbatim from the 'Philosophical Transactions.' Yet, even this reprint is so slovenly executed, that at pages 228, 229, 230, there are references by letters, &c., to a diagram which does not exist in the volume, but for which the author acknowledges in a note he is *compelled* to refer the reader to the 'Philosophical Transactions.' The whole of this essay, excepting the first five and the last two paragraphs, Dr. Hall had already once before reprinted, verbatim from the original, in his article on "Irritability," in Dr. Todd's 'Cyclopædia of Anatomy'—where, however, as well as in the 'Philosophical Transactions,' the anxious reader may find the diagram.

The chapter on "Hybernation," which occupies forty-one pages of this new series of *Practical Observations in Medicine* (from page 248 to page 289), is in like manner a verbatim reprint, with all the errors of the original. We have taken the trouble to examine this embalmed relic, not only with the original memoir in the 'Transactions of the Royal Society,' but also with its *double* in the 'Cyclopædia of Anatomy.' The article in the Cyclopædia consists of only twenty-three columns and a half, or a little more than eleven pages, yet *three fourths* of the whole are a verbatim reprint of Dr. Hall's Memoir in the 'Philosophical Transactions!' Among the new matter contained in this article, we have a formal correction of one of the chief views of the original memoir, viz. that "*nervous sensibility* is not impaired during hybernation." This he states in the Cyclopædia to be an error, explaining that, what he originally attributed to sensibility is of the "*excito-motory kind.*" And yet our readers will hardly believe, that in this second series of the present so-called *Practical Observations in Medicine*, Dr. Hall has reprinted in 1846 every word of his memoir of 1832, extending over forty-one pages, and has added a postscript of eleven lines, to show that views which occupy two pages of this very reprint are erroneous, although he had corrected formerly in the Cyclopædia the words now reprinted, yet repudiated!

In regard to these reprints, it will further strike the reader who knows the facts of the case, as curious, that, in by far the greater number of instances, no indication is given in the body of the work, nor in the table of contents, that they are reprints at all,—all of them being formally introduced under a consecutive numeration of Chapters, as if they had appeared for the first time, and without any reference (with one or two exceptions) to the work or works in which they had previously appeared! Whether this singular incommunicativeness of the author be the consequence of design or of mere carelessness, we will leave for others to say: we content ourselves with denouncing the whole as one of the most remarkable instances of what is vulgarly called *Bookmaking* that we have ever met with.

We would fain here close our notice of these volumes, and with it all further remarks, for the present, on the literary and scientific merits and claims of Dr. Hall; but the eternal trumpeting of his own great deeds in the matter of physiology, particularly the physiology of the nervous system, and specially in regard to the reflex function, and his reiterated charges against ourselves, compel us to add something on this subject.

In every work Dr. Hall has published, in almost every scrap he has printed, and, of course, in the volumes before us (see *supra*, p. 211), he has over and over again, and loudly, proclaimed his own unequalled merits in this department, and scoffed at the pretensions of other excellent men, his predecessors or contemporaries. Knowing Dr. Hall's undoubted merits, grateful for what he has really done in this branch of physiology and therapeutics, we have been willing to overlook much that we knew to be unjust and wrong, both in his pretensions and imputations. But the same sense of justice to others and to ourselves, which has led us in the preceding pages to illustrate and expose some of his therapeutical vagaries and vanities, has at length induced us, however reluctantly, to break our silence once more on the subject of his physiological discoveries. On the present occasion we shall confine ourselves to the endeavour to rescue from unmerited obloquy and disregard one illustrious name, which Dr. Hall has so pertinaciously and ungenerously sought to deprive of its due honours. Our available space, our time and inclination, forbid our now doing more than this, though there are other names also, both of the living and the dead, which may justly demand a like vindication.

On former occasions we took some pains to notice the physiological views of George Prochaska. In our Fifth Volume we reprinted the whole of his chapter on the functions of the Common Sensory; and in our Ninth Volume we discussed, more at length, the claims to priority of neurological discoveries, in an historical retrospect of the whole subject. We then pointed out the real meaning of the terms used by Whytt and by Prochaska, and maintained that on a due consideration of the true value of those used by Prochaska, his doctrines did, in fact, constitute, as we stated, "a complete anticipation" of Dr. Hall's doctrine of a reflex function. "It is perfectly evident," we said, "that Prochaska had a very distinct idea of the function of the spinal cord and medulla oblongata as a centre of reflected action; and that he specified the principal classes of these, referring them all to the excitement of impressions. We do not see," we added, "what essential difference there is between this doctrine and that of the *Reflex Function* of the spinal cord, as *first* propounded by Dr. M. Hall." (p. 115.) Since that period Dr. Hall—when he has condescended to mention them—has taken every opportunity to depreciate the merits of Whytt and Prochaska, as if he were impelled, by the conviction that his struggles for fame would be valueless, unless he scornfully trampled their claims under foot. We may add, that Dr. Hall has never forgiven us our tribute to the fame of the professor of Prague, and has never ceased from attempts to force from us a concession which truth has hitherto forbidden and still forbids us to make. He is always about to say to us his final farewell, but casts long lingering looks upon us, as if his very immortality depended upon the opinions expressed in this journal. In his work on 'Diseases and Derangements,' he parts with us, he "trusts, for ever," with the declaration that we have "sacrificed truth, honour, and justice on the altar of pert folly, vanity, and insolence;" and yet in his quarto volume he reprints nearly the whole of the chapter quoted by us from Prochaska. Not satisfied with this, he again reverts, in the second series of his present 'Observations' to "ill-natured and disreputable inuendoes about Whytt, Prochaska," &c. &c.

and finally pours forth the bitterness of his soul against us in a letter published lately in one of the weekly journals.*

Every reader of this Review is aware that the opinions expressed in it are fortified, as much as is possible, by trustworthy facts and statements; and doubtless it is from a knowledge of this that Dr. Hall estimates our opinions so highly; but every reader is equally aware that they are, after all, only opinions which he may at his good pleasure either admit or reject, and for which, if they be erroneous or unjust, we are responsible to the tribunal of public opinion. It is a grievous charge that Dr. Hall brings against us before this tribunal; it is a charge to which we boldly plead not guilty; and as truth, honour, and justice are as dear to us as to him, we hope the reader will bear with us if we occupy his attention for a brief space, in vindicating the fair fame of a deceased philosopher; and in vindicating him make our own defence.

And first we will let Dr. Hall state his own objects and discoveries, and we shall allow him to do so in their last perfected form, although it is obvious that we could have made our case much stronger by confining ourselves, as we might justly have done, to his earliest promulgated views:—

"My real objects have been—

"5. First, to separate the reflex actions from any movements resulting from sensation and volition.

"6. Secondly, to trace these actions to an acknowledged source or principle of action in the animal economy—the *vis nervosa* of Haller acting according to *newly-discovered laws.*

"7. Thirdly, to *limit* these actions to *the true spinal marrow,* with its appropriate *incident and reflex nerves,* exclusively of the cerebral and ganglionic systems.

"8. Fourthly, to apply the principle of action involved in these facts to *physiology,* viz. to the physiology of *all the acts of exclusion, of ingestion, of retention,* and *of expulsion* in the animal frame.

"9. Fifthly, to trace this principle of action in its relation to pathology, viz. to the pathology of the *entire class of spasmodic diseases;* and,

"10. Sixthly, to show its relation to *therapeutics,* and especially to *the action of certain remedial, and certain deleterious physical agents.*

"11. Finally, it is to these objects, taken as a *whole* or as a *system,* that I prefer my claims: and I do not pretend that an occasional remark may not have been incidentally made by some previous writer, bearing upon some one or more of them." (New Memoir on the Nervous System, p. 5. Lond. 1843.)

How much of these views belong to Prochaska in the opinion of Dr. Hall is stated very specifically by that gentleman in the work just quoted:

"19. Having on a former occasion (Memoir II, p. 53.) sufficiently refuted the

* We subjoin a part of this effusion: it illustrates well the singular constitution of Dr. Hall's mind; "Dr. Forbes, as editor of the *British and Foreign Medical Review*, inserted a paragraph with the following heading,—'*Complete Anticipation of Dr. Marshall Hall by Prochaska.*' I know not in what allowable terms I can express my indignation, after the lapse of so many years, at this most malignant calumny. In the first place, it is totally unfounded in truth; in the second, Dr. Forbes might have known this by a comparison of a few intellectual moments of Prochaska's work, and my own; in the third, such a comparison was due to his own character (I should think) as well as to my own; fourthly, he must have received the paragraph from some ignorant and very malignant person, and should, therefore, have taken double pains to make that comparison. Lastly, Dr. Forbes has submitted, during a long series of years, to be viewed by a brother in the profession (nay, by many) as a false calumniator, without coming forward with either justification (a thing impossible), or confession (a thing which should have been spontaneous and immediate), when the calumny was properly exposed," &c. (Lancet, Aug. 15, 1846, p. 200.)

claims of several writers, both of remote and recent date, in regard to the subject of my labours, I shall only, in this place, make a very few and brief remarks on the imagined similarity between the views of Prochaska and my own.

"20. Let any one compare the distinct detail of my views just given in sections 5-11, with the observations of Prochaska, and he will at once discover that there is *nothing* in that author possessing *the most remote similarity* to those views." (Ibid. p. 7.)

Dr. Hall then gives long extracts from Prochaska's volume in proof of his statement "that there is *nothing* in that author possessing the *most remote similarity*" to the views quoted above, and numbered five to eleven consecutively. The italics in the extract are Dr. Hall's, and we trust that our readers will remember this emphatic declaration when they have perused the subjoined translation of the fourth chapter, third fasciculus, of the 'Adnotationes Academicæ,' written and published before Dr. Marshall Hall was born.*

On the occasion already referred to (vol. IX, p. 114) we remarked that the term *sensorium commune* would not be understood if reference were not made to the meaning attached to certain words when Prochaska wrote. "In modern times," we then observed, "the term *sensorium commune* is applied to the part of the nervous system where *impressions* become *sensations;* and if we set out with this notion, we should find it difficult to interpret any part of his (Prochaska's) doctrines; but he takes care to define his meaning in the very first paragraph, where he explains the *sensorium commune* to be that part of the nervous centres at which *external impressions* conducted to it by different nerves, give rise to certain and determinate *motions* by respondent motor nerves." We added, "To avoid confusion, we might substitute the term *centre of reflexion* for sensorium commune;" better, perhaps, if we had said "we might substitute the term, *true spinal marrow,* according to the nomenclature of Dr. Hall," the sole difference being, as we shall see presently, that Prochaska's Sensorium Commune included a little more than Dr. Hall's completed "true spinal system:" we say *completed* system, because we believe Dr. Hall's field of reflex action, as first laid out by him (in 1832) comprehended quite as large an extent as Prochaska's. Thus, in his first paper communicated to the Zoological Society, he tells us, "The peculiarity of this motion consists in its being excited by irritation of the extreme portion of the sentient nerves, whence the impression is conveyed through the corresponding portion of *brain and spinal marrow, as a centre,* to the extremities of the motor nerves;" and he there speaks of what he afterwards (1836) termed the excito-motor power as "a property which attaches itself to any part of an animal, the corresponding portion of *the brain and spinal marrow* of which is entire." (Proceedings of the Zoological Society, Nov. 27, 1832.) The subsequent greater limitation of the field of reflexion by Dr. Hall was so far a decided improvement on the doctrine he previously held; but this had nothing to do with what alone concerns us at present, the great fundamental principle of the doctrines—the reflex action or function of the spinal marrow. We are, therefore, fully authorized to request the reader, in perusing the translation, to regard as synonymous not only these two terms, but also the other terms which hold the same relative place in the two versions of the doc-

* Georgii Prochaska, M.D., et Professoris Anatomiæ, &c., in Cæsareo-Regia Universitate Pragensi, Adnotationum Academicarum Fasciculus Tertius. Pragæ, 1784.

trine, the Prochaskean and Hallean. Thus, he must read *common sensory* as *true spinal marrow; sensorial* nerves as *afferent* or *incident-excitor; motor* nerves as *reflex-motor*, &c. If he do so, and if it should happen that he reads this chapter for the first time, we think he will not be surprised that we have felt it to be a paramount duty, if we would preserve the character of impartial critics, to endeavour to set things in their proper light.

To avoid all risk of unfairness in the comparison we are about to institute between Dr. Hall's views and Prochaska's, we shall, in the first place, give a simple translation of the extracts from the Professor's works. We at first thought of introducing, within parentheses, the synonymous modern terms; but we leave this for the reader to do for himself. We will only add, that if our translation reads stiffly, it is because, in making it, we have been desirous that no imputation should be thrown upon our accuracy, which might perhaps have been the case if we had rendered the author's meaning in freer language.

"CHAPTER IV. THE FUNCTIONS OF THE COMMON SENSORY.

"§ 1. *What is the common sensory, what its functions, and what its seat?*

"The external impressions which are made on the sensorial nerves are very quickly propagated throughout the whole length of the nerves, as far as their origin: when they [the impressions] have arrived there, they are reflected by a certain law, and pass on to certain and corresponding motor nerves, through which, being again very quickly propagated to muscles, they excite certain and definite motions. This locality, in which, as in a centre, the sensorial nerves, as well as the motor nerves, meet and communicate, and in which the impressions made on the sensorial nerves are reflected on the motor nerves, is designated by a term, now adopted by most physiologists—the Common Sensory.

"The most distinguished men have not fixed on one place as the seat of the common sensory. Bontekoe, Lancisi, De la Peyronie have placed the common sensory in the corpus callosum; Willis derived the perception of sensation and the source of movements from the corpora striata; Des Cartes attributed the function of the common sensory to the pineal gland; Vieussens to the centrum ovale; Boerhaave decided that aggregate of points to be the common sensory, in which all the sensory nerves terminate, and from which all the motor nerves arise, and accordingly placed it in the medulla fornicata, surrounding the cavity of the ventricles. In a later work, *de Morbis Nervorum*, Boerhaave places the common sensory in the boundary line of the medullary and cortical substance, which opinion Tissot thought to be extremely probable, regarding it as confirmed by the observations of Wepfer. Mayer seems to place the common sensory in the medulla oblongata; that distinguished man, J. D. Metzger, appears to be also of this opinion; the celebrated Camper said, that if the common sensory has a seat at all, it ought to be in the pineal gland, and in the nates and testes, and that, therefore, the opinion of Des Cartes was not so very absurd. It certainly does not appear that the whole of the cerebrum and cerebellum enters into the constitution of the common sensory, which portions of the nervous system appear rather to be the instruments that the soul directly uses for performing its own actions, termed animal; but the common sensory, properly so called, seems not improbably to extend through the medulla oblongata, the crura of the cerebrum and cerebellum, also part of the thalami optici, and the whole of the medulla spinalis, in a word, it is coextensive with the origin of the nerves. That the common sensory* extends to the medulla spinalis is manifest from the motions exhibited by decapitated animals, which cannot take place without the consensus and intervention of the nerves arising from the medulla spinalis; for

* Marherr contends that the medulla spinalis ought also to be referred to the common sensory, in Prælect. ad Instit. Med. Boerhaavii, tom. ii, p. 404.

the decapitated frog, if pricked, not only withdraws the punctured part, but also creeps and leaps, which cannot be done without the consensus of the sensorial and motor nerves, the seat of which consensus must necessarily be in the medulla spinalis—the portion remaining of the common sensory.

"The reflexion of sensorial into motor impressions, which takes place in the common sensory, is not performed according to mere physical laws, where the angle of reflexion is equal to the angle of incidence, and where the re-action is equal to the action; but that reflexion follows according to certain laws writ, as it were, by Nature on the medullary pulp of the sensory, which laws we are able to know from their effects only, and in no wise to find out by our reason. The general law, however, by which the common sensory reflects sensorial into motor impressions, is the preservation of the individual; so that certain motor impressions follow certain external impressions calculated to injure our body, and give rise to movements having this object, namely, that the annoying cause be averted and removed from our body: and, *vice versa*, internal or motor impressions follow external or sensorial impressions beneficial to us, giving rise to motions tending to this end, namely, that the agreeable condition shall be further conserved. Very many instances which might be adduced undoubtedly prove this general law of the reflexions of the common sensory, of which it may be sufficient to adduce a few. Irritation being made on the internal membrane of the nostrils excites sneezing, because the impression made on the olfactory nerves by the irritation, is conducted along them to the common sensory, there by a definite law is reflected upon motor nerves going to muscles employed in respiration, and through these produces a strong expiration through the nostrils, whereby, the air passing with force, the cause of the irritation is removed and ejected. In like manner it happens that when irritation is caused in the trachea by the descent of a particle of food or a drop of fluid, the irritation caused is conducted to the common sensory, and there reflected on the nerves devoted to the movement of respiration, so that a violent cough is excited,—a most suitable means for expelling the cause of irritation—which does not cease until the cause of irritation be ejected. If a friend brings his finger near to our eye, although we may be persuaded that no injury is about to be done to us, nevertheless the impression carried along the optic nerve to the common sensory is so reflected upon the nerves devoted to the motion of the eyelids, that the eyelids are involuntarily closed, and prevent the offensive contact of the finger with the eye. These and innumerable other examples which might be brought forward, manifestly show how much the reflexion of sensorial impressions into motorial, effected through the common sensory, has reference to the maintaining of the conservation of our body. Wherefore, Tissot enumerates the action of the common sensory amongst those powers, the sum and co-ordination of which constitute the nature of our living body.

"Since, therefore, the principal function of the common sensory consists in the reflection of sensorial impressions into motor, it is to be noted, that this reflexion may take place, either with consciousness or without consciousness. The movements of the heart, stomach, and intestinal canal depend in no way upon consciousness; yet, forasmuch as no muscular movement can be excited, unless a stimulus applied to the sensorial nerves passes by a peculiar reflexion to the motor nerves, and excites contraction of the muscle, it is certain that the reflexion of the impressions suitable for exciting those movements, if it takes place in the common sensory, is effected without consciousness. But it is a question whether these impressions, in order that they may be reflected, do really travel so far as the common sensory, or are, without taking this long circuit, reflected nearer, in the ganglia, from whence these parts derive many nerves? This matter is further to be considered afterwards. But that reflexions of sensorial impressions into motor are effected in the common sensory itself, the mind being utterly unaware, is shown by certain acts remaining in apoplectics deprived entirely of consciousness: for they have a strong pulse, breathe strongly, and also

raise the hand, and very often unconsciously apply it to the affected part. The common sensory also acts independently of consciousness in producing the convulsive movements of epileptics, and also those which are sometimes observed in persons buried in profound sleep, namely, the retractions of pricked or irritated limbs,—to say nothing of the motion of the heart and the respiratory acts. To this category also belong all those motions which remain some time in the body of a decapitated man, or other animal, and are excited when the trunk, and particularly the medulla spinalis are irritated, which motions certainly take place without consciousness, and are regulated by the remaining portion of the common sensory existing in the medulla spinalis. All these actions flow from the organism, and by physical laws peculiar to the common sensory, and are, therefore, spontaneous and automatic. The actions taking place in the animal body, with attendant consciousness, are either such as the soul has no voluntary control over, or such as the soul can restrain and prohibit at pleasure; the former being governed by the common sensory alone, independently of the soul, are as much automatic as those of which the soul is unconscious. Of this character are sneezing from an irritant applied to the nostrils, cough from an irritant fallen into the trachea, vomiting from a titillation of the fauces, or after taking an emetic; the tremors and convulsions in St. Vitus's dance, and in a paroxysm of intermittent fever, &c. Those actions, however, which the soul directs or limits by its own power, even although the common sensory has its share in producing them, are nevertheless called animal and not automatic; concerning which we treat in the next chapter." (pp. 114-20.)

The views here detailed are too definite and too lucidly expounded to be considered for one moment as nothing better than random hints or guesses; and, accordingly, we find them repeated by the author, in the text-book of his class at Vienna,* published in 1802; and again in another physiological work, published in 1820, and repeated with the same, or even greater lucidity, after a lapse of thirty-six years:

"That part of the nervous system is termed the general sensory [allgemeine Sensorium] in which external impressions meet, and from which internal impressions are distributed to all parts of our body, where, consequently, the accord [Uebereinstimmung] of all organs necessary to life, takes place, and where external impressions are reflected, according to the law of self-preservation, with or without consciousness, into inner impressions, or those operating outwardly [nach aussen]. The sensory, in which the impressions with consciousness are reflected, may be termed the mental or soul-sensory [Seelensensorium], and the other the corporeal sensory [körperliche Sensorium]. Willis termed the one the reasoning and the other the corporeal soul. The seat of the soul-sensory is particularly [vorzüglich] the brain; of the body-sensory, the spinal marrow; and also, as it appears, the nervous plexuses and ganglia. The last is shown to be the case by acephalous monstrosities, which sometimes for many hours, and even a whole day, remain alive, move their limbs, utter cries, suck the nipple, and the like. So also decapitated animals are observed to continue for a few moments to perform movements with design. By virtue of this accord of the nerves, the operation of a stimulus is not limited to the nerves immediately irritated, but is extended to remote nerves and their organs, which is named the *consensus nervorum;* as, for example, the irritation of the gravid uterus excites nausea, vomiting, headache, toothache, and the like." (Physiologie, oder Lehre von der Natur des Menschen, von Dr. u. Prof. Georg Prochaska, &c. &c. 8vo, Wien, 1820, p. 92)

Let us now briefly consider the views here stated, and compare them with those of Dr. Hall. In the first place, we find that Prochaska *did* "separate the reflex actions fro many movements resulting from sensation

* Lehrsätze aus der Physiologie des Menschen. Von D. Georg Prochaska, ordentl. Lehrer der Anatomie, Physiologie und Angenarzneykunde in Wien, &c. &c. Zum Gebrauche seiner Vorlesungen. Zweyte verbesserte und vermehrte Auflage. Wien, 1802.

and volition." He makes this distinction repeatedly, and in terms so positive and so clear, that there can be no mistake about the matter. This separation by Prochaska is the first of the views respecting which Dr. Hall most emphatically asseverates, "there is *nothing* in that author possessing the *most remote similarity!*"

We find, too, that Prochaska *did* limit these reflex actions to a part of the nervous system, namely, that part comprising the medulla spinalis, the medulla oblongata, the crura of the cerebrum and cerebellum, and part of the thalami optici; Dr. Hall's "true spinal marrow" comprises, even in its last development, the corpora quadrigemina, the medulla oblongata, and the medulla spinalis. Prochaska also discriminated between the reflex, the intellectual, and the ganglionic centres. As regards the reflex and intellectual, the quotation already given is decisive; the subjoined contains his views respecting the ganglionic; it is from the section headed, "*An nervi in gangliis suis consentiunt?*"

"Unzer* and Winterl taught that external impressions are reflected in the ganglia as they are reflected in the common sensory, and that the ganglia are special [particularia] sensories, which opinion appears to be by no means destitute of probability. For, considering that the minute and invisible nerves disseminated on the internal membrane of the heart and of its auricles, receive a stimulus from the inflowing venous blood,—if they cannot transmit the impression of that stimulus to the common sensory through the ganglia of the intercostal nerve [the sympathetic],† and yet communicate it to the motor nerves distributed to the fleshy structure of the heart and auricles,—there is necessarily a consensus of the sensory nerves on the inner membrane of the heart with the motor nerves distributed through the fleshy structure of the heart and auricles, which (consensus) must have its seat either in the ganglia of the intercostal [or sympathetic] nerve, or below them, in the conjunctions and communications of the nerves. It seems, therefore, probable that besides the common sensory which we conjecture to be in the medulla oblongata, medulla spinalis, pons varolii, and crura of the cerebrum and cerebellum, &c., there exist special sensories in the ganglia and plexuses (concatenationes) of the nerves, in which (special sensories) external impressions ascending along the nerves are reflected, without needing to ascend to the common sensory, in order to be there reflected." (p. 29.)

Prochaska also applied the principle of action of his "common sensory" to physiology, beautifully and clearly pointing out the general law by which it regulated its operations, namely, the law of preservation of the individual; not simply by the avoidance of the painful, but by attaining the pleasurable. He also applied his views to the illustration of pathological phenomena, as those of chorea Sti Viti, &c., mentioning especially the tremors or rigors, and convulsions which occur during the paroxysms of intermittent fevers; and this, we think, a most important application of the doctrine of reflexion overlooked altogether by Dr. Hall.

With these plain statements before them, we leave our readers to draw their own conclusion on the question, as to whether Dr. Hall's assertion be correct, or whether it presents even the smallest approach to accuracy.

* The works of Unzer, viz. his 'Grundriss eines Lehrgebäudes von der Sinnlichkeit der thierischen Körper,' published in 1768, and another, published in 1771, entitled 'Erste Gründe einer Physiologie der eigentlichen thierischen Natur thierischer Körper,' are very remarkable productions, of which we hope to give some account on a future occasion. Although the doctrines contained in them have an intimate relation with some parts of the present discussion, we purposely avoid referring to them, as we wish to confine ourselves, for the present, strictly to the defence of Prochaska.

† The sympathetic nerve was formerly termed the great intercostal nerve.

Is it TRUE that there is *nothing* in the writings of Prochaska possessing the *most remote similarity* to his own views, for such we have seen is the unqualified, emphatic assertion of Dr. Hall? We will add one or two other random assertions, just as audacious, and just as untenable. The first is made immediately after extracts from the chapter just translated:

"It is surely needless to add another remark upon this author. I will therefore only further observe, or rather repeat, that we have not, in *any* author, the idea of the true *reflex* action of the *vis nervosa*, or of a reflex physiological act; that no one had detected the *reflex* nature of the physiological acts of *respiration*, or discovered the system of *incident* respiratory nerves," &c. (New Memoir, p. 10.)

We shall revert to the assertion respecting these claims to the discovery of "the idea of the true *reflex* action of the *vis nervosa*" and of the respiratory muscles; all we need ask is this: Is it not staringly apparent that there *is* an idea of a *reflex physiological act* in Prochaska's illustrations, derived from the acts of sneezing or coughing? Does not Dr. Hall himself reckon them among the acts of "*expulsion?*"

We add yet another assertion as companion to the above:

"It would be easy to select passages from Whytt and Prochaska to whom so much reference has been made of late, to show their profound ignorance of the nervous system, as it is now understood. It is not necessary for me to add any commentary. Both these authors have, doubtless, great merit; but they were both entirely and absolutely unacquainted with the distinct and exclusive *physiology* of the true spinal marrow, in the acts of ingestion and of egestion, in their relation to the preservation of the individual, and the propagation of the species." (Observations, 2d Series, p. 16.)

And this monstrous assertion is writ in the face of Prochaska's clear and lucid announcement, "THE GENERAL LAW by which the Common Sensory reflects sensorial impressions into motor is *the preservation of the individual*—EST NOSTRI CONSERVATIO!"*

We subjoin yet another example of Dr. Hall's reckless disregard of historical truth:

"My First Memoir was entitled 'On the reflex *function* of the medulla oblongata and medulla spinalis.' This important *function*, as the nervous agent in all the Acts of Ingestion and of Egestion in the animal economy, was previously unknown. It is *not* mentioned by Whytt, or Prochaska, or any other author; who, however, they may cite the term reflex, or detail experiments, or treat of sympathetic actions, have not, I affirm, associated *one physiological act* with any such reflex *function* of the spinal marrow. This is, therefore, my discovery." (New Memoir on the Nervous System, p. 9.)

If the emphatic term in this sentence had been spinal marrow, we could have supposed that Dr. Hall meant to play upon that term, and that as Prochaska used the term common sensory, Dr. Hall's statement might be verbally although not essentially correct. But the emphatic term is "reflex *function*," and if Prochaska has not associated the physiological acts of vomiting, sneezing, coughing (from an impression on the

* He even mentions this physiological law of preservation twice, in as many words, in the same chapter, as will be seen in our translation. We here subjoin the original: "Hæc et innumera quæ afferri possent exempla manifeste ostendunt quantopere *reflexio sensoriarum impressionum in motorias* per sensorium commune facienda *conservationem nostri corporis* respiciat." (p. 118.) "Generalis tamen lex, qua *commune Sensorium impressiones sensorias in motorias reflectit*, EST NOSTRI CONSERVATIO." (p. 117.) We may add also, that in the physiological works subsequently published by him, he continually repeats this general law, and as one of fundamental importance.

sensory nerves) with a "reflex *function*" appropriated to a certain portion of the central axis, then words have no meaning, and language conveys no ideas.

We might stay our pen here, for we feel convinced that the preceding proofs of *our* truthfulness are unanswerable, and must bear conviction to the minds of the most prejudiced reader. And as we remarked respecting the "practical observations," so we have to remark respecting Dr. Hall's pretensions to physiological discovery; *ex uno disce omnes.* We need not, therefore, trouble our readers with further exposures of Dr. Hall's conduct to other physiologists, his predecessors and contemporaries, because it is abundantly manifest that no reliance whatever ought to be placed on his assertions when his vanity is concerned, and how seldom is it not! We may, however, revert to one other assumed discovery of Dr. Hall's, because *that* belongs also, as well as the "reflex function," to George Prochaska.

Dr. Hall states in his new memoir "that we have not in *any* author the idea of the true reflex action of the vis nervosa," and further adds:

"I have been always struck by three remarkable facts. The first is, that there should have been a principle of muscular action in the animal economy, acknowledged amongst physiologists, the *vis nervosa* of Haller, without any application to physiology. The second, that there should have been a series of experimental phenomena—certain muscular actions—untraced, either *backwards* to any *principle* of action, or *forwards* to any *physiological* act. The third, that there should have been a series of functions, and a class of diseases hitherto unexplained, which the excito-motor principle, just noticed, and involved in those experiments, should be calculated to explain in the most perfect and satisfactory manner." (New Memoir, p. 6.)

Now Dr. Hall terms this *vis nervosa* the excito-motor power, and arranges the portions of the nervous system according as they are endowed or unendowed with this vis nervosa; or in other words, are excitor or in-excitor. We will quote Dr. Hall, to prevent mistake:

"The nervous system may be divided into different portions, according as they are endowed or unendowed with the excito-motor power; thus, in general terms, the cerebrum and cerebellum are *in*-excitor; the medulla oblongata and medulla spinalis are excitor; the nerves of special sense are *in*-excitor; the trifacial and the analogous special nerves are incident excitor nerves; the nerves distributed to muscles are direct excitors; the ganglionic system is excito-motor, but, for reasons which will be given hereafter, in a less prompt and energetic degree." (New Memoir, p. 17.)

The discovery of the vis nervosa is not claimed by Dr. Hall, nor of the fact that it acts from the central ganglia to the muscles; the fact was already too well known. The discovery he claims is of its application to physiology, and of its action from the surface along a special class of nerves to the central ganglia, and there being reflected on the motor nerves. Let us now then examine Prochaska's views in relation to this point. The title of the eighth section of chapter I of the Fasciculus from which we have already quoted,—and the title alone indicates what Dr. Hall denies to any other writer, viz., an extended application of the vis nervosa to physiology—thus runs:

"*The functions of the nervous system are explained by the vis nervosa.*

"At length also in this department of animal physics we abandon the

Cartesian method of philosophizing, and embrace the Newtonian, being persuaded that the way to truth is tedious, and truly most doubtful through hypotheses and conjectures, but far more certain, excellent, and short that which is analytical—(*quæ a posteriori ad causam ducit*). Newton has given the name of *attraction* [vis attractiva] to the inscrutable cause of physical attractions, observed its effects, discovered and arranged its laws of action, and so has established a doctrine useful and honorable to the human mind; thus also ought we to act in reference to the functions of the nervous system. We will term the cause inherent in the pulp of the nerves there producing its effects, and hitherto not determined, the *vis nervosa;* its effects which are the functions of the nervous system, having observed, we will arrange—we will discover their laws; and by this method we may be able to found a true and useful doctrine, which will certainly afford a new light and a more elegant form to medical art. Haller has already used the term *vis nervosa* for the purpose of indicating the agent by which the nerves effect contractions of muscles; but J. A. Unzer has thrown the greatest light on this matter; for although he still may use the term *animal spirits* to make himself more readily comprehended, his system is quite independent of any belief in the existence of these, as he himself explains." (pp. 28-9.)

Here it is manifest that Prochaska has in view a much wider application of the vis nervosa than that adopted by Haller; he applies it to the whole nervous system, and not to the motor portion only, and exults, with the prevision of a seer, over the results which will follow the application of his views to medicine. "They will surely," he declares, "afford new light and give a finer form to medical art." This we esteem a singularly touching trait in Prochaska's mind, and of itself is a token of deep and powerful thought, and of a clear conviction of the truth of his doctrines. His candour towards Unzer, too, marks a generous nature; would that in our day it had met with a more generous requital!

The seat of the vis nervosa was fixed by Prochaska in the medullary pulp of the nerves and cerebro-spinal axis; it was latent there, and required to be excited by a stimulus. Just as the iron requires to be struck by a flint to elicit a spark, so, he says, the vis nervosa is latent and produces no actions of the nervous system until a stimulus be applied. The stimulus may be either material or immaterial, mechanical or mental. (p. 47.) He then proceeds to treat of the vis nervosa in subsections. Two of these, the 4th and 5th (pp. 49, 56) are headed "*Under what conditions the vis nervosa is increased and diminished*"—titles, by the way, which naturally remind us of an expression used by Dr. Hall in his first paper in the Philosophical Transactions for 1833, viz. "*that the reflex function admits of exaltation and diminution,*" (p. 651); though, of course, Dr. Hall will find no similarity between the two. The author enumerates several pathological states in which the increase takes place, &c. He thus explains convulsive attacks:

"If the vis nervosa be increased in the common sensory, there results, firstly, that the external impressions made on the sensory nerves and carried to the common sensory, are too suddenly and violently reflected and pass on to the motor nerves, and thus excite movement and convulsions contrary to the will of the soul, as happens in the frights of infants, and also of some adults who are terrified by some trifling crash or noise." (p. 52.)

He also considers the circumstances under which it is diminished. (§ 5, p. 56.) He further states (§ 6, p. 60) that it is divisible, for it exists in

nerves when separated from the central axis by ligature or section, and in the spinal cord when separated from the brain; quoting, in illustration, experiments on decapitated frogs, the muscular contractions of the heart when separated from the body, and the vital acts of acephalous fœtuses. Prochaska expressly states that when a nerve is divided it retains the power of exciting muscular contractions when irritated, but cannot excite sensations; but he emphatically observes, that nothing departs from the nerve by section, and it only loses the power of exciting sensation because the power of propagating impressions to the brain is taken away by the severance of continuity. (p. 68.) His views as to the vis nervosa of the nerves are very clear; it must, however, be remembered that the nerve spoken of by him is a *compound* nerve.

"If an impression be made on the extremity of a nerve, termed an external impression, it is most quickly propagated along the whole length of the nerve as far as its origin; and, *vice versa*, if an impression be made on the origin of the nerve, termed an internal impression, this also most quickly descends along the nerve to its termination; but if the impression be made on the trunk of a nerve it is at the same moment quickly propagated both to the origin and termination. This fitness of the nerves to receive impressions and quickly transmit them, either way along their whole length, ought to be called the *vis nervosa* of the nerves; it is also properly called the *sensibility* or *mobility* of nerves, or, as Unzer also rightly termed it, the *corporeal sense without concomitant perception*." (p. 76.)*

Prochaska expressly declines defining the nature of this vis nervosa as to whether it be electricity, or phlogiston, or some kind of air, or the material of light; he leaves that question to others to decide. Nor will he say positively whether this *vis* is derived from the numerous blood-vessels accompanying the nerves, or is drawn through inorganic pores from the air, &c.; but he insists that it is *not derived from the brain*, but is a something conjoined with the medullary pulp, which the nerves, however they acquire it, acquire in the same way as the brain itself. (p. 77.) He is positive, too, as to "the equal function of the nerve in exciting sensation and motion; namely, to receive the impression of a stimulus, and to pass it quickly along its whole length; which impression, when it comes to the brain, causes a perception of a sensation, but if to the muscle excites its contraction." (p. 78.)

The following extract shows that Prochaska considered this vis nervosa the agent in reflecting impressions. It opens the fifth chapter devoted to the cerebral functions, and we may here observe that Prochaska not only clearly proves the brain to be the organ of the mind, and distinct from the common sensory or "true spinal marrow," but he devotes a section to the special consideration of the question, whether it be a compound organ, and decides in the affirmative: such was his "profound ignorance of the nervous system as now understood"!

"In that part of the nervous system which we have termed the common sensory, there inheres such a mechanism that the external sensorial impressions of

* It is obvious from the context, that the word *sensibility* (sensibilitas), as here used by Prochaska, meant, not the capacity of sensation or consciousness of impressions, but merely an inherent power of respondence to impression. The word is continually used in this sense by the French and German Physiologists at the present time; and Prochaska's acceptation of the term *sensorium commune* is in harmony with this.

the nerves are reflected in it upon the motor nerves, in a singular manner and by peculiar laws, so that they excite certain and determinate muscular acts. It has been previously stated that many truly automatic motions in man are excited solely by the vis nervosa of the common sensory; and many animals live and are regulated by this vis nervosa of the common sensory alone, which animals are destitute of brain or the higher animal endowments, and may therefore be truly termed *automata;* but to the nervous system of man, and many animals having an affinity with him, is superadded the brain, and besides this a certain principle which we term the mind or soul, an Ens of incorporeal origin," &c. (p. 130.)

After this, can we regard the vis nervosa of the common sensory of Prochaska, as different in any essential respect from the excito-motor power of Dr. Hall? To be sure the latter considers it the *sole* property of the true spinal system; but whether this be an improvement on Prochaska's theory or not remains to be seen. It is clear, however, that, according to Prochaska's views, the vis nervosa gives the nerves and spinal marrow the property of receiving and transmitting impressions, which, when conducted to the brain, cause sensation, and when transferred from afferent to muscular nerves, or directly applied to muscular nerves, excite motion; and that, therefore, they are endowed with an excitor power; and that the vis nervosa is the excito-motor power.

How clearly and precisely Prochaska distinguished between the functions of the brain and true-spinal system, and between the acts of the vis nervosa excited by the soul or the will, and the acts of the vis nervosa or excito-motor power of the common sensory excited by impressions, will be demonstrated by another extract, and this, we hope, will be considered sufficient. In the last quotation, it would be observed that Prochaska termed the reflex acts *automatic*, because they were dependent upon the mechanism of the common sensory or "true spinal marrow" acted on by its vis nervosa or "excito-motor power."

"§ IV. *What movements are properly termed animal [voluntary]?*

"The muscular acts of the human body are of two kinds according to their exciting cause; thus, one class is called animal or voluntary, because the soul willing and commanding, they may be excited, increased, diminished, or checked; the other is involuntary of which the soul is unconscious, or if conscious, they are performed against its will and are excited by a mere mechanical corporeal stimulus applied to the nervous system; which movement is, therefore, termed spontaneous and automatic. Nerves are required for the production of both kinds of motion. But nerves do not act without a stimulus, which [stimulus] is either afforded by the soul, willing, or—it being unconscious or unwilling,—by any substance applied to the nerves. From whence it is evident that those movements only should be termed animal, which depend upon the free command of the soul; which it excites or checks by its own free will; on the contrary, those movements which in no wise depend on the volition of the soul, but are performed when it is either unconscious or unwilling, cannot be called animal, but are purely mechanical and automatic.

"Observation teaches that there are muscles in the human body over which the soul has no control whatever, and which are moved automatically during the whole life: of this kind are the heart [ventricles], auricles, œsophagus, stomach, intestinal canal, and among these also the movements of the iris may be reckoned. Other muscles there are, which ordinarily during life respond to volitions, and are, therefore, termed voluntary: such are the muscles of the limbs, trunk, head, face, eyes, tongue, genitals, with which also the sphincters of the anus and urinary

bladder may be included. Sometimes, however, it happens that all these muscles refuse submission to the soul, and, without its concurrence or consciousness, are powerfully and preternaturally agitated by some mechanical stimulus, which thing may be observed to occur in the convulsions of hysterical females, of epileptics, of infants, of those suffering from St. Vitus's dance; and these movements, although effected by voluntary muscles, cannot be otherwise regarded than as automatic. These muscles are not as yet moved voluntarily, but for the most part automatically in the fœtus in utero or the newly born; the cerebrum at this early age is not sufficiently developed, nor until the organs of the faculty of thought are successively evolved, does the soul attain the power to think and to use the muscles subject to its volition. To automatic movement belongs also the lifting and application of the hand to the head by apoplectics, the turning of the body in sleep, and in some degree somnambulism itself, but which, however, it appears, should also be partly ascribed to obscure sensations and volitions, of which the soul becomes immediately oblivious. Thirdly, there are muscles which perpetually act independently of the volitions of the soul, excited solely by a mechanical stimulus, over which muscles the soul has, however, a free voluntary power, being able *pro libitu*, to accelerate or retard their motions, or even to check them for a time. The action of these muscles we call a mixed action. Such are the muscles subservient to respiration, which almost constantly act automatically, but over which there is a free power of the soul, so that it is able to accelerate or retard respiration, or even to stop it for a while. But if the mechanical stimulus be too powerful, then, although the soul be unwilling, the muscles of respiration are excited to action; as for example, when a crumb gets into the trachea the most violent cough is excited, which cannot be restrained by any effort of the soul; so also the soul cannot restrain sneezing when the pituitary membrane of the nostrils is excited by an acrid stimulus." (pp. 144-46.)

Dr. Hall may say that this is a jumble of incoherent facts, and especially that the movements of the heart, stomach, and intestines belong to the ganglionic system. But this opinion Dr. Hall himself seems to have adopted very lately, abandoning his previous division into cerebral, true spinal, and ganglionic; while Prochaska himself (as we have already seen) doubted whether the sympathetic ganglia might not be special centres of reflexion, quite independent of the great central axis of reflexion; yet Dr. Hall makes no mention of Prochaska in that change of opinion.

"The intra-spinal structure is also the central organ of the ganglionic system· I formerly supposed that the ganglia were themselves the centre of this system—an opinion founded on an experiment, the accuracy of which I am now led to doubt." (Observations in Medicine, 2d Series, p. 57.)

And on the strength of that solitary experiment, Dr. Hall not only deduced a general principle, but ridiculed the doctrines of a man in every way his superior,—whose doctrines he copies, and whose identical doubts he shares!

Such is the imperfect exposition we must, for the present, content ourselves with making of the despised doctrines of GEORGE PROCHASKA. But imperfect as it is, it is sufficient to justify us in stating the general result of our inquiry to be a much more complete anticipation than we have hitherto declared, and that all the *fundamental* and *acknowledged* views which are claimed by Dr. Hall as exclusively his own, are to be found in Prochaska's writings most succinctly, and most clearly set forth. There is first, the vis nervosa or excito-motor principle demonstrated, and its laws of action laid down; its excitation by acts of will in the

cerebrum; its excitation by impressions made on afferent nerves leading to the common sensory; its excitation by reflection of impressions on motor or efferent nerves; its presence in the nerves; and its share in their functions. The cerebral system is distinguished as the seat of will and of intellect, from another central axis, the seat or centre of reflexion, and the limits of the two systems are accurately defined. The distinction is broadly made between voluntary and reflex acts, and motions which may be produced in either way; the exciting causes of the two classes are lucidly pointed out, and it is shown that the will is the excitor of the action of the vis nervosa in voluntary acts, while a mechanical or material stimulus is shown to originate the reflex acts. The distinction between reflex acts with, and reflex acts without consciousness, is well and most plainly drawn; and the influence of the will in restraining or modifying the reflex movements is clearly indicated. Thus respiration is shown to be an excited act, and, therefore, ordinarily and habitually, reflex in its nature, but that the respiratory muscles are under some control of the will, and, therefore, it may be modified by volition when required. Lastly, the general law or final cause of reflex action—the preservation of the individual, *nostri conservatio*—is most distinctly stated and most admirably illustrated.

Do we, then, mean to accuse Dr. Hall of having been acquainted with Prochaska's writings before the publication of his neurological views, and, consequently, of knowingly promulgating another man's doctrines as his own? We do not. Dr. Hall has distinctly denied all such knowledge; and we have no proof that he had it. Wherefore, we can only say, that, however discreditable it might be to Dr. Hall, as a scholar, to have been unacquainted with so striking a work as that of the Professor of Prague, published to the world so long before his time, and on the very subject of his own inquiries, we must still charitably believe that he was ignorant of its existence at the time he first promulgated his views relative to the Reflex Function, although he was acquainted with the writings of Whytt, Blane, &c., containing a good deal of analogous matter. We consequently acquit Dr. Hall of all serious blame in this respect. We merely say that he was culpably ignorant of what he ought to have known, and that he had the misfortune—not the fault—to be entirely anticipated, in all the essential part of his doctrines, by others, and especially by Prochaska. So stood the case, we may admit, in 1832, or even in 1833, the date of Dr. Hall's first Memoir. But how stands it now? How has it stood since 1835, the period at which Dr. Hall acknowledges he became acquainted with the writings of Prochaska? How, more especially, has it stood since April 1838, when we, in this Journal, and, about the same time, Mr. George, in the 'Medical Gazette,'* first disclosed to physiologists the claims of the Professor of Prague, and forced them more particularly on the attention of Dr. Hall? Very different indeed: to Dr. Hall disgracefully different: inasmuch as, instead of acknowledging the merits of his predecessor, when made known to him, or admitting, in any degree, the

* Med. Gaz., April 7th and 14th, 1838. In these clever papers the author has given a very striking and, in some respects, even a more complete exposition of the identity of the views of Prochaska and Dr. Hall, than we have given in the present article. Mr. George's essay is well worthy the attention of all who are interested in the present discussion, or in the history of neurology.

similarity of his doctrines to his own—a similarity, to use no stronger term, which it is impossible for any unprejudiced person to deny—he has either studiously passed over in silence or openly ridiculed and maligned Prochaska's doctrines, and poured on the heads of those who did no more than assert their resemblance to his own, all the venom which his bitter nature could engender. And up to the very hour at which we write, he continues, as we have seen, to boast, as loudly as ever, of his originality; although for more than ten years he has, by his own showing, been familiar with the very writings which we have proved to contain not merely the elements but the developed germs of his early doctrines, and from which he has demonstrably drawn not a few of the materials wherewith he has since so greatly modified them. We can add no comment in words that can in any way emulate, in damning potency, the eloquence of this simple statement. It is grievous to be forced to write it down; it is melancholy to contemplate its full import: we shall therefore turn from the painful theme, and hasten to conclude our task.

After all this, it will naturally be asked, what is due to Dr. Hall? What can he justly claim? What are his deserts? We reply, that much is due to him; and how much, we, of all men, are best entitled to say, seeing that we have devoted more time, and space, and labour in unfolding his undoubted claims in this Journal, than have been accorded to him by all the other Journals of Europe put together. It is no subject of concern to us, that Dr. Hall has somewhat ungraciously received our humble but well-meant aid, because it was mixed with homage to others. We only did what we believed to be our duty, although we are well aware that some of the best physiologists of our time have considered our deference to him as too profound, and our commendations as too highly coloured. We do not share in this judgment; we still think as we have heretofore thought; and the frequency with which we have dwelt on this theme, is our sole reason for not reiterating our opinions here. In sober earnestness, indeed, we think, it is impossible to contemplate Dr. Hall's actual position in the estimation and regard of his contemporaries and fellow-labourers in science, without a compassionate sympathy which is at once melancholy and distressing. Instead of being, as, perhaps, he might have been, the head and leader of the physiologists of his country, he lives the very Pariah of the physiological caste, the Ishmael of a desert created and sought out by himself, with his hand against every man and every man's hand against him. How much better would it have been for his fame—how much better, we should think, for his happiness—had Dr. Hall had the fortune or the wisdom to have chosen another course, and studied his own heart; and, if not gifted by Nature with a genial or generous temperament, had at least learned, as he might have learned, to discipline his rebellious propensities in the school of a better philosophy. Then would he have found his contemporaries willing and happy to yield him more than he claimed; would have seen that, by allowing merit to others, he increased his own; and would have known nought of that worst bitterness to a vain-glorious man, of being slighted by those on whose breath he lives, and compelled, in expiation of unjust pretensions, to look to the tribunal of posterity for the recognition even of the merits which he knows to be his own.

Art. XV.

Du Hachisch et de l'Aliénation Mentale, Etudes Psychologiques. Par J. Moreau (de Tours), Médecin de l'Hospice de Bicêtre, Membre de la Société Orientale de Paris.—*Paris*, 1845.

Psychological Studies on Hachisch and on Mental Derangement. By J. Moreau (of Tours), Physician to the Hospital of the Bicêtre, and Member of the Oriental Society of Paris.—*Paris*, 1845. 8vo, pp. 431.

What is this Hachisch, and what it has to do with Mental Derangement, are questions which our readers will naturally ask, on perusing the above title; and to these questions it will answer our purpose better to give them a reply at once, than to play with their curiosity by keeping them in suspense, *Hachisch* is the oriental name of the plant which is scientifically known as the *cannabis indica*, and in our own vernacular as the Indian hemp; of which the extract has been lately used in this country as a sedative, narcotic, and antispasmodic. In the East, however, it is rather valued on account of its power of producing a species of intoxication, whose peculiar characters we shall presently analyse; and it is used for this purpose among the Arabs, as extensively as opium among the Turks and Chinese, and alcoholic liquors among the European nations. The most common preparation of Hachisch,—that which forms the foundation of all others, is a sort of fatty extract; this is prepared by boiling the leaves and the flowers of the plant with water to which a certain quantity of fresh butter has been added; the decoction is reduced by evaporation to the consistence of a syrup, and is then strained through a cloth. The butter thus becomes charged with the active principle, and acquires a greenish hue; its taste is very disagreeable, and hence it is seldom taken alone, but is mixed with other confections and aromatics, so as to form a sort of electuary. The one in most common use is termed by the Arabs *dawamesc;* but this is frequently mingled with substances of reputed aphrodisiac virtues, to enable it to minister more effectually to the grossly sensual purposes which seem to be the great object of life among many of the orientals. The leaves of the Indian hemp may be smoked with tobacco; and when freshly gathered they have a rapid and energetic action. Their peculiar powers are destroyed, however, by drying; whilst the dawamesc retains its activity for many years, the only change it undergoes being the acquirement of a slight rancidity, which does not seem to injure the vegetable extract, as M. Moreau informs us that he has had some of this substance in his possession for twelve years, and that it has lost none of its powers. We commend this fact to the attention of our pharmaceutists; for it certainly appears to us remarkable, that a substance of so little stability as to be altered by the simple drying of the leaves, should be preserved unaltered during so long a period by being mingled with fatty matter; and it suggests the question whether many of our more delicate and uncertain watery extracts might not be advantageously superseded by preparations made upon this plan.

There seems little doubt that the effects of the hachisch have been known from a very remote antiquity. The plant is very common in the

centre and west of Asia, growing without any cultivation; and its peculiar properties, therefore, would be easily discovered. It is, indeed, remarkable how ingenious mankind have been in finding out means of inebriating themselves. The spontaneous fermentation of saccharine juices probably furnished the first intoxicating beverage; and we find in the oldest history of our race that the postdiluvian patriarchs were occasionally overcome with wine, in a manner which renders it probable that drunkenness was one of the vices of the antediluvian nations. At the present time we have, as is well known, an immense variety of these alcoholic beverages; some produced by the simple fermentation of expressed vegetable juices, whilst in others the art of distillation is employed for their concentration; some again being prepared from saccharine matters artificially produced, as in the process of malting; and one (as far as we know, a solitary instance) being obtained from an animal secretion,—the Tartars, who are cut off from all ordinary sources of alcoholic supply, managing to obtain a spirituous liquor which they term *koumiss* from the milk of their mares, which is well known to be peculiarly rich in saccharine matter whilst correspondingly poor in butter. Then again in the tropical portion of the Old World, we have the opium and the Indian hemp, and the tobacco in the New; whilst among some of the nations of Siberia, whose inhospitable climate will neither bring these productions to perfection, nor supply the means of obtaining alcoholic drinks, we find an extraordinary kind of intoxication induced by an agariciform fungus, the *amanita muscaria.*

The use of all these substances appears to have commenced at a period anterior to any historical or traditional record. We find numerous allusions to intoxicating draughts not only in sacred history, but in the oldest of the classic writers. Thus it will be remembered that Homer makes Helen give to Telemachus, in the house of Menelaus, a potion prepared from the *nepenthes*, which made him forget his misfortunes; and this plant was said to have been given to Helen by a woman of the Egyptian Thebes. We learn from Diodorus Siculus that on this circumstance much stress was laid by the Egyptians, who argued from various allusions in his writings that Homer had sojourned among them; for the women of Thebes, or as it was afterwards named Diospolis, were noted for possessing a secret by which they could dissipate anger or melancholy. The celebrated oriental scholar, M. Sylvestre de Sacy, appears to have made it pretty plain, that our word assassin is derived from the Arabic name of the Indian hemp. It is well known that it was originally employed in Syria to designate the followers of the "Old Man of the Mountain;" who were accustomed to devote themselves with blind obedience to the execution of his orders, however barbarous their character. These followers, according to the old traveller Marco Polo, were selected from the most robust young men of the country which was under this singular domination; their education tended in every way to impress upon them the duty of blind obedience, in return for which they were promised after death all the sensual pleasures they could imagine; and a foretaste of these was every now and then given to them by intoxicating them with hachisch, in the midst of scenes in which everything was provided to gratify their senses. In this manner a sort of fanaticism was gradually induced, which made

the *Hachischins* (from a corruption of which name the word assassin was formed) ready to sacrifice either themselves or others at the direction of their chief, without the slightest hesitation. From the relations of various of the older travellers in the East, it appears that the peculiar intoxicating powers of this plant have long been made subservient to the purposes of the priestly classes, in practising upon the credulity of the vulgar; and for anything we know, the same use may be made of them at the present time. The employment of the extract of hemp is more extended than M. Moreau seems aware of. His own knowledge of it is confined to the East; but from the accounts of travellers, it would seem to be nearly as much used by the Hottentots (and probably by other African nations), and even by the aborigines of Brazil. Of the identity or difference between the American, African, Asiatic, and European species of hemp we are not able to speak positively; but there seems little doubt that they all possess the same active principle; the amount of it which is produced, however, varying with the nature of the climate in which the plant grows. Of the extract obtained from the common hemp (cannabis sativa) Endlicher says, "Emollitum exhilarat animum, impotentibus desideriis tristem stultam lætitiam provocat, et jucundissima somniorum conciliat phantasmata."

Whatever opinion we may hold in regard to the much-vexed question of the connexion between mind and body, there can be no doubt of the influence which the condition of the latter exerts over the operations of the former; and there are no more striking examples of such an influence, than those which are presented by the introduction of alcohol, opium, hachisch, nitrous oxide, or some other intoxicating substance into the current of the circulation. That the presence of a minute portion of any of these substances,—a portion almost too minute to be recognized by ordinary chemical processes,—in the blood which is passing through the capillaries of the brain, should so alter its relations to the nervous substance as to produce results which manifest themselves in an entire change of the ordinary course of psychical phenomena, must always be included, we apprehend, as a fundamental fact in any theory that may be framed by philosophers who please themselves with speculating on this mysterious question. The marked correspondence which may be traced between the phenomena of insanity and those which are induced by the introduction of such substances into the blood, must not be overlooked in any attempt to arrive at the true pathology of the former condition, or to bring it within the domain of the therapeutic art. We believe that Mr. Sheppard may claim the merit of having first prominently directed attention to this method of viewing the phenomena of insanity; and we would take this opportunity of stating our present feeling that in our unfavorable criticism of his little work 'Insanity a Blood-disease' (see Vol. XVII, p. 526), we had rather too strongly before our eyes the *demerits* of his hypothesis, than its positive value. His notion was, we are ready to admit, quite correct in regard to a certain class of cases of insanity: and his fault was that which is so common with young writers, namely, hasty generalization; the same idea being most unwarrantably stretched, so as to include *all* forms of this disease. There can be no doubt that the properties of the blood may be perverted by abnormal changes going on within the system, as well as by the direct introduction of poisonous substances from without;

and its due relations to the nervous structure may be thus completely changed, so that the psychical operations are seriously interfered with, and a form of insanity develops itself, which is capable of being removed by the adoption of measures calculated to eliminate the morbific matter from the blood, and to restore it to its pristine purity. And we have little doubt that a part at least of the phenomena of those forms of insanity which are brought on by what are commonly termed *moral* causes, are referrible to the same agency; for every physiologist well knows how much the excitement of the passions and emotions involuntarily and indeed unconsciously affects those organic functions by which the blood is prepared and renovated; and how speedily any imperfection in the depurating actions (those of the liver and kidney more especially) is manifested in the abatement or irregularity of the functional powers of the nervous centres. We believe that an attentive study of the etiology and phenomena of insanity will gradually lead to the establishment of well-marked distinctions between this class of cases, and that in which disease of the cerebral structure itself is the proximate cause of the disordered psychical manifestations; and that in proportion as this difference is kept in view will be the clearness of our prognosis and the efficiency of our remedial measures.

We are rather surprised that this view of the subject has not occurred to M. Moreau, whose treatise is almost purely psychological, his principal object being to show that there is a positive *identity*—not merely the *analogy* usually admitted—between the psychical states of insanity and dreaming; the phenomena of the excitement produced by hachisch and other intoxicating agents, serving, in his opinion, to connect the two in such a manner as to prove that there is no essential difference between them. On this question we shall offer a few remarks as we proceed; stopping first to notice the principal effects of the *hachisch* upon the bodily system, and then endeavouring to analyse the chief psychical phenomena which result from its use.

The account of M. Moreau is principally founded upon his own experience; one characteristic of the condition which is thus induced being that the individual does not altogether lose his power of self-restraint and self-observation, and that he is subsequently able to retrace most of what he has felt and acted during the state of excitement. "I am satisfied," he remarks, "that by a certain exertion of the will, these effects may be altogether checked, or at least considerably diminished; just as we master the passion of anger by a strong voluntary effort." The effects vary considerably with the dose taken; but there is no certain rule for its administration, the results of the same dose being often very different in different individuals. There are some, indeed, on whom it seems to produce no effect whatever; no result having followed the administration of doses, which would have usually been followed by strongly marked phenomena. A small dose usually seems to have no other result than a moderate exhilaration of the spirits, or at most, to produce a tendency to unseasonable laughter; and this is generally one of the first effects of a dose which is large enough to occasion the "higher phenomena." There is at the same time a slight acceleration of the pulse with a somewhat retarded respiration; and a genial warmth diffuses itself through the body, with the ex-

ception of the feet which are usually chill. The wrists and forearms seem as if loaded with a weight; and movements are automatically performed as if to shake them free. At the same time vague muscular sensations, *des inquiétudes*, prompting to continual restlessness, are experienced in the lower extremities; corresponding, we presume, to what is known on this side of the channel as *the fidgets.* Other strange sensations, varying in different individuals, are very commonly experienced; many of them depending, we have no doubt, on the special direction of the attention to the part. Thus a very common feeling is that of the brain boiling over, and lifting the cranial arch like the lid of a tea-kettle. The epigastric region is very often the seat of similar odd feelings; the most common is a sense of anxiety or of constriction. The movements of the heart seem to the individual to be performed with unusual violence; but a bystander does not find that its impulse and sounds have undergone any increase, the change being in the sensorium of the subject of the experiment. Slight, or sometimes rather energetic spasmodic actions, never amounting however to proper convulsions, affect the limbs; the contraction of the flexor muscles predominating. There is usually a disposition to assume the recumbent position; and the limbs and trunk are then all brought together by their action. Sometimes the muscles of the face, especially those of the jaw, are affected with spasmodic twitches; and even a state of temporary trismus may supervene. These physical effects of the hachisch are usually but of short duration; but they often depart only to return again after an interval of variable duration. They commonly develop themselves most when the disturbance of the psychical functions has reached its height, or is taking its departure; and those who are in the habit of taking hachisch know how, by graduating the dose, to obtain the latter without the disagreeables of the former.

The first result of a dose of hachisch sufficient to produce the *fantasia* (as this remarkable condition is termed in the Levant), is usually an intense sentiment of *happiness*, which attends all the operations of the mind:

"It is really *happiness* which is produced by the hachisch; and by this I imply an enjoyment entirely moral, and by no means sensual as we might be induced to suppose. This is surely a very curious circumstance; and some remarkable inferences might be drawn from it, this for instance among others;—that every feeling of joy and gladness, even when the cause of it is exclusively moral, that those enjoyments which are least connected with material objects, the most spiritual, the most ideal, may be nothing else than sensations purely physical, developed in the interior of the system, as are those procured by the hachisch. At least, so far as relates to that of which we are internally conscious, there is no distinction to be made between these two orders of sensations, in spite of the diversity in the causes to which they are due; for the hachisch-eater is happy, not like the gourmand or the famished man when satisfying his appetite, or the voluptuary in the gratification of his amative desires, but like him who hears tidings which fill him with joy, like the miser counting his treasures, the gambler who is successful at play, or the ambitious man who is intoxicated with success," &c. (p. 54.)

We believe that our author is here quite correct in his general notion, although not very happy in his illustrations. Without having recourse to hachisch for the proof, we believe that most of our readers will be able to recall analogous states of exhilaration, and the reverse condition of

depression in themselves; the former being characterized by a feeling of general well-being, a sentiment of pleasure in the use of all the bodily and mental powers, and a disposition to look with enjoyment upon the present, and with hope to the future; whilst in the latter state there is a feeling of general but indefinable discomfort, every exertion, whether mental or bodily, is felt as a burden, the present is wearisome, and the future is gloomy.* Such conditions are so distinctly referrible, in a great majority of cases, to the state of the general system, that we have no doubt whatever of their being universally dependent upon it. There are many individuals whose life is passed in an alternation between these two states, corresponding with alternations in the weather; the one being produced by a moderate north-easterly breeze and a bright sun, whilst the other is an invariable sequence upon a damp south-westerly wind and an overclouded sky. In others, again, the depression is induced by a deficient biliary excretion; and the relief given by the depuration of the blood consequent upon a moderate dose of blue-pill is attended with positive exhilaration. Our readers will remember that we attempted in our last number (vol. XIV, p. 512) to establish the probability of the existence of a single seat of consciousness and of pleasurable or painful sensations, both for impressions received through the organs of the senses, and for the ideas formed by the instrumentality of the cerebrum. And it seems to us to correspond well with this view, that in the conditions to which we refer, the same feelings of pleasure and discomfort attend *all* the operations of the mind—the simple sensational, and the intellectual. In the state of exhilaration we feel a gratification from sensations which at other times pass unnoticed; whilst those which are usually pleasurable are remarkably enhanced; and in like manner, the trains of ideas which are started being generally attended with similar agreeable feelings, we are said to be under the influence of the pleasurable or elevating emotions. On the other hand, in the state of depression we feel an indescribable discomfort from the very sensations which before produced the liveliest gratification; and the very thoughts of the past, the present, and the future, which we before dwelt on with delight, now excite no feelings but those of pain, or at best of *insouciance*. There appears to us just as much reason for considering these simple feelings to be immediately dependent upon physical conditions of the central sensorium, as there is for regarding sensations themselves in that light; and in that case all our emotional states, however elevated their character may appear, are immediately dependent, as maintained by M. Moreau, upon such physical conditions; for, as we have attempted to show, there is no other essential difference between a lofty

* These, as all other moods of mind (we may almost say, as a matter of course), are delineated by Shakespeare. Thus, Romeo gives expression to the feelings inspired by the first state:

> My bosom's lord sits lightly in his throne,
> And, all this day, an unaccustomed spirit
> Lifts me above the ground with cheerful thoughts." (*Rom. and Jul.*, v, 1.)

While the reverse state is delineated by Hamlet, in his well-known soliloquy;

"I have of late—but wherefore I know not—lost all my mirth, forgone all custom of exercises; and, indeed, it goes so heavily with my disposition; that this goodly frame, the earth, seems to me a sterile promontory; this most excellent canopy, the air, look you,—this brave o'erhanging firmament, this majestic roof fretted with golden fire, why, it appears no other thing to me than a foul and pestilent congregation of vapours." (*Hamlet*, ii, 1.)

emotion or moral feling and a sensual gratification, than that the one consists of pleasure connected with certain *ideas*, whilst the other consists of pleasure excited by certain *sensations*. It is, then, in the elevation of the ideas which form a constituent part of the emotion, that the elevation of the sentiment really consists. As there are some individuals who by nature and habit attach themselves to sensual gratifications, so there are others who find their pleasure in the contemplation of the ideal; but these may really be as low in their moral nature as the preceding, for their ideas may be merely the recollections of past sensations of the most debased nature, as in the case of the enervated voluptuary who can no longer do anything but gloat over the past; and it is only when these ideas are habitually entertained, which have reference to loftier objects, that the real superiority of intellect over sense becomes apparent.

In the account which we have cited from M. Moreau, as to the peculiar sense of happiness resulting from the employment of hachisch, with the further light which we gain from other parts of his description, it appears to us that this peculiarity is to be discerned;—namely, that the feelings of pleasure are not excited so much by the *sensations* received from the external world, as from the *ideas* which are passing through the mind; and that where sensations *are* productive of pleasure, the pleasure is not sensational but ideal; in other words, the sensation itself does not give pleasure, but excites or modifies an idea which becomes a source of gratification. Now this is most remarkably seen in certain cases of dreaming. Every one knows how much the current of our thoughts in this condition is influenced by external impressions made upon the bodily system; of these impressions we are not conscious *as such;* but they arouse ideas, which become sources of pleasure or pain. Thus the application of a blister may cause a person to dream of being burned at the stake, and to feel the horrors of his ideal situation most acutely, whilst utterly unconscious of the real source of his torment; whilst, in like manner, a draught of cold air blowing upon us, may cause us to be transported in thought to the polar regions, and to suffer in imagination all the pains of a temperature of 40° below zero. We shall presently have occasion to notice the same class of phenomena as characteristic of insanity. Before proceeding with our account of the *fantasia,* we may stop to notice the remark of M. Moreau, that a peculiar state of general exhilaration or joyous excitement, closely analogous to that which is the first result of the hachisch, is often the precursor of an attack of insanity; showing, even in the mode of access of the disordered mental state, a close correspondence between the two conditions.

One of the first appreciable effects of the hachisch is the gradual weakening of that power of voluntarily controlling and directing the thoughts, which is so characteristic of the vigorous mind. The individual feels himself incapable of fixing his attention upon any subject; his thoughts being continually drawn off by a succession of ideas which force themselves (as it were) into his mind, without his being able in the least to trace their origin. These speedily occupy his attention, and present themselves in strange combinations, so as to produce the most fantastic and impossible creations. By a strong effort of the will, however, the original thread of the ideas may still be recovered; and the interlopers

may be driven away, their remembrance however being preserved, like that of a dream recalling events long since past. These lucid intervals, however, become of shorter duration and can be less frequently procured by a voluntary effort; for the internal tempest becomes more violent, the torrents of disconnected ideas are so powerful as completely to arrest the attention, and the mind is gradually withdrawn altogether from the contemplation of external realities, being conscious only of its own internal workings. There is always preserved, however, a much greater amount of self-consciousness than exists in ordinary dreaming; the condition rather corresponding with that, in which the sleeper knows that he dreams, and if his dream be agreeable makes an effort to prolong it, and is conscious of a fear lest he should by awaking, cause the dissipation of the pleasant illusion. It is another characteristic of the action of the hachisch, that the succession of ideas has *at first* less of incoherence than in ordinary dreaming, and the ideal events do not so far depart from possible realities; and the disorder of the mind is at first manifested in errors of sense, in false convictions, or in the predominance of one or more extravagant ideas. These ideas and convictions are generally not altogether of an imaginary character, but are rather suggested by external impressions; these impressions being erroneously interpreted by the perceptive faculties, and giving origin, therefore, to fallacious notions of the objects which excited them. It is in the stage of the fantasia which immediately precedes the complete withdrawal of the mind from external things, and in which the self-consciousness and power of the will are weakened, that this perverted impressibility becomes most remarkable; more especially as the general excitement of the feelings causes the erroneous notions to have a powerful effect in arousing them:

"We become the sport of impressions of the most opposite kind; the course of our ideas may be broken by the slightest cause; we are turned, according to a common expression, by every wind. By a word or a gesture our thoughts may be successively directed to a multitude of different subjects, with a rapidity and a lucidity which are truly marvellous. The mind becomes possessed with a feeling of pride, corresponding with the exaltation of its faculties, of whose increase in energy and power it becomes conscious. It will be entirely dependent on the circumstances in which we are placed, the objects which strike the eyes, the words which fall on our ears, whether the most lively sentiments of gaiety or of sadness shall be produced, or passions of the most opposite character shall be excited, sometimes with extraordinary violence; for irritation shall rapidly pass into rage, dislike to hatred and desire of vengeance, and the calmest affection to the most transporting passion. Fear becomes terror, courage is developed into rashness, which nothing checks, and which seems not to be conscious of danger, and the most unfounded doubt or suspicion becomes a certainty. The mind has a tendency to exaggerate everything; and the slightest impulse carries it along. Those who make use of the hachisch in the East, when they wish to give themselves up to the intoxication of the *fantasia*, take great care to withdraw themselves from everything which could give to their delirium a tendency to melancholy, or excite in them anything else than feelings of pleasurable enjoyment. They profit by all the means which the dissolute manners of the East place at their disposal. It is in the midst of the harem, surrounded by their women, under the charm of music and of lascivious dances executed by the Almees, that they enjoy the intoxicating dawamesc; and with the aid of superstition, they find themselves almost transported to the scene of the numberless marvels which the Prophet has collected in his Paradise." (p. 67.)

The error of perception is remarkably shown in regard to time and space. Minutes seem hours, and hours are prolonged into years; and at last all idea of time seems obliterated, and the past and present are confounded together. This error seems to be easily accounted for, when we remember that our individual notions of time are founded upon the changes which occur in our own minds; so that a few minutes of very active succession of ideas may seem (as in a dream) like the lapse of years. But it does not appear so easy to account for the error in regard to space; except that every notion formed in this curious condition seems to partake of a certain exaggeration, and that the estimation of space in particular is greatly dependent upon that of time. M. Moreau mentions as an illustration, that on one evening he was traversing the passage of the Opera when under the influence of a moderate dose of hachisch. He had made but a few steps, when it seemed to him as if he had been there two or three hours; and as he advanced, the passage appeared interminable, its extremity receding as he pressed forwards. Here we think the error of time might suffice to explain the illusion. But he gives another more remarkable instance. In walking along the Boulevards, he has frequently seen persons and things at a certain distance presenting the same aspect as if he had viewed them through the large end of an opera-glass; that is, diminished in apparent size, and therefore suggesting the idea of increased distance. This erroneous perception of space is one of the effects of the *amanita muscaria*, or intoxicating fungus; a person under its influence being said to take a jump or a stride sufficient to clear the trunk of a tree, when he wishes only to step over a straw or a small stick. Such erroneous perceptions are common enough among lunatics, and become in them the foundation of fixed illusions; whilst in the person intoxicated by hachisch, there is still a certain consciousness of their deceptive character.

Though all the senses appear to be peculiarly impressible in this condition, yet that of hearing seems the one through which the greatest influence may be exerted upon the mind, especially through the medium of musical sounds. M. Theodore Gaultier, an artist of celebrity, describes himself as hearing sounds from colours, which produced undulations that were perfectly distinct to him. Here we recognize a little of the artistic imagination. But he goes on to say that the slightest deep sound produced the effect of the rolling thunder; his own voice seemed so tremendous to him, that he did not dare to speak out for fear of throwing down the walls, or of himself bursting like a bomb; more than five hundred clocks seemed to be striking the hour with a variety of tones, &c. &c. Of course those individuals who have a natural or an acquired musical ear, are the most likely to be influenced by the concord or succession of sweet sounds; and in such the simplest music of the commonest instrument, or even an air sung by a voice and in a style of the most mediocre kind, shall excite the strongest emotions of joy or melancholy, according as the air is cheerful or plaintive; the mental excitement being communicated to the body, and being accompanied with muscular movements of a semi-convulsive nature. This influence of music is not merely sensual, but depends like that of other external impressions, upon the associations which it excites, and upon the habitual disposition to connect with it the play of the imaginative faculties.

It is seldom that the excitement produced by the hachisch fixes itself upon any particular train of ideas, and gives rise to a settled delusion; for in general one set of ideas chases another so rapidly, that there is not time for either of them to engross the attention of the intellect, more especially since (as already remarked) there is usually such a degree of self-consciousness preserved throughout, as prevents the individual from entirely yielding himself up to the suggestions of his ideal faculties. M. Moreau mentions however that on one occasion, having taken an over-dose and being sensible of unusual effects, he thought himself poisoned by the friend who had administered it, and persisted in this idea in spite of every proof to the contrary, until it gave way to another, namely, that he was dead and was about to be buried; his self-consciousness however being yet so far preserved that he believed his body only to be defunct, his soul having quitted it. But when this is altogether suspended, as it seems to be by a larger dose, the erroneous ideas become transformed into convictions, taking full possession of the mind; although sudden gleams of common sense occasionally burst through the mists of the imagination, and show the illusive nature of the pictures which they have conveyed to the sensorium. All this, as every one knows, who has made the phenomena of insanity his study, has its exact representation in the different stages of mental derangement; the illusive ideas and erroneous convictions being in the first instance capable of being dissipated by a strong effort of the will, gradually exerting more and more influence on the general current of thought, and at last acquiring such complete mastery over it, that the reasoning processes cannot be called into effectual operation.

Our readers must be well aware that there has been at different times much controversy among those who have attempted to investigate the mental pathology of insanity, in regard to the degree in which the feelings and affections may be deranged without intellectual disorder. Of late the general tendency of writers upon the subject has been towards the affirmative view of this question; admitting both a *general* disorder of the feelings, affections, and active powers, without any illusion or erroneous convictions impressed upon the understanding; and a *special* disorder caused by the exaggeration of some one particular feeling or propensity, which may exhibit its influence before any fixed delusion takes possession of the mind, and which may be regarded as the cause of that delusion where it exists. It is urged by M. Moreau, on the other hand, that although the feelings, affections, &c., may be in a state of excitement, either general or special, there is no abnormal character about the mental operations, until the intellect becomes so far disturbed that it cannot keep them any longer under restraint; and that, in consequence, there is no such thing as a purely moral insanity. The excited feelings have dominion over the mind, not so much on account of their own strength, as on account of the weakness of the reflective powers and of the will, which should keep them in check.—Such would certainly appear to be the case in the disordered mental condition induced by the hachisch; but although the phenomena may be to a certain extent the same as those of insanity, and may serve to elucidate them on many points, it seems to us that we must be careful of interpreting the latter altogether by the former, or supposing that the disorder always supervenes in the same way. It is

reasonable to suppose that, when it is occasioned by the presence of morbific matter in the blood, or by any cause affecting the general circulation through the brain, all the powers and propensities of the mind should be more or less affected; and this we see in ordinary mania, the delirium of fever, puerperal mania, intoxication by alcohol, &c., as in the fantasia of hachisch or of opium. But it is still quite possible that one or more of the individual components of the healthy mental fabric should undergo alteration from some less general cause; whilst the remainder undergo no other change than that which results from the perverted action of the faculties or propensities which are primarily affected; and thus a monomaniacal state is produced, in which the individual is insane upon one point, whilst perfectly sane upon every other. It does not seem to us to be necessary, as M. Moreau thinks, that the intellectual powers should have undergone a considerable weakening, before the insane tendency or idea can acquire that predominance which shall allow it to operate upon the general current of thought, and to guide the determinations of the will. For anything which destroys the regular balance between the emotions and propensities on the one hand, and the will on the other, will cause the former to exert an undue influence: and this may happen, either by an extraordinary development of a particular propensity, the intellect and will retaining their normal power but being insufficient to keep it in check; or by a general weakening of the intellectual powers, which renders them unable to control even the ordinary propensities, and thus allows all of them full play or permits any one which is naturally strongest to assert its predominance; or lastly, both these changes may take place together, as seems to occur under the influence of hachisch.

It appears to us, however, that all the phenomena of disordered mental states will need to be analysed anew, in accordance with the progress of our knowledge of the physiology of the brain, and of the normal operations of the mind of which it is the instrument; and we shall now endeavour briefly to point out a few of the applications of which we believe the views we have propounded on that subject (in the article in our last Number to which we have already referred) to be capable, in aiding the elucidation of the numerous problems which these interesting conditions present for investigation. In the first place, the localization which we attempted to make of the *sensorium commune* (using that term in its limited sense, as the centre of consciousness,) appears to harmonize well with the fact, that its portals may be open to sensations immediately derived from the external world, whilst they may be closed to the perception of those cerebral operations, which, when we become conscious of them we call ideas: or that, on the other hand, they may be completely open to the latter, and altogether closed to the former. This last condition is that which presents itself when dreaming occurs in a sleep which is otherwise profound. The individual is altogether unconscious of external impressions, but he is vividly conscious of the operations of those parts of his encephalon, which were well designated by Reil as the "nerves of the internal senses." He does not always possess that peculiar sense of individuality or self-consciousness, which seems closely connected with the feeling of externality (if we may use the expression) that is called up by impressions made upon his organs of sense; for the ideas of which he is conscious, though relating to external things, seem not so strongly to call

up the feeling of personal existence as do the sensations that first called them into being. When the self-consciousness or sentiment of individuality has a more distinct existence, as in reverie and some forms of dreaming, and also in somnambulism, it would seem as if the sensorium were less completely closed against impressions from without; although as we have already remarked, the attention is usually fixed so completely upon the cerebral sensations or ideas, that the influence of external impressions is not so much *direct* as *reflected* through them. This seems to us to be the explanation of the well known fact, that in many forms of somnambulism the individual seems to be conscious of those external impressions alone which either completely fall in with his train of thought, or which harmonize with it to a degree which renders them capable of directing and modifying it. And even in those other cases, in which the individual seems alive to every external impression, it seems as if he did not perceive them as ordinary sensations, but was conscious of them only through the ideas they excited.

In one instance of *induced* somnambulism also (by Mr. Braid's *Hypnotism*), a man remarkable for the poverty of his muscular development lifted with the greatest apparent ease a 28lb. weight *upon his little finger*, and swung it round his head, upon being assured that it was as light as a feather; whilst he declared himself altogether unable to raise a handkerchief from the table, after many apparently strenuous efforts, having been assured that its weight was too great for him to move. We have every reason to believe that the personal character of the individual placed him above the suspicion of intentional deceit; and if our readers will only try the first of these experiments for themselves in the waking state, we feel assured from our own experience that they will find great difficulty in accomplishing such a feat on the first trial, unless they should happen to possess a remarkable muscular development in their superior extremities; whilst the training necessary to enable it to be performed with the ease with which we saw it executed, ought to have manifested itself in the individual whose performance we witnessed, had that performance been the result of practice with a deceptive intention. Of course there is no *proof* of the absence of deception in the second experiment; and, therefore, we would not draw any inference from it alone. But both seem to agree in this remarkable phenomenon, which we think we may regard as *typical* of the state to which we allude; namely, that the actions of the individual were completely governed by his *ideas*, and that these ideas were not corrected by his *sensations* in the way that they are in the waking state. He was led to *believe* that the weight was light: and the *experience* of its actual pressure on his finger did not undeceive him. The degree in which our muscular sense and our muscular exertion are influenced by our mental condition, must be familiar to every one. Most of our readers have probably heard of Dr. Wollaston's exclamation, "Bless me, how heavy it is!" when he first poised a globule of potassium upon his finger, and judged of its weight from its metallic appearance; although it is really much lighter than water. And there is abundant evidence of the great increase of muscular power, which results from the confidence of success in any particular effort, or from the overpowering desire of saving one's life; whilst a doubt of failure, still more a settled determination of the impossibility of the success of the exertion, "unnerves" the frame, and deprives it of even

its ordinary degree of power. We very well remember an experiment which was much in vogue a few years ago, in which four persons were to lift a fifth from the ground with extraordinary ease, provided that they all took a full inspiration at the same moment; and according to some accounts, the individual so elevated went up like a feather, and was capable of being sustained upon the fore-fingers of his bearers. Now it is easy to understand upon physiological principles the benefit of a full inspiration in communicating a temporary increase to the muscular power of those who were to exert it; but how it could make any difference in the weight of the party lifted whether his chest were full or empty (except by altering his specific gravity in an inappreciable degree) we never could understand; and after an attentive observation of the experiment, as performed under a variety of circumstances and by many different parties, we came to the conclusion, that the facility with which it was performed depended entirely (*cæteris paribus*) upon the strength of the impression previously made upon the mind of the parties; and that whilst those who were confident of the efficiency of the plan would accomplish the feat without effort, those who were sceptical could find out no other effect than that which might be predicted on the grounds already mentioned. If this explanation be fully carried out, and applied to cases in which the idea receives no contradiction nor correction from actual sensations, we believe that it will be found completely adequate to explain the remarkable phenomena we have mentioned. The whole character of the state of somnambulism, indeed, when carefully considered, appears to us explicable on this view,—that the mind is conscious only of ideas or cerebral operations, and that external impressions do not excite its attention as ordinary sensations, but only by exciting those operations, or in other words by generating ideas. We shall presently see that the phenomena of the *fantasia* and of insanity indicate that these states have many points of alliance with that which we have just been considering; but we shall first revert for a short time to the converse state, in which the sensorium remains open to external impressions, and the respondent consensual actions are performed; but in which it seems altogether incapable of being influenced by cerebral changes, that is, of receiving ideas or following out a process of reasoning.

Of this state, a remarkable case published not long since by a very intelligent observer,* affords a most apposite illustration. Our attention has been directed to it subsequently to the publication of the article to which we have already referred; and we could not wish a better practical exemplification of the theoretical views we then advanced. The subject of the case was a young woman of robust constitution and good health, who accidentally fell into a river and was nearly drowned. She remained insensible for six hours after the immersion; but recovered so far as to be able to give some account of the accident and of her subsequent feelings, though she continued far from well. Ten days subsequently, however, she was seized with a fit of complete stupor, which lasted four hours; at the end of that time she opened her eyes, but did not seem to recognize any of her friends around her; and she appeared to be utterly deprived of the senses of hearing, taste, and smell, as well as of the power of speech. Her mental faculties seemed to be entirely suspended; her only medium

* Mr. Robert Dunn, in Lancet, 1845.

of communication with the external world being through the senses of sight and touch, neither of which appeared to arouse *ideas* in her mind, though respondent *movements* of various kinds were excited through them.

"Her vision at short distances was quick; and so great was the sense of exaltation of the general sensibility upon the surface of the body, that the slightest touch would startle her; still, unless she was touched, or an object or a person was so placed that she could not help seeing the one or the other, she appeared to be quite lost to everything that was passing around her. She had no notion that she was at home, nor the least knowledge of anything about her; she did not even know her own mother, who attended upon her with the most unwearied assiduity and kindness. Her memory and the power of associating ideas [apparently the power of *forming* them also,—REV.] were quite gone. Wherever she was placed, there she remained during the day. She was very weak, but her bodily health was not much deranged; the tongue was clean; the skin moist; and the pulse quiet and regular. . . . . . Her appetite was good; but having neither taste nor smell, she ate alike indifferently whatever she was fed with, and took nauseous medicines as readily as delicious viands. She required to be fed. When I first saw her, she had no notion of taking the food that was placed before her; but, a few days afterwards, if a spoon was put into her hands, and filled by her mother, and conveyed a few times to her mouth, she would afterwards go on by herself until the whole was eaten. Her wants were sedulously attended to; but she *manifested no uneasiness as to food*, however long she might be kept without it. The same thing was observed in reference to drink. The calls of nature were alike unheeded by her; the urine and fæces were voided *unconsciously*; but with the striking peculiarity that, during the expulsion of the fæces, such was the reflex *action* induced, that the extremities became spasmodically convulsed and rigid, the head was thrown backwards, the muscles of the neck were stiff, and the eyelids closed; so that her mother considered that her bowels never acted without her having what she called "a convulsion fit;" the same thing occasionally happened when the bladder expelled its contents; and what is still more remarkable, the same tonic rigidity of the muscles invariably took place when she went to sleep."

Before quoting any more of the details of this interesting case, we may stop to remark, that all the automatic movements unconnected with sensation, of which the spinal cord is the instrument, seemed to go on without interference; as did also those dependent upon the sensations of sight and touch; whilst the functions of the other ganglia, together with those of the cerebral hemispheres, appeared to be in complete abeyance. The analysis of the facts stated regarding her ingestion of food seems to make this clear. She swallowed food when it was put into her mouth; this was a purely automatic action, the reception by the lips being probably excited by sensation (for this movement is different from that of suction, which has been shown to be purely reflex), whilst the act of deglutition, when the food is carried within reach of the pharyngeal muscles, is excited without the necessary concurrence of sensation. She made no spontaneous effort, however, to feed herself with the spoon; showing that she had not even that simple idea of helping herself, which infants so early acquire. But after her mother had conveyed the spoon a few times to her mouth, and had thus caused the muscular action to become associated with the sensorial stimulus, the patient continued the operation. It appears, however, to have been necessary to repeat this lesson on every occasion; showing the complete absence of memory for an idea of a character so simple and immediately connected with the supply of the bodily wants. The difference between an *instinct* and a *desire or propensity*, on which we

formerly dwelt, is here most strikingly manifested. The patient had an instinctive tendency to ingest food; as is shown by her performance of the actions already alluded to: but these actions required the stimulus of the present sensation, and do not seem to have been connected with any notion of the character of the object *as food;* at any rate, there was no manifestation of the existence of any such notion or idea, causing her to manifest a *desire for food* or drink in the absence of the objects, when she must have been conscious of the uneasy sensations of hunger and thirst. The very limited nature of her faculties, and the *automatic* life she was leading, appear further evident from the followiug particulars.

"One of her first acts on recovering from the fit had been to busy herself in picking the bed-clothes; and as soon as she was able to sit up and be dressed, she continued the habit by incessantly picking some portion of her dress. She seemed to want an occupation for her fingers, and accordingly part of an old straw bonnet was given to her, which she pulled to pieces of great minuteness; she was afterwards bountifully supplied with roses; she picked off the leaves, and then tore them into the smallest particles imaginable. A few days subsequently, she began forming upon the table, out of these minute particles, rude figures of roses and other common garden flowers; she had never received any instructions in drawing.—Roses not being so plentiful in London, waste paper and a pair of scissors were put into her hands; and for some days she found an occupation in cutting the paper into shreds; after a time these cuttings assumed rude figures and shapes, and more particularly the shapes used in patchwork. At length she was supplied with proper materials for patchwork; and after some initiatory instruction, she took to her needle and in good earnest to this employment. She now laboured incessantly at patchwork from morning to night, and on Sundays and week-days, for she knew no difference of days; nor could she be made to comprehend the difference. She had no remembrance from day to day of what she had been doing on the previous day, and so every morning commenced *de novo.* Whatever she began, that she continued to work at while daylight lasted; manifesting no uneasiness for anything to eat or drink, taking not the slightest heed of anything which was going on around her, but intent only on her patchwork."

She gradually began, like a child, to register ideas and acquire experience. This was first shown in connexion with her manual occupation. From patchwork, after having exhausted all the materials within her reach, she was led to the higher art of worsted work, by which her attention was soon engrossed as constantly as it had before been by her humbler employment. She was delighted with the colours and the flowers upon the patterns that were brought to her, and seemed to derive special enjoyment from the harmony of colours; nor did she conceal her want of respect towards any specimen of work that was placed before her, but immediately threw it aside if the arrangement displeased her. She still had no recollection from day to day what she had done, and every morning began something new, unless her unfinished work was placed before her; and after imitating the patterns of others, she began devising some of her own. The first *ideas* derived from her former experience, that seemed to be awakened within her, were connected with two subjects which had naturally made a strong impression upon her; namely, her fall into the river, and a love-affair. It will be obvious that her pleasure in the symmetrical arrangement of patterns, the harmony of colours, &c. was at first simply *sensorial;* but she gradually took an interest in looking at pictures or prints, more especially of flowers, trees, and animals. When, however,

she was shown a landscape in which there was a river, or the view of a troubled sea, she became intensely excited and violently agitated, and one of her fits of spasmodic rigidity and insensibility immediately followed. If the picture were removed before the paroxysm had subsided, she manifested no recollection of what had taken place; but so great was the feeling of dread or fright associated with water, that the mere sight of it in motion, its mere running from one vessel to another, made her shudder and tremble; and in the act of washing her hands they were merely placed in water An attempt to introduce the use of the shower-bath, by simply pouring water through a colander upon her head, induced such alarming excitement and fright, and was followed by a fit of insensibility of such long continuance, that the experiment was not repeated. We do not conceive that these phenomena warrant the inference that she had as yet any idea, even an indistinct one, of her accident, or of the circumstances attending upon it; for we have known a case of spontaneous double consciousness, in which a strong feeling excited in one state left an impression that manifested itself strongly in the other, although the individual was completely oblivious of its cause. But the manner in which the convulsions were brought on, in this case, shows that simple *ideas* were now being formed; for whilst the actual sight or contact of moving water excited them by the simple sensorial channel, the sight of a picture containing a river or water in motion could only do so by giving rise to the idea of water. Had she been able to comprehend the meaning of names in this state, we doubt not that the sight of the word *water* would have produced the same effect. Our readers will at once perceive the exact coincidence of these phenomena with those of the hydrophobic convulsion, to which we alluded on a former occasion; and will also agree with us, we trust, that the peculiar condition of this patient confirms the explanation of those actions which we then gave,—namely, that they are excited through the sensory ganglia; the *idea* transmitted from the cerebrum having exactly the same influence upon them as would have been exerted by a sensation directly transmitted from without.

From an early stage of her illness she had derived evident pleasure from the proximity of a young man, to whom she had been attached; he was evidently an object of interest when nothing else would rouse her; and nothing seemed to give her so much pleasure as his presence. He came regularly every evening to see her, and she as regularly looked for his coming. At a time when she did not remember from one hour to another what she was doing, she would look anxiously for the opening of the door about the time he was accustomed to pay her a visit; and if he came not, she was fidgety and fretful throughout the evening. When by her removal into the country she lost sight of him for some time, she became unhappy and irritable, manifested no pleasure in anything, and suffered very frequently from fits of spasmodic rigidity and insensibility. When, on the other hand, he remained constantly near her, she improved in bodily health, early associations were gradually awakened, and her intellectual powers and memory of words progressively returned. We here see very clearly, as it appears to us, the composite nature of the emotion of affection. At first, there was simple pleasure in the presence of her lover, excited by the gratification which former association had connected with the *sensation*. Afterwards, however, it was evident that the pleasure

became connected with the *idea;* she *thought* of him when absent, expected his return (even showing a power of measuring time when she had no memory for anything else), and manifested discomfort if he did not make his appearance. Here we see the true *emotion*, namely, the association of pleasure with the *idea;* and the manner in which the *desire* would spring out of it. The desire, in her then condition, would be inoperative in causing voluntary movement for its gratification; simply because there was no intellect for it to act upon. Her mental powers, however, were gradually returning. She took greater heed of the objects by which she was surrounded; and on one occasion, seeing her mother in a state of excessive agitation and grief, she became excited herself, and in the emotional excitement of the moment suddenly ejaculated, with some hesitation, "What's the matter?" From this time she began to articulate a few words; but she neither called persons nor things by their right names. The pronoun "this" was her favorite word; and it was applied alike to every individual object, animate and inanimate. The first objects which she called by their right names were wild flowers, for which she had shown quite a passion when a child; and it is remarkable that her interest in these and her recollection of their names should have manifested itself at a time when she exhibited not the least recollection of the "old familiar friends and places" of her childhood. As her intellect gradually expanded, and her *ideas* became more numerous and definite, they manifested themselves chiefly in the form of *emotions;* that is, the chief indications of them were through the signs of pleasure and pain. The last were frequently exhibited, in the attacks of insensibility and spasmodic rigidity, which came on at the slightest alarm. It is worth remarking that these attacks, throughout this remarkable period, were apt to recur three or four times a day, when her eyes had been long directed intently upon her work; which affords another proof how closely the emotional cause of them must have been akin to the influence of sensory impressions, the effects of the two being precisely the same. On one occasion, being alarmed by a stranger, she had quite an hysterical paroxysm, followed by insensibility; and in consequence she lost her speech, taste, and smell (which she had been gradually recovering) for some days afterwards. The mere sight of the same person again (evidently calling up the first disagreeable impression) was followed by a scream and excessive agitation.

The mode of recovery of this patient was quite as remarkable as anything in her history. Her health and bodily strength seemed completely re-established, her vocabulary was being extended, and her mental capacity was improving, when she became aware that her lover was paying attention to another woman. This idea immediately and very naturally excited the emotion of jealousy; which, if we analyse it, will appear to be nothing else than a painful *feeling* connected with the *idea* of the faithlessness of the object beloved. On one occasion this feeling was so strongly excited, that she fell down in a fit of insensibility, which resembled her first attack in duration and severity. This, however, proved sanatory.

"When the insensibility passed off, she was no longer spell-bound. The veil of oblivion was withdrawn; and, as if awaking from a sleep of twelve months' duration, she found herself surrounded by her grandfather, grandmother, and their familiar friends and acquaintances, in the old house at Shoreham. She awoke in the possession of her natural faculties and former knowledge; but without the slightest remembrance of anything which had taken place in the interval

from the invasion of the first fit up to the present time. She spoke, but she heard not; she was still deaf, but as she could read and write as formerly, she was no longer cut off from communication with others."

From this time she rapidly improved, but for some time continued deaf. She soon perfectly understood by the motion of the lips what her mother said; they conversed with facility and quickness together, but she did not understand the language of the lips of a stranger. She was completely unaware of the change in her lover's affections which had taken place in her state of second consciousness; and a painful explanation was necessary. This, however, she bore very well, and has since recovered her previous bodily and mental health. We commend the attentive study of this interesting case (of which we have only given the leading features) to our readers, as illustrating the condition of a human being completely destitute as it would seem of cerebral power, and having only two sources of sensorial change, but these two in active operation, and serving as the direct stimuli to consensual movements, having associated with them also the simple feelings of pleasure and pain. As her power of forming ideas returned, pleasure and pain were associated with these ideas, constituting emotions; and as the higher powers of the intellect were not yet called into operation, these emotions were the dominant springs of her conduct, acting without the control of the reasoning processes, to which in the well-regulated mind they are subject, serving only as motives to them. Numerous cases might be alluded to, which present the same general features; but we know of none in which the details have been so carefully watched and recorded. Thus there is one mentioned by Dr. Rush, of a man who was so violently affected by some losses in trade that he was deprived almost instantly of all his mental faculties. He did not take any notice of anything, not even expressing a desire for food, but merely taking it when it was put into his mouth. A servant dressed him in the morning, and conducted him to a seat in his parlour, where he remained the whole day, with his body bent forward, and his eyes fixed on the floor. In this state he continued nearly five years, and then recovered completely and rather suddenly. There is also the well-known case of the sailor who was reduced to this condition by a fracture of the skull with depression; he remained in it for some time; and at last was immediately restored by the elevation of the depressed bone, which was effected by Mr. Cline. The Cretins of the first degree, also, are nearly in the same state. They spend their time in basking in the sun or in sitting by the fire (experiencing merely sensorial pleasure) without any traces of intelligence; and show no higher sensibility to the common wants of nature, than is evinced by their going when excited by hunger to places where they have been accustomed to receive their food. This, it will be observed, is a grade in the scale of intelligence somewhat above that exhibited in the cases already quoted. The condition of the Cretins of the second degree, who are more susceptible of education and can form simple ideas on the most common subjects, presents a remarkable parallelism with that of the young woman whose case we have been analysing, during the latter part of the time that preceded the second fit. And there is this further point of resemblance; that in this condition, the emotions and propensities, formed by the attaching of pleasure or pain to these ideas, are peculiarly strong, not being kept in check by any superior power.

We think it will be obvious to those who have followed us through our previous investigations, that phenomena of this kind afford very strong additional evidence in favour of our view of the functional distinctness of the sensory ganglia in man. The idiot mentioned by Dr. Rush, and the sailor on whom Mr. Cline operated, present all the psychical characters of M. Flourens' pigeon, from which the hemispheres had been removed. They were less alive to external objects, however, than the Cretins and Mr. Dunn's patient; in whom the sensory ganglia were evidently in a state of higher functional activity, and who obviously felt pleasure and pain in connexion with their sensations, these feelings manifesting themselves in the actions which they instinctively prompted. The formation of true emotions, as soon as ever ideas were generated with which feelings could be connected, is another point which seems to us to derive striking illustration from the phenomena we have adverted to. It is worth noticing in regard to the Cretins, that their various grades of idiocy correspond very closely to the successive stages of the development of the intellectual powers, which we find in the lower animals; and that the fact of the possession of full sensorial power with complete absence of intellectual, also harmonizes well with the fact that the sensory ganglia are fully developed, at a time when the cerebral hemispheres have scarcely made their appearance; so that their condition would seem to be one of pure arrest of development. We should not expect to find a complete absence of cerebrum in such cases; but we doubt not that an attentive examination would discover some imperfection, which suffices to prevent it from duly performing its functions.

We may seem to have wandered far from our original subject, in entering upon the preceding disquisition; but we have been desirous of laying the foundation for a new and more discriminating investigation of abnormal mental phenomena; and we have thought it best to commence with those cases in which the cerebral action is either suspended altogether, or is weak and inefficient, whilst the sensorial is unaffected. In most forms of imbecility, either congenital or acquired, this will be found, we believe, to be the general state. There does not seem to be an *error* of perception, so much as an *inability* to perceive, consequent upon deficiency of these powers of memory, comparison, &c., on which the formation of definite notions of external objects are dependent. This deficiency may not be complete, but may extend only to particular classes of objects; or it may not exist so far as external objects are concerned; being only manifested in the want of power to reason upon abstract ideas.

But there is also a certain class of imbeciles, in whom—to use the language of Hoffbauer—the *intensity* predominates over the *extensity;* that is, the mind is too much occupied with its own thoughts, and too little attentive to external objects. This is simply "absence of mind" carried to an excess, and constant instead of occasional; and the state appears to us to correspond closely with that of dreaming and some forms of somnambulism. The cerebrum is active, but feeds as it were upon its own thoughts; the sensory ganglia not being readily acted on by external impressions, nor easily communicating these to the intellect, which consequently remains isolated as it were from the external world, and cannot be brought *en rapport* with its condition.

We shall not enter at present into the analysis of the various forms of

insanity, which result from perversion of the regular functions of the sensory ganglia and cerebrum, but shall confine ourselves to one point, on which M. Moreau dwells at great length, and which is in fact the chief "argument" of his book; namely, that in cases of monomania the delusion, where it exists, is purely intellectual, and is not induced by the exaltation of the particular moral feeling or emotion with which it seems to be connected. Our readers are doubtless aware that much discussion has taken place upon this question; the prevalent idea having been of late, that the disordered moral state is usually the cause of the illusion. The views we have propounded on the nature of the moral feelings tend to harmonize these conflicting doctrines, and to show that both are true in a certain sense. For if we consider a moral feeling to be (as its name almost implies) a simple feeling of pleasure or pain connected with a particular class of ideas, we see that where any such emotion exists in an exaggerated degree, it *must* involve an undue predominance of a certain class of ideas, which will manifest itself in the whole intellectual state of the individual, and in the interpretation he will put upon the occurrences going on around him. Thus, when we say that the depressing emotions (as is a very common occurrence in these days of eager competition with those who have overtasked their mental powers) are unduly predominant, we mean that painful ideas fill the mind; the individual looks at the past, present, and future in a gloomy light; he is thus led to give every possible unfavorable interpretation to the actions of others, even his dearest friends being in league to injure him; in a further stage of the malady, the patient's notions of actual occurrences are distorted by the erroneous perceptions which result from his habitual tendency to misinterpretation; and if the condition be still more exaggerated, he comes to believe in the reality, not merely of his view of the feelings of others towards him (judging of them by a reasoning process, in which the influence of his disordered feeling is sufficiently apparent); nor only of his erroneous interpretation of occurrences which have actually taken place, but of which the unpleasant character is entirely due to his habitual train of thought; but also of supposed occurrences, which have never had even the remotest foundation in fact, and which are altogether the products of his own disordered imagination. Thus we see that the tendency to indulge in a certain class of ideas, which is in itself purely intellectual, and the feeling which, when connected with those ideas, gives them their emotional character, are usually both concerned in the formation of hallucinations; and if our view is correct, it will be necessary to modify in a considerable degree the definitions now in vogue. As an illustration of how completely the true *propensities*, as well as the proper emotions, are dependent upon the formation of ideas, we may adduce an anecdote of a man who was strongly affected with the destructive tendency; and when restrained from doing mischief, he would manifest the pleasure he felt in dwelling on the idea, in the continual use of the words "crush," "smash," &c., which afforded a harmless vent to the passion that was struggling in his mind, at the same time that it indicated its nature. We now quit the subject for the present, to revert to it on some future opportunity.

PART SECOND.

Bibliographical Notices.

ART. I.—*Dr. Underwood's Treatise on the Diseases of Children; with Directions for the Management of Infants.* Tenth Edition, with Additions. By HENRY DAVIES, M.D., Senior Physician to the British Lying-in Hospital.—*London,* 1846. 8vo, pp. 596.

NONE of our readers, however old, or however young, but must be familiar with the title, at least, of this book. 'Underwood on Children' was the standing authority on its subject, in the days of our youth, and much pains have been taken, at different times, to keep the standard up to the level of the requirements of the passing day. Dr. Davies is the third editor and annotator of the book in our time,—Drs. Merriman and Marshall Hall having been his immediate predecessors. The present edition contains all the notes of these two gentlemen, as well as a large number of the editor's own, who moreover tells us that "in the conduct and production of the work he has received the assistance of the late Dr. Domeier, Dr. Klein Grant, and Dr. Sayer." Now, when it is considered that all, or nearly all the notes of the former editors, and all those of the present editor and his coadjutors, are *embodied in the text,* and that Dr. Davies, as he himself tells us, has, moreover, "deemed it advisable to adhere to Dr. Underwood's arrangement and, for the most part, also to retain his language and opinions," it will not surprise the reader, who will try to imagine a book so constructed, to learn, as he learns from the advertisement, that it contains "varieties of style and language, as well as some incongruities especially relative to the use of pronouns." And, sure enough, the pages of the volume do occasionally present such a composite character of structure, both as regards language and doctrines, as reminds us rather more of the coat of Joseph, than of the temples of Athens. We are sorry that Dr. Davies yielded to the wish of the publishers to have the original treatise reproduced, under any modification of plan and form, and did not devote the many precious hours that this mosaic must have cost him, to the task of giving us an original work on the same subject. This is a task for which his long experience had well prepared him, and which many of the excellent living notes he has here thrust into the carcase of old Underwood, prove he was well qualified for executing. But *dîs aliter visum*; the gods of Paternoster row and Princes street, doubtless, would not forego *the telling Title;* and hence we are startled with yet another metempsychosis of the dear defunct partner of our youthful studies.

Nevertheless, and notwithstanding, we make the old friend with a new face welcome. With all its faults of form and substance, the book as it

now stands is far from a bad book after all:—"unswept" as it is, and "besmeared with sluttish time," it contains a great deal of matter of real and substantial value, which will prove of permanent interest, not merely to the young, but to the old practitioner. With an amiable modesty which it would be well if some others could imitate, all that the editor says of his book is, that he "hopes that it will be found an improvement upon former editions, and a good practical treatise." We beg to assure him that he may justly take the credit of having done much more for the original, than the other editors did; and consequently, that, as it now stands, the book is, in our opinion, vastly superior to the preceding editions. Nay more—due allowance being made for its inherent and unavoidable defects,—we think Dr. Davies is entitled to claim for it the character of "a good practical treatise."

Dr. Davies has enriched the volume with many excellent practical observations of his own, and has drawn copiously from the stores of recent English writers. His own additions are all of a plain, unpretending practical character, conveying useful information much in the style in which Underwood himself would have given it. We regret that our present available space will not allow us to give even a single specimen of this new matter; but we can assure our readers that wherever they find the initials H. D. appended to an interpolated passage, they will find it worth perusal. By the way, it is a great defect in the volume that it is unprovided with an INDEX, as this is particularly needed in a book of such heterogeneous materials, and these so inartistically put together. A copious alphabetical index would have gone far to make up for the want of classification and arrangement inseparable from the plan on which the work has been composed. Among the many passages we had marked as bearing the stamp of Dr. Davies's own practical hand, we would refer the reader, for favorable specimens, to pages 186 and 313. He will there find excellent observations on two important diseases, "Spasm of the Glottis," and "Typhus or low fever of Children." Many others of a similar kind will strike the eye of the practised reader as he turns over the many-coloured pages now before our own; and we believe that few practical men whose business it is to treat the diseases of children, will fail, after even a slight examination, to add the volume to the shelf where he has deposited the works for daily reference in the exigencies of practice.

ART. II.—*Chemistry of the Four Seasons.* By THOMAS GRIFFITHS.—*London*, 1846. pp. 495.

THIS little volume combines, in an emiment degree, amusement with instruction. It contains a discussion of the chemical, physical, and vital phenomena characteristic of the four grand periods or seasons of the year; of those changes which ever recur with the varying amount of light and heat, depending on the varying obliquity of the solar rays. The author states that the chief object of his essay is to adduce a few of the principal phenomena which admit of explanation and illustration through the medium of chemistry; and he has well executed his task. Commencing with a popular description of those elementary bodies which play an im-

portant part in the economy of Nature; which alike form the constituents of a fertile soil, and the atmosphere, of plants, and of animals; he proceeds to the description of the chief phenomena of vegetation in its chemical relations. His statements are illustrated by the details of the more simple experiments on which they are grounded, and these are accompanied by appropriate diagrams. The laws and properties of those wonderful and mysterious agents—heat, light, electricity, galvanism, and magnetism, are appropriately discussed, and their influence on vegetation noticed.

Taken as a whole, we may say with confidence that this volume illustrates in a simple, popular, and amusing manner, the chemical physiology of plants, and completely fulfils the author's aim in writing it, viz., the communicating the more important facts of agricultural chemistry, in an agreeable manner, and unclogged by the deeper investigations and speculations of science. Any person ignorant of the facts on which our knowledge of the chemistry of vegetation is founded, and of the laws which regulate the chemical changes constantly going on in plants, will do well to possess himself of this volume; for by an attentive perusal, aided by a repetition of the experiments there indicated, he will prepare himself for the study of more extended and profound works on the same subjects. We would especially recommend it to youths commencing the study of medicine, both as an incentive to their natural curiosity and an introduction to several of those branches of science which will necessarily soon occupy their attention. We would notice further, and with commendation, that a sound and rational natural theology is spread through the whole work.

Art. III.—*A Practical Treatise on the Diseases peculiar to Women.* By Samuel Ashwell, M.D., &c. Second Edition.—*London,* 1846. 8vo, pp. 737.

In our Journal for April, 1845 (vol. XIX, p. 345), we gave so favorable an opinion of Dr. Ashwell's 'Treatise on the Diseases of Women,' that the speedy appearance of a second edition was far from surprising us. On the contrary, we looked upon this as a matter of course, and accordingly laid the new volume on our table with great satisfaction. Happening, however, to place the old and new copies together, and to look at the last leaf of both, we were struck with the oddness of the identity of the two numerations, and the linear coincidence of the two final pages—each was page 737, and each page 737 contained eleven lines! This roused our suspicions—roused our well-known editorial keenness of scent after pseudo-second editions—and we then proceeded to compare the old and new volumes more carefully. Advancing from the end of the books towards the beginning, we soon had demonstrative evidence, in identity of type, spacing, capitals, italics, catch-words, literal errors, &c., &c., that the two portions we were examining were precisely the same! And as these evidences of identity continued till we had got over more than two thirds of the book, we naturally concluded that we had here a most flagrant instance of that trick of publishers, which we had already, more than once, exposed,—the palming on the public an old book with a new title-page, as a new edition, in a word, a pseudo-second-edition. And yet our conclusion was not

altogether correct, as we at length arrived at a point, namely page 208, where the evidences of identity ceased, and marks of obvious difference presented themselves. And so it turns out, that this so-called second edition of Dr. Ashwell's treatise is only a second edition as far as page 208, all the remaining 529 pages being the identical pages, words, letters, of the edition printed and published and sold to us and to our readers in 1844. Now, as this new volume bears on its title-page the words SECOND EDITION, and as the author gives no hint in his preface (which is also headed *Preface to the* SECOND EDITION) that it is *not* a *bonâ fide* reprint throughout, we think we have just grounds of complaint against both the author and publisher, for thus tacitly allowing the public to be misled in fancying they are really buying a complete new edition of a book, when they are in fact only buying a new edition of less than one third of a book. How this curious thing comes about, we know not, and we care not; but come about as it may, we denounce it as wrong, and we feel that it is our duty thus publicly to expose it. We hope and believe that Dr. Ashwell is no party—or at least, if a party, an innocent party—in this transaction; but it is his duty to explain its precise character to the profession.

As a necessary result of this Mezentian mode of editing, we find some odd little anomalies in reading the old part of the volume with reference to its ostensible date of publication. For instance, at page 280 the profession are justly declared to be "under great obligations to Dr. Walshe for one of the ablest and most complete essays on cancer ever published;" and a warm hope is expressed "that no long time will elapse ere its accomplished author presents it to the medical world as a *distinct work*" (reference being made to the 'Cyclopædia of Surgery'); the truth being, as our readers well know, that this hoped-for *work* was actually given to the public one full year before this notice of it was professedly penned; Dr. Walshe's preface bearing date November, 1845, and Dr. Ashwell's November, 1846.

ART. IV.—*The Health of Towns as influenced by defective Cleansing and Drainage; and on the application of the Refuse of Towns to Agricultural Purposes: being a Lecture delivered at the Russell Institution, May* 5, 1846. By WILLIAM A. GUY, M.B. Cantab., &c. &c.—*London*, 1846. 8vo, pp. 48.

DR. GUY, in this pamphlet, very lucidly sets forth the advantage and economy of a better system of civic drainage. The gross value of the manure annually wasted amounts, he thinks, to 10,000,000*l.*; the health-tax inflicted upon the population by sewerage defects he estimates, for the United Kingdom, at nearly 20,000,000*l.* If we halve and quarter these sums, after the approved method of estimating an heiress's fortune, there remains a very handsome sum; and we certainly think that several millions sterling are annually wasted in the way Dr. Guy points out, although we hardly think his numerical estimate will be received as being mathematically accurate. Much more investigation is required on the points Dr. Guy moots in this essay, before ACTION will arise. The public will thank him, we trust, for being willing to lead in the van in an attempt at great social improvement.

Art. V.—*Fever physiologically considered; considerations on Yellow Fever, Typhus Fever, Plague, Cholera, and Sea-Scurvy; also the Questions of Contagion and the Quarantine Laws; with an Address to the Public, &c. on the popular Treatment of Cholera.* By David M'Connell Reed, Esq. Licentiate of Medicine &c.—*London,* 1846. Small 8vo, pp. 262.

Dr. M'Connell Reed has taken great pains with his book, or at least with that part of it which refers to fever. The history and whole pathology of the different forms of fever are considered at length and in minute detail; but, we are sorry to say, most unsatisfactorily.

With regard to the second part of the work we cannot express any favorable opinion. It is devoted to a consideration of the nature, cause, and treatment of epidemic yellow fever, typhus fever, plague, cholera, and sea-scurvy, the latter disease being most erroneously considered identical with the others mentioned.

The appendix is an address "to the humane public, and to the honorable members of the medical profession" on the near advent of Asiatic cholera, and on the method of treating it. The author appears to be possessed of some talent, but it is sadly obscured by singular religious expressions, and by curiously new terms. For example, in his physiological estimate of the symptoms which depend on disorder of the brain and nervous system, Dr. Reed observes,—

"Fainting or syncope depends on suspended or probably retrograde action of the capillary vessels of the fibro-serous membrane of the brain; whence result, loss of the mento-organic faculty of sensibility, and of the animo-organic function of sensation; the mento-muscular faculty of mobility-at-will, and of the animo-organic function of voluntary motion; the total suspension of the animo- or mento-organic faculties, &c." (p. 57.)

Doubtless Dr. Reed uses these terms with a definite meaning, but we think he ought to have added a dictionary or glossary to his book.

With regard to the prophetic and religious expressions of the author, we shall content ourselves with quoting the following piece of presumption:

"The author confidently predicts a dreadful visitation of the cholera at the close of summer, or at the beginning of autumn, most likely in the month of August, to be succeeded by a desolating outbreak of typhus fever. He moreover predicts that those who will fall victims to these diseases are the *very highest* and the *very lowest* classes of society—those who, in consequence of their imprudence and profligacy, have induced want and starvation, with its accompanying miseries; and those who, in consequence of their avarice, gluttony, intemperance, and inhumanity, have induced an unhealthy, plethoric state of the system, coupled with an *uneasy conscience.* The author thinks he could go farther, and point out some of the individuals on whom this judgment will fall; but he forbears, leaving *secret things to the Allwise Judge, and Disposer* of all *events* both in heaven and earth." (pp. 225-6.)

Dr. Reed further trusts much, as we learn, to "the efficacy of faithful prayer, vested in the church," relief in the coming troublous times.

Our author seems to be a man mainly of *three* books; Thomas's 'Practice of Physic;' Hooper's 'Medical Dictionary;' and the 'Bible.' The latter affords mottoes; the first two numerous extracts. We cannot approve of the mixture of sacred and profane learning; the assumption of the power of prophecy, the judging of others; and the praying in public (on the sheets of his book) displayed by Dr. Reed. He should

"heartily thank God" in his closet and not on the wings of the press; and he should remember that divine law, "judge not, that ye be not judged," and not hint dark suspicions respecting his neighbours. There are evidently some elements of power in Dr. Reed's mind, but to use them aright he must undergo a course of mental discipline, or at least should learn to restrain his philological vagaries and check the exuberance of his religious imaginings.

Art. VI.—1. *Introductory Discourse on the mode of Investigating the Sciences belonging to the Medical Profession, delivered in the Theatre of St. George's Hospital, October* 1, 1846. By Sir Benjamin C. Brodie, Bart., &c.—*London*, 1846. 8vo, pp. 13.

2. *On Medical Education: being a Lecture delivered at King's College, London, at the opening of the Medical Session*, 1846-7, *to which is added a Lecture delivered on the same occasion in the year*, 1842. By William A. Guy, M.B., Cantab., Professor of Forensic Medicine, King's College, &c.—*London*, 1846. 8vo, pp. 64.

3. *The Motives of Industry in the Study of Medicine: an address delivered at St. Bartholomew's Hospital, on Thursday, October* 1, 1846. By James Paget, F.R.C.S., Warden of the College and Lecturer on Physiology in the Hospital.—*London*, 1846. 8vo, pp. 30.

1. That the readers and auditors of a long succession of introductory lectures should turn away with a sort of frigid aversion from either the sight or hearing of another, is not, perhaps, very unnatural, when we consider the elements of which they are usually composed and the small amount of variety which is generally exhibited in their construction. But there is that about a superior mind which displays itself in the mode of discharging even the most formal duties or expounding even the most common-place truths. In the three "introductories" before us, we recognize as many exemplifications of this fact, for each is distinguished by some excellence that renders it worthy of special notice. The Discourse of Sir B. Brodie is expressly addressed to the pupils of the school of which he has been so long an ornament; and it exhibits, like all his addresses on medical and surgical topics, his strong practical sense, and his earnest desire that his hearers should be trained in habits of correct observation and logical deduction, and that they should acquire the knowledge under the guidance of which alone the reasoning powers can be rightly exercised upon the phenomena of life and especially of disease. The peculiar excellence of this lecture consists in its plainness and simplicity, in the aptness of the illustrations afforded by its author's vast professional experience, and in the soundness of the advice which he has imparted to his young friends—equally applicable, however, to those in all periods of life who desire that their advance in knowledge should be coeval with its duration, as to the method in which they should study the complex problems that present themselves for investigation.

2. Dr. Guy's Lecture is chiefly remarkable for the force with which the introduction of scientific study as a branch of general education, and more especially as a preliminary to the prosecution of medical study, is advocated, both on the ground of its direct utility, and of its beneficial influence on

the intellectual and moral faculties. We need not say that this view is most completely accordant with that which we have repeatedly urged ; and it gives us the greatest pleasure to find it so well enunciated in a college whose conservative tendencies might be supposed to make it look with a suspicious eye upon any show of a disposition to undervalue the pursuits of literature, or to substitute for the poetic contemplation of the past the practical claims of the present and the anticipated results of the future. That before science could be said to have an existence, and when all philosophy was to be found in the writings of the ancients, literature should be the chosen means of developing the higher powers of the intellect, is not surprising ; but that those who regulate the higher educational establishments of our country should still maintain so firm an adhesion to the wisdom of their ancestors, and should keep their eyes closed to the benefits which can be proved to result from an alteration of that system, speaks forcibly, we venture to think, as to their unfitness for the responsible trust of which they hold such firm possession.

3. The title of Mr. Paget's Lecture, whilst it truly indicates the principal subject of his address, does not convey an idea of the elevated manner in which that subject is treated; and we cannot deny ourselves the pleasure of quoting one or two passages which will show the high tone of feeling that he labours, and we hope not unsuccessfully, to excite in his pupils. After speaking of the peculiar demand for the energetic and persevering use of our best faculties which the study of medicine involves, he thus continues :

"Yield to the demand, and give them. For there is not in this world a nobler spectacle than that of a rational being devoting himself, with patient, earnest perseverance, to the cultivation of his powers, that they may be employed in the discharge of duty Knowing that a force within him is capable of unlimited expansion, and confessing in his inmost consciousness, that its development and its exercise are duties of strongest obligation, he pauses not to ask whether outward reward will crown his work or not ; much less, with scrupulous calculation, does he count the cost and gain. But, because he knows that the powers and opportunities he has received were given him for use, he resolves that not one of them shall run waste or wild : for him, to be indolent, were to be unthankful. And so, in toil, yet not in weariness, he pursues his way ; sowing seed, of which he reckons not whether he shall reap the fruit ; content, because he is in the path of duty ; blest, if only he may see or think that he ministers to the welfare of his fellow-men. (p. 11.)

But the labour of acquisition, if rightly directed, is in itself a pleasure, as Mr. Paget eloquently urges. After pointing to the movements of animals as signs of the pleasure of energy, he continues :

"Now, there is an exact analogy here between the mental and the bodily faculties. In health, every exercise of the mind, provided it be voluntary and natural, is a true source of pleasure. And, therefore, we should count it as a privilege, that the pleasures of intellectual activity are offered to us in all their various forms in the study of our science ; for therein, we may always have the calm and abiding satisfaction which attends the gradual acquirement of knowledge ; and, not seldom, that intenser pleasure which is perceived when difficulties, long striven against, are overcome ; and sometimes, if we carry our researches beyond the limit of that which is already known, we may enjoy the same excitement and expectation as others pursue in more perilous adventure ; and, then, we may attain the thrills of delight which accompany the first perception, and the slow unfolding, and, at last, the clear and perfect view, of some new truth or principle. And all these pleasures we may enjoy as long as we continue our study ; for the

science is inexhaustible, and the pleasure becomes more intense in the same proportion as the faculties that are exercised are higher, and as the mind is more guided and illustrated by knowledge." (pp. 16-7.)

The responsibilities of the practitioner are equally well set forth in the pages immediately preceding that last quoted from. We greatly regret that we can find room for only a portion of the beautiful passage:

"We sometimes see the beam of life and death so nearly balanced, that it turns this way or that, according to the more or less of skill that can be cast into the scale of life. And surely, if we could gather into thought all the issues that are involved in the life or death of any man, the anxiety of ignorance at such a time should be intolerable. For at all such times, the issues and the responsibilities are manifold; it is not alone the fate of the sufferer (though in that, indeed, may be the most fearful consequence of all), but, as each of us must have felt in some instance very near to his own heart, those that stand around have all their various griefs and fears, their hopes, yet sad forebodings. And now, all is permitted to depend upon the skill of one. Conceive that one yourself: what would be your remorse, if, when in their confusion and distress they look to you, you feel helpless as themselves, utterly unworthy of the confidence with which they still lean on you; your hand paralysed by the fear of ignorance, your mind confused in that half-knowledge, whose glimmerings only show that more skill might save the dying man! Yet this must be the remorse of every one who will neglect the study of his profession, and yet dare to undertake its responsibilities." (pp. 13-4.)

"Do not imagine that your responsibilities will be limited to the events of life or death. As you visit the wards of this Hospital, mark some of the hardly less portentous questions which, before a few years are past, you may be permitted to determine. In one, you will find it a doubt whether the remainder of the patient's life is to be spent in misery, or in ease and comfort; in another, whether he, and those who depend upon his labours, are to live in hopeless destitution, or in comparative abundance. One who used to help his fellow-men, finds ground to fear that he may be a heavy burthen on their charity. Another counts the days of sickness, not more by pain and weariness, than by the sufferings and confusion of those who are left at home without a guide, and, it may be, starving. Oh! gentlemen, I can imagine no boldness greater than this would be, who would neglect the study of his profession, and yet venture on the charge of interests like these; and I can imagine no ambition more honorable, no envy so praiseworthy, as that which strives to emulate the acquirements of those who are daily occupied in giving safe guidance through the perilous passages of disease, and who, in all these various difficulties and dangers, can act with the energy and calmness that are the just property of knowledge." (pp. 14-5.)

Art. VII.—*A Manual of Materia Medica and Therapeutics, including the preparations of the Pharmacopœias of London, Edinburgh, and Dublin, with many new Medicines.* By J. Forbes Royle, m.d. f.r.s., Professor of Materia Medica and Therapeutics, King's College, London. With ninety-eight woodcuts.—*London,* 1847. 8vo, pp. 716.

This is another member of that beautiful and cheap series of Manuals published by Mr. Churchill for the benefit of the student and general practitioner. In the execution of the woodcuts, particularly of the plants, flowers, and fruits, Mr. Bagg seems almost to have exceeded his former doings, while the paper-maker and printer have performed their parts with equal credit. In regard to the yet more essential constituent, the literary portion of the work, no one who is acquainted with the former productions of Dr. Royle will doubt that the author has discharged his duties with the same skill as the artist. The work is, indeed, a most valuable

and impressiveness of an original production. We regret that we have received the book at so late a period as precludes our giving more than a mere notice of it, as although essentially and necessary a compilation, it contains many things which we should be glad to reproduce in our pages, whether in the shape of new pathological views, of old errors corrected, or of sound principles of practice in doubtful cases clearly laid down. But we dare say most of our readers will shortly have an opportunity of seeing these in their original locality, as we entertain little doubt that this book will become what its author hoped it might become, a manual for daily reference and consultation by the student and the general practitioner. The work is marked by that correctness, clearness, and precision of style which distinguish all the productions of the learned author.

Art. X.—*On the Correlation of Physical Forces: being the substance of a Course of Lectures delivered in the London Institution in the year* 1843. By W. R. Grove, Esq., M.A. F.R.S., Barrister at Law. Published at the request of the Proprietors of the London Institution.—*London*, 1846. Royal 8vo, pp. 52.

This pamphlet gives formal expression to a view which has long been gaining ground amongst philosophers in this and other countries, that we must discard altogether the old notion of "imponderable forms of matter," as a clumsy hypothesis invented to elude a difficulty which could not be solved in an earlier condition of scientific inquiry; and that we are to consider light, heat, electricity, magnetism, and chemical affinity, as *affections* of ordinary forms of matter, or manifestations of their properties. And further, that these agencies are all *correlative;* that is, they possess such a reciprocal dependence, that either of them may, as a force, produce or be convertible into another. We cannot here stop to investigate the principal arguments upon which Mr. Grove bases this conclusion; but we may state that they appear to us to be entirely satisfactory, and that we cannot refer to any brief exposition of the question which gives so lucid and complete a view of it as that which is contained in the pamphlet before us. Since its publication, (as many of our readers are doubtless aware,) a discovery has been announced by Mr. Grove himself, which goes very much to strengthen his argument; namely, that heat, like electricity, has the power of *decomposing* water, as well as of causing the union of its elements, according to the mode in which it is applied. For if a pure platinum wire, immersed in water, be kept at the highest temperature at which it can be maintained, short of fusion, the surrounding liquid will be resolved into its constituent gases, just as when a galvanic current is passed through it. And yet at a temperature a very little lower, the very same heated wire would cause the reunion of the two gases which it has separated, in the same manner as they may be made to combine by transmitting an electric spark through the mixture.

On one point Mr. Grove has gone beyond other philosophers who have taken up the same line of speculative reasoning; for he has included motion among the agencies which are thus correlated. At first sight it might appear to be altogether removed from them; but he gives some very ingenious arguments, founded upon the phenomena of friction, to the effect that motion, if checked, produces heat; whilst, as is well known, heat pro-

duces motion. Thus, if these two agencies be mutually convertible, the correlation of motion with all the rest is established, through the intermediation of heat. Further, when friction takes place between heterogeneous substances, electricity is developed; and thus another link is afforded between motion and the other agencies, any of which may be excited by electricity, and most of them by heat.

"I have said that either of the forces above mentioned may, *mediately* or *immediately*, produce the others; and this is all I can venture to predicate of them in the present state of science; but I will venture as an opinion, founded after much consideration, that science is rapidly progressing towards the establishment of immediate or direct relations between all these forces. Where at present no immediate relation is established between any of them, electricity generally forms the intervening link or middle term." (p. 13.)

In regard to light, Mr. Grove embraces a view which was long since suggested by the eminent Euler, and which appears well worthy of being thoroughly tested by mathematical research; namely, that light is not, as in the Newtonian hypothesis, an emanation of material but imponderable particles; nor, as supposed by the partisans of the undulatory theory, the result of the vibrations of a hypothetical ether; but that it results from a vibration of the molecules of matter itself, just as sound is propagated by the vibrations of wood, or as waves are by water. The striking analogies between sound and light, as regards their mode of transmission, are all in favour of such a view: and it is no objection to it to say that a vacuum still permits the transmission of light; since it may safely be affirmed that no real vacuum has ever been produced; and there is fully as much reason to presume the existence, even in the spaces that intervene amongst the heavenly bodies, of matter (in the ordinary sense of the term) in a very finely-divided condition, as there is to suppose that this space is occupied by the luminous particles of Newton's imponderable, or by the luminiferous ether of more recent times.

Altogether this outline of Mr. Grove's views is one which is most fertile in topics of interesting speculation; whilst it shows how closely such speculations may be connected, by a logical mind, with phenomena obvious to our senses. We commend its study, to those who are capable of mastering it, as an example of the mode in which we trust that the phenomena of vitality may be ultimately generalized; when the same preparation shall have been made, by patient and accurate research, for the development of analogous views.

Art. XI..—*Urinary Deposits; their Diagnosis, Pathology, and Therapeutical Indications.* By Golding Bird, A.M., M.D., F.R.S., &c. Second Edition.—*London*, 1846. 8vo, pp. 356.

It is only eighteen months since we gave an analysis of the first edition of this valuable treatise (Brit. and For. Med. Rev., July, 1845). The rapid sale of the work has confirmed the justness of our high opinion of it, as there expressed. So short a period has elapsed since the book was first published that the author has found little room for the addition of new matter; but it is evident that this edition has been carefully revised and improved in various particulars. It is a book which ought to be in every medical library.

Art. XII.—*A descriptive Catalogue of the Anatomical Museum of St. Bartholomew's Hospital. Vol. I, containing the description of the specimens illustrative of Pathological Anatomy.—London,* 1846. 8vo, pp. 487.

This is a catalogue of a very extensive and valuable collection, and is itself a model of what such catalogues should be. The specimens are arranged in series (thirty-eight in all), each series being preceded by classified tables of reference to the objects contained in it, by which means the advantages of an arrangement, founded on pathological principles, are attained, without interfering with the ordinary and convenient method of displaying preparations. "By the help of these tables it will be easy both to find any specimen in the museum, and to study the preparations in each series in the order in which they may best serve for illustrations of the disease of the part to which that series is devoted." There is also prefixed to the work "a general table of references to the principal specimens for the illustration of general pathology." This is a most valuable key to the student, as by means of it he can at once refer to any of the specimens, however dispersed over the museum, almost as conveniently as if they had been arranged in a separate series for the special purpose of illustrating general pathology. All these tables, and indeed the greater portion of the catalogue, are the work of Mr. Paget; and the whole bears the impress of his master hand.

Art. XIII.—*Records of Harvey: in Extracts from the Journals of the Royal Hospital of St. Bartholomew. With Notes.* By James Paget, Warden of the Collegiate Establishment, and Lecturer on Physiology in the Hospital.—*London,* 1846. 8vo, pp. 44.

We recommend this pamphlet to the attention of all the admirers—and who are not admirers?—of Harvey. It consists mainly, as we are told in the preface, of "literal copies of all that is recorded concerning Harvey in the Journals of St. Bartholomew's Hospital; of which the great discoverer of the circulation—'Physiologiæ lumen, Angliæ immortale decus'—was for four and thirty years physician."

"Some of these records are remarkable as illustrations, both of the life and character of Harvey, and of the medical history of hospitals in the 17th century." Mr. Paget's notes are highly interesting, and add greatly to the value of the original extracts. We cannot resist the gratification of presenting to our readers the following delightful eulogy on Harvey's mother (doubtless composed by her great son), which may still be seen on her tombstone in Folkstone church; it is extracted from Mr. Paget's notes:

"A. D. 1605 Nov. 8th, dyed in y^e^ 50th yeere of her age
Joan, Wife of Tho: Harvey. Mother of 7 Sones & 2 Daughters.
A Godly harmles Woman: A chaste loveing Wife:
A charitable quiet Neighbour: A comfortable frendly Matron:
A provident diligent Huswyfe: A careful tender harted Mother.
Deere to her Husband: Reverensed of her Children:
Beloved of her Neighbours: Elected of God.
Whose Soule Rest in Heaven: her Body in this Grave:
To Her a Happy Advantage: to Hers an Unhappy Loss."

ART. XIV.—*Lectures and Observations on Clinical Surgery.* By ANDREW ELLIS, F.R.C.S.I. &c.—*Dublin,* 1846. 8vo, pp. 275.

THIS volume contains the substance of certain clinical lectures delivered by the author at the Jervis-street Hospital and the Cecilia-street School of Medicine, in Dublin, and was published, he adds, at the expressed desire of some of the pupils who attended the hospital during the session.

The subjects treated of are medical education, wounds of arteries, traumatic aneurisms, injuries of the head and their consequences, peritonitis and wounds of the abdomen, delirium tremens, catalepsy and hydrophobia. These topics are handled with a respectable degree of ability, and though we have looked in vain through the Lectures for anything which can entitle them to be considered as a valuable addition to medical literature, they nevertheless contain directions as to the details of practice such as form the proper staple of a clinical lecture, and which, no doubt, rendered them useful to those to whom they were originally addressed.

Whether such a publication was needed by the "students of Great Britain and Ireland," to whom the author has somewhat ambitiously addressed it, is another matter, and one to which we should be disposed to give a pretty decided negative. We cannot refrain, moreover, from adding that the author's literary researches have proved singularly infelicitous since they led him to the discovery of only *scattered* information in the works of former writers on such subjects as those we have named above. We think Mr. Ellis would have better consulted his own interests and that of his readers in offering his lectures to some one of our Journals than by publishing them in their present form; at all events we will venture to suggest for the future, seeing that his book is now *un fait accompli,* that he would act more wisely before committing himself to the public to consult some judicious senior on the matter, rather than to act on suggestions of admiring pupils.

We observe in his lecture on hydrophobia that the author not only comes to the conclusion that the bite of a dog, not rabid, can produce hydrophobia in the human species, but expresses his belief that the saliva of a dog in perfect health, and in a state of tranquillity when it neither bites nor attempts to bite, may, if applied to a wound, produce hydrophobia in the human subject. This, to say the least, is somewhat strong doctrine, and by no means sufficiently borne out by the evidence adduced, which consists of two or three cases related at second-hand, and without any of those details necessary to satisfy the mind as to the absence of fallacies so likely to exist in a matter of this sort. On this point we beg to refer our readers to an article on the work of Dr. Wright, in the preceding part of the present Number.

ART. XV.—*The Pathological Anatomy of the Human Body.* By JULIUS VOGEL, M.D. Translated by G. E. DAY, M.A. and L.M. Cantab. Illustrated by Ten Plates.—*London,* 1847. 8vo, pp. 587.

IN our last Number we gave a pretty full analysis of the original of this very valuable work, to which we must refer the reader. We have only to add here our opinion that the translator has performed his task in an excellent manner, and has enriched the work with many valuable additions.

one, and will fill up an important gap that existed between Dr. Pereira's most learned and complete system of materia medica, and the class of productions at the other extreme, which are necessarily imperfect from their small extent. Such a work as this does not admit of analysis and scarcely of detailed critical examination. It would, however, be injustice to the learned author not to state that, in addition to what former works on the subject necessarily contained, the reader will find here not a little that is either original, or introduced for the first time, more especially in the details of botany and natural history, and in what may be termed the archæology of drugs.

ART. VIII.—*Principles of Human Physiology, with their chief applications to Pathology, Hygiene, and Forensic Medicine.* By W. B. CARPENTER, M.D. F.R.S., Fullerian Professor of Physiology to the Royal Institution of Great Britain. Third Edition, with two copper-plates and 175 woodcuts.—*London*, 1846. 8vo, pp. 776.

As twice before, on the publication of the first and second editions, we had occasion to notice this incomparable work, we shall at this time content ourselves with simply calling the attention of our readers to the new edition, and stating, in a few words, some of the more important additions we have observed in turning over its well-filled pages. We may say generally of this as of the other works of Dr. Carpenter, that the reader may safely trust to the author's industry and quick-sightedness for supplying him with every novelty relating to his subject up to the day of publication. Nothing escapes his observation and his grasp, and few things are unimproved by his handling—*nihil tetigit quod non ornavit.* In so vast a collection of facts, in such a voluminous code of doctrines, it would be difficult, had we both space and time at command, to give even an outline of the numerous and important improvements, both in the way of additions and alterations introduced into the present edition. The following are some of the novelties (not a few of which are either wholly or in part original) that attracted our notice in comparing the new volume with the old. We put them down in the order of the pages, and we have to apologise both to the author and the reader for the dry index-like form in which we present them:

Application of the simple phenomena of cell-growth in the lowest plants to an explanation of similar process in animals. (pp. 87-9).

Clearer view of the two elements of human hair obtained by comparative inquiries into the structure of hair in other animals. (p. 121.)

Additional light thrown on the structure of bone by the author's investigations on shells, &c., of invertebrata, which show that a skeleton may be formed by the calcification of *fibres* or of *cells*, both of which modes are employed in the production of bone. (p. 141 et seq.)

Mr. Quekett's observations on differences in size and form of osseous lacunæ characteristic of different groups of animals. (p. 143.)

Von Bibra's analyses of bone. (p. 146.)

Dr. Sharpey's researches on the development of bone in membrane by calcification of the fibre, confirmed by the author. (p. 147.)

Professor Owen's researches into the early development of tooth structures. (pp, 157-61.)

Ultimate structure of muscular fibre. (p. 176.)

Clear distinction of the *animal* functions as *destructive* in their exercise, and of the *vegetative*, as *reparative* and *preservative* (Liebig had announced this as true only of muscular action). (pp. 201-2.)

Improvements in the arrangement of the subjects in the chapter on the nervous system. Fuller consideration of sensory ganglia and actions of nerves connected with them.

General sketch of the mental powers. (pp. 371-2.)

Fuller consideration of muscular tonicity. (p. 446.)

Various recent researches on digestion, showing the destination of different articles of aliment more clearly. (pp. 510-11.)

Fuller consideration of the process of sanguification. Chap. XI.

Improved mode of viewing the character of the spleen and other vascular glands. (pp. 520-27.)

Chemical novelties regarding the constitution of the blood and process of respiration.

Goodsir's researches on secretion. (p. 633.)

In conclusion, we can only reiterate our former high appreciation of this work and recommend it in the most earnest manner to every student and lover of the highest and most interesting department of medical science.

Art. IX.—*A Manual of the Principles and Practices of Ophthalmic Medicine and Surgery.* By T. Wharton Jones, f.r.s., Lecturer on Anatomy, Physiology, and Pathology, at the Charing-cross Hospital, &c. With four copper-plates, coloured, and ninety woodcuts.—*London*, 1847. 8vo, pp. 570.

The object of the author in writing this book, and its general character, are so concisely set forth in the preface that we cannot do better than transcribe a portion of it.

"To produce a work on diseases of the eye which should serve at once as a text-book for students, and a book of reference for practitioners, has been the great aim of the author in composing this Manual. Accordingly, besides carefully discussing the principles, he has laboured to give such practical exposition of the subjects as will be found available at the bedside of the patient, and in the operating-room. At the same time, he has not neglected the opportunity which the subject offers, of illustrating the general doctrines of pathology, especially those of inflammation. . . . . . . The author thinks it proper to mention, that he has incorporated in the present volume the various contributions to ophthalmic medicine and surgery, some anonymously, in the course of the last fifteen years, and also that he has freely availed himself of the information contained in the principal works, British and Foreign, on the subject."

We are confident that the reader will find, on perusal, that the execution of the work amply fulfils the promise of the preface, and sustains in every point the already high reputation of the author as an ophthalmic surgeon as well as a physiologist and pathologist. The book is evidently the result of much labour and research, and has been written with the greatest care and attention; it possesses that best quality which a general work, like a system, or manual can show—viz., the quality of having all the materials whencesoever derived, so thoroughly wrought up and digested in the author's mind as to come forth with the freshness

Works published by the Sydenham Society.

Art. XVI.—1. *An Anatomical Description of the Diseases of the Organs of Respiration.* By C. E. Hasse, m.d. Translated and edited by W. E. Swaine, m.d.—*London,* 1846. 8vo, pp. 400.

2. *The Seven Books of Paulus Ægineta.* Translated from the Greek with a Commentary. By Francis Adams. Vol. II.—*London,* 1846. 8vo, pp. 510.

3. *The Works of William Hewson, F.R.S.* Edited with an introduction and notes, by George Gulliver, f.r.s.—*London,* 1846. 8vo, pp. 360.

These three volumes, so beautifully printed and so elegantly got up, are the handsome return which theSydenham Society has made to each of its subscribers for his guinea of 1846, the lowest price of which, in the shops, would be nearer three guineas than two. And yet the beauty of these volumes is their least charm. We have made to ourselves a vow not to analyse any of the works of this Society, and for these reasons: our readers either have the books or they have them not: if they have them they do not require any account of them from us: if they have them not, we do them a service by thus lending our aid to compel them to join the Society and so to possess these volumes and others of like value. Of the works before us, we will merely say that *Hasse's* is a unique treatise on its important subject, much improved by the author's manuscript additions and excellently translated by Dr. Swaine; that the second volume of *Paulus* is no less curious than the first, and displays the same store of classic learning in its accomplished translator, Dr. Adams; and that the great original value of *Hewson's* works is here doubled by the commentaries or notes of Mr. Gulliver. This volume is, moreover, illustrated with all Hewson's plates, finely re-engraved in a reduced size, and has a charming portrait of the author as a frontispiece.

We are happy to learn that this excellent Society is very prosperous, having now considerably more than 2000 members. We trust the Council will proceed in their present course, giving us works of real value without regard to any stupid clamour that may be raised against them. It is their duty to do what they themselves think best for directing and improving the literary and scientific taste of their subscribers generally, not to cater to the unripe judgment and silly wants of the uninformed.

Art. XVII.—*On Diseases of the Skin.* By Erasmus Wilson, f.r.s. Second Edition.—*London,* 1847. 8vo, pp. 482. With Eight Coloured Plates.

In our Number for April, 1843, we gave a critical review of the first edition of this book. Although disapproving of the arrangement adopted by the author, we spoke favorably of the production as a whole. We refer to our previous article for the particulars of our judgment on the treatise generally, and content ourselves, on this occasion, with stating that the work is very considerably improved in the present edition, and is for the first time illustrated with plates. Of these plates it is impossible to speak too highly. The representation of the various forms of cutaneous disease are singularly accurate, and the colouring exceeds almost anything we have met with in point of delicacy and finish.

ART. XVIII. — *Knowledge; an Introductory Address, delivered at the Bristol Athenæum, October* 5, 1846. By JOHN ADDINGTON SYMONDS, M.D.—*London,* 1846. 12mo, pp. 48.

"WHAT is knowledge? How is it obtained? What are its principal divisions? In what spirit should it be pursued?" are the questions which Dr. Symonds has essayed to answer in this address; and notwithstanding its brevity they are most clearly and attractively handled. This lecture, in common with all that proceeds from Dr. Symonds's pen, evinces the high intellectual and moral cultivation, the elegant taste, and the comprehensive acquirements of its accomplished author; and we strongly recommend its perusal to our younger friends, as likely to give them a definite apprehension of the objects of their pursuit, to stimulate their zeal in the laborious studies they have undertaken, and to elevate their ideas of the real value and ultimate purposes of that which they seek to acquire.

ART. XIX.—*The Microscopic Anatomy of the Human Body in Health and Disease, illustrated with numerous Drawings in Colour.* By ARTHUR HILL HASSALL, F.L.S. M.R.C.S., &c. Parts III-V. 8vo, pp. 114. With ten Plates.

WE should not have so soon returned to this publication, but for the sake of redeeming a pledge which we gave in our former notice of it, to speak more favorably of its execution, should the improved character of the work as it proceeded, enable us to do so. It is now our pleasing duty to state, that there is not only a considerable amendment in the plates of the three numbers before us, but that in the fifth part are given seven additional plates, to replace those issued in the early numbers, to which they are vastly superior, both in drawing and colouring. The justice of that part of our criticism which relates to the plates has thus been practically admitted by the author and his artist. It is right that we should further mention that the proportion of figures taken from the human subject is much increased in the later numbers. But we must still express our conviction that the work cannot be comprised within anything like the limits first announced, without doing injustice to a large proportion of the topics it is intended to embrace. For in the five numbers now issued, the animal *fluids* (blood, mucus, pus, milk, and semen) are not completed; and less than seven parts remain, therefore, for the description of all the *solid* textures of the human body. Moreover, of the large amount of letter-press which accompanies the plates, the greater part is occupied by details of a chemical nature, altogether foreign to the subject. The author seems to forget that he is writing about *Microscopic* Anatomy, not *General* Anatomy; and the investigations of Andral and Gavarret, Becquerel and Rodier, &c., on the proportion of the different constituents of the blood, have not a more just place in it than a full discussion of the microscopic characters of the blood would have in Turner's Chemistry. We are still constrained to say that Mr. Hassall appears to have entered upon his work too hastily; and that he seems ignorant of much that is current amongst intelligent microscopists at the present time, his acquaintance with the subject lying too much in the past.

ART. XX.—*Œuvres Complètes d'Hippocrate, Traduction Nouvelle, avec le Texte Grec en regard, collationné sur les Manuscrits et toutes les Editions; accompagnée d'une Introduction, de Commentaires Medicaux, de Variantes, et de Notes Philologiques; suivie d'une Table Générale des Matières.* Par E. LITTRÉ, de l'Institut, de l'Académie des Inscriptions et Belles Lettres, et de la Société d'Histoire Naturelle de Halle.—*Paris,* 8vo, Vol. IV. 1844, pp. xx, 670; Vol. V. 1846, pp. 733.

The Complete Works of Hippocrates, a new Translation, with the Greek Text on the opposite Pages, collated with the Manuscripts and all the Editions; with an Introduction, Medical Commentaries, various Readings, and Philological Notes; followed by a general Index of the matter therein contained. By E. LITTRÉ, &c.

THIS work proceeds somewhat slowly, at least much more slowly than the learned Editor anticipated when seven years ago he published his first volume, and promised that the whole should be finished at the rate of three volumes a year. It goes on however steadily and satisfactorily, which is of more consequence; and we know by the experience of two or three memorable instances in this country that it is not easy to attain the punctuality of the Editors of the *Penny Cyclopædia* (and may we not add) the *Cyclopædia of Practical Medicine,* in works of equal magnitude.

The fourth volume which was published in 1844 finishes the list of the genuine works of Hippocrates, and contains the treatises "De Articulis," "Mochlicus," "Aphorismi," "Jusjurandum," and "Lex." In his "Avertissement" and his "Addenda et Corrigenda" the Editor takes the opportunity of noticing some points in the former volumes which required further consideration, and in some cases, with his usual candour, corrects and modifies his former statements. To each work he prefixes an "Argument," or introduction, in which he discusses (and sometimes at considerable length,) the subject matter of the treatise, and supplies such preliminary information as may be necessary for understanding it more thoroughly. Some of these introductions are extremely interesting, and contain a valuable store of antiquarian information, illustrated and explained by frequent reference to the present state of Medical Science. The volume closes with some "Remarques Rétrospectives," on the character of the genuine works of Hippocrates and the general ideas by which they were inspired.

The fifth volume contains the Second, Fourth, Fifth, Sixth, and Seventh books, "De Morbis Popularibus," "De Humoribus," the first book of the "Praedictiones," and the "Coacal Praenotiones;" all of which treatises are edited, translated, and explained in the same style as the preceding works. One of the most interesting discussions introduced in this volume is that on the meaning of the word ἄνθραξ, which introduces a digression on the history of the Smallpox. (p. 484.) The learned editor not only quotes the chronicles of Marius and Gregory of Tours to prove that this disease had appeared in Europe earlier than is commonly supposed, but also remarks of the plague of Athens, and of that in the reign of M.

Aurelius Antoninus, that, "if they were not identical with the Smallpox, they had at least considerable resemblances with that disease." Into this "vexata quæstio" we shall not enter at present, but shall content ourselves with once more recommending M. Littré's work to all our public libraries, and to those individuals who take an interest in ancient medical literature, as being the most important work of the kind in completeness, if not in extent, that has been undertaken for more than a century.

Art. XXI.—*Remarks on National Education.* By George Combe.—*Edinburgh,* 1847. 8vo, pp. 33.

Although the question to which these Remarks relate is one which it would be foreign to our purpose to discuss, yet we cannot pass Mr. Combe's pamphlet without notice; since it presents within a narrow compass a large mass of well-directed argument in favour of the position that the State ought not only to provide for, but to compel, the education of the people. He combats with great earnestness the prevalent idea that religion can only be taught dogmatically, and that an education into which it is not formally introduced is thereby "godless;" and points out very clearly that any instruction which tends to enforce those laws on which the Creator has framed our physical and moral constitution, is essentially and directly religious. "The moral and religious sentiments," he truly says, "are the grand levers of civilized society. He who commands them is irresistible; and until science shall discover her own character and vocation, that she is the messenger of God, speaking directly to these sentiments in strains calculated to thrill and rouse them to the most energetic action, she will never wield her proper influence over society for the promotion of their moral, religious, and physical welfare. Never, until she does so, will she take that place in social esteem and veneration, which, as the fountain of divine wisdom, she is entitled to possess."

Art. XXII.—*A Practical Manual, containing a Description of the General, Chemical. and Microscopical Characters of the Blood and Secretions of the Human Body, as well as of their Components, including both their Healthy and Diseased States, with the best methods of separating and estimating their Ingredients; also, a succinct Account of the various Concretions occasionally found in the Body, and forming Calculi.* By John W. Griffith, m.d., f.l.s., Senior Physician to the Finsbury Dispensary.—*London,* 1846. Part II. 12mo, pp. 168. With two Copper-plates.

We noticed the former part of Dr. Griffith's Manual soon after its publication, three years since, and then commended it as valuable so far as it went, though we felt called upon to express our surprise that so ample a title-page should be the prelude to so partial a survey of the subjects announced. In the little volume now before us, however, we have the completion of the entire range; and this in a very satisfactory manner. We can only regret, on Dr. Griffith's account, that he did not publish the

whole work at once; as it would then have possessed an equality of character which it now wants, the interval between the two parts having been one of much progress in regard to the subjects of the first, and would doubtless have commanded a much more extensive sale, the public having a very reasonable objection to a work issued in parts. We can strongly recommend it, in its now complete state, to the attention of our readers; not as superseding larger and more elaborate treatises, but as presenting, in a very compendious form, a great quantity of most valuable information. The following apposite remarks form part of the preface to the present part:

"The important light which has been thrown upon several points in physiology and pathology, by the researches of modern chemistry and microscopy, are so striking that to be alone acquainted with them is sufficient to ensure a due appreciation of their importance. To argue that such investigations are idle, merely because each new truth which is elicited is not immediately applicable to the elucidation of some point in the history of a disease, or to the improved application of remedial means for its alleviation, is as absurd as unfortunately it is frequent. There is, however, one consolation in this matter; which is, that those who are the most ready to urge these views, and to decry the utility of calling in the aid of the collateral sciences, are such as are least acquainted with their details." (p. v.)

Art. XXIII.—*Handbook of Human Anatomy, General, Special, and Topographical.* Translated from the original German of Dr. Alfred von Behr, and adapted to the use of the English Student, by John Birkett, Fellow of the Royal College of Surgeons of England, and Demonstrator of Anatomy at Guy's Hospital.—*London,* 1846. 8vo, pp. 457.

This work, according to the author's preface, is intended to serve either as an introduction to the study of anatomy, or for refreshing the memory, more especially of those preparing for examination. For the latter purpose it appears to be well enough suited, but scarcely for the former, and for this reason: the different departments of general, special, and topographical anatomy being all included in the short compass of the work, the descriptions are necessarily extremely brief, and therefore not very intelligible, except to one who has already gone over the subject. Although a brief, it is not an elementary book. The translator, however, is of a different opinion.

In regard to the translation, it is carefully made. Too much pains, however, appear to have been taken to keep by the author's text, whereas the translator would have better accomplished his purpose by adopting the plan and substance of the work merely, and employing his own expressions. By this means the translator would have corrected some inaccuracies of the original, and have avoided some unintelligibilities almost inseparable from an attempt at too close a translation. That the translator was well able for this is shown by the additions which he has himself made.

Art. XXIV.—*An Essay on the Treatment of Compound and Complicated Fractures, being an Annual Address before the Massachusetts Medical Society, in Boston, U. S. America, May* 28, 1845. By William J. Walker, m.d., Fellow of the Society—*Boston*, 1845. 8vo, pp. 100.

We have no personal knowledge of the author of this pamphlet, but should suppose, from internal evidence, that he is an easy old gentleman who at some trouble had delivered himself of an oration, and being mightily pleased with his own prowess as displayed thereby, had resolved to perpetuate the event in print. The work, however, although containing nothing striking or important, is creditable enough.

It consists of an introductory section, and sundry cases of extensive injury, culled mainly from the older authors. The object is stated to be the elucidation of certain principles in practice, which—so far as we understand them—have been, for many years, on this side of the water, regarded as fixed and determined. The young surgeon, however, may profit by their re-statement:

"These cases, as I think, prove the powers of Nature in healing compound fractures attended with great contusion, laceration, and loss of substance of the bones, and of the skin, and other soft parts, to be much greater than is generally supposed. They also show us that large portions of bone, when lost by accident, or removed by art, may be reproduced by the powers of Nature; and they illustrate, in a striking manner, the truth and importance of certain practical precepts established by the older writers on surgery, with respect to the treatment of wounds, complicated with fractures of the bones. The precepts to which I allude are:

"First, that all tendinous or membranous structures which obstruct the removal of foreign bodies, or unduly confine or strangulate the soft parts, when swollen by inflammation, should be promptly and freely divided.

"Second, that such dependent orifices should be preserved, or counter-openings made, as will, when aided by position or dressings, secure the free discharge of all fluids which might otherwise stagnate within the wound.

"Third, that portions of bone protruding through the integuments which cannot easily and without violence be reduced to their proper place, should at once be removed by the saw; and that all foreign bodies, loose portions, or shivers of bone, should be promptly extracted from the wound.

"Fourth, that great pain, inflammation, or nervous symptoms depend rather on peculiar complication, than on the exent of the wound. And that they indicate great danger unless rightly understood, appropriately treated, and relieved." (pp. 3-5.)

The only objection we have to his doctrines is, that in illustrating the last clause of his fourth head, the author rather runs into an extreme; advocating the incision of tendinous and membranous structures at once; not waiting to see whether or not, or to what extent, they may be implicated injuriously. In fact, he would seek to revive a rule of practice in regard to fractures, similar to the exploded treatment of punctured wounds; which wont to be dilated on the spot, simply because they came under the special category of "punctured." There is nothing worse in surgery than a wanton and uncalled-for use of the knife; nothing to be more studiously deprecated and shunned.

PART THIRD.

CONTRIBUTIONS TOWARDS THE ADVANCEMENT

OF THE

Natural History and Treatment of Diseases.

I. ON THE OBSERVATION OF NATURE IN THE STUDY AND TREATMENT OF DISEASE.

BY ANDREW COMBE, M.D.

(*In a Letter to John Forbes, M.D., F.R.S.*)

Edinburgh, December 11, 1846.

My Dear Sir.—Dr. Symonds's able letter in the last number of your Review reminds me forcibly of an occurrence which took place during my graduation here, upwards of twenty years ago. I had written a thesis, the chief purpose of which was to show that the dyspeptic symptoms in hypochondriasis are frequently *secondary*, both in importance and in the order of their appearance, and that while they may remain intractable under a treatment directed exclusively to the digestive organs, they often yield with facility after the removal of the *primary* cerebral and mental affection which originated them.

Such was my doctrine. Judge then of my surprise when gravely informed by the late venerable professor to whose perusal my thesis was intrusted, that it was a very good dissertation, but that I had certainly committed an important practical error in maintaining that *dyspepsia was never present in hypochondriasis!* On attempting to explain that I had broached no such opinion, the worthy professor cut me short by adding, with a bland and forgiving smile, that farther explanation was needless, as he felt sure I would profit by the hint he had thrown out. At that time such occurrences were new to me, and being taken by surprise I could only make my bow and retire to digest the remark at leisure and make the best of it.

Further experience, however, has since convinced me that in the scientific and literary worlds, it is by no means so rare a thing as I had imagined, to be criticised for the advocacy of opinions which were never entertained, and by critics whose views on all essential points are nearly the same as our own. Indeed, I believe that this very assumption of the existence of a contradiction where in reality there is little or none, constitutes one of the most serious impediments to the discovery and diffusion of new truths. Profiting by the lesson, it seems to me a wise precaution, before proceeding to refute a supposed antagonist by either argument or ridicule, to ascertain very clearly whether a difference exists, and if so, in what it really consists. If Dr. Symonds had acted on this principle, before writing his letter against excessive trust in Nature, I think he would have discovered that he was about to take much trouble to refute errors which were mostly imaginary; and that

when he condemned as wholly inert and absurd, plans of treatment conducted in accordance with Nature, he had formed an idea of them in his own mind, not only unwarranted by the published letters, but directly opposed to their spirit, and as such, repudiated not less by our principles than by his own. In fact, wherever Dr. Symonds observes and describes without any reference to theory, and wherever his practical views are fairly tried by the standard we recommend, there is found to be far less divergence between them than he seems to believe, or than any one would suppose from the mere perusal of his letter. It is true that he very rarely makes any special reference to the standard of Nature, but he has too much acuteness of observation and shrewd sagacity not to make frequent unconscious approaches to it in practice, and whenever he does so, there is generally harmony and almost never contradiction between us. If he were more familiar with the study of Nature as an aim, he would see that most of the opinions which he denounces are the unsubstantial offspring of his own misapprehension, and as such need not cause him any uneasiness, since they cannot continue their existence in his mind beyond the lifetime of their parent-error. But to come to proofs.

After alluding in general terms to the recent articles in your Review,—mine among the number—on the better observation of Nature in the study and treatment of disease, Dr. Symonds proceeds to say, "The question before us is this: *whether it is safer to leave diseases to their own course, or to interpose what we call remedies?* And really this," he continues, "*is a very frightful question* to be asking at the present state of the world's history, when medicine has been proposed as an art two thousand years and more." (p. 558.)

Concurring as I do most cordially in this latter sentiment, it seems to me strange that this very frightfulness should not have suggested a doubt to Dr. Symonds, whether he had not greatly *misapprehended*, and consequently unintentionally *misrepresented* the question that was really before him. If such a doubt had suggested itself, and he had thereby been led to inquire more carefully, all his fears would have vanished, for he would have seen that such was NOT the question which was before him, and that were it possible for a question so inherently absurd to be ever seriously put, there would be *all but unanimity* in the answer which it would meet with from the profession and from the public. It is true that in his letter, Dr. Symonds *seems* to express a contrary conviction; but so far from that conviction being well founded, I feel assured that it would puzzle him to produce one medical man who would, in his sound senses, affirm that it is "safer to leave diseases to their own course than to interpose remedies," and he has been led to an opposite conclusion only by adopting the mistaken impression, that treatment conducted in accordance with the laws and intentions of Nature (which, for the sake of brevity, I shall call the *natural treatment*) is *identical with no treatment at all*—a construction at once palpably erroneous and unwarranted. So common, however, is this misconstruction among those who have not thought on the subject, that it is everywhere the first objection made to natural treatment; and accordingly in your April Number I protested against it by anticipation (at p. 508), and even accused *you* of "an omission in your article which will tend to the diffusion of a misapprehension already too prevalent in a point of vital importance, and of having unguardedly used expressions which, *per se*, might be held to justify it." Again, at page 513, I recurred to the subject, and said, "By thus insisting on the necessity of a more complete and faithful observation of the course of Nature, and of acting more systematically under her guidance, *I am far from meaning that we are to sit with our hands across and allow things to take their own way.* So far from it, it is certain that the principle I inculcate would *demand more watchfulness, and give room for a nicer exercise of judgment, and a more consistent, and, I believe, successful treatment;*" and yet Dr. Symonds argues as if my object was to "leave things to take their own way"—a proceeding which he cannot condemn more strongly than I do.

These references may perhaps suffice to show that the question, as stated by Dr. Symonds, is neither the question really before the profession, nor that warranted by the spirit and substance of the article on which he comments; and it will not be difficult to show farther that, as already remarked, his practical views differ little in substance from those deducible from the principles to which he imagines himself to be so strongly opposed. To put this in a clear light, let us take two or three of the examples selected by himself as supposed contrasts to the practice he believes us to advocate: "If one were at a loss," he says, "for instances in which *art* interfered with advantage when *Nature* was doing nothing, or rather doing mischief, I doubt if any instance could be more striking than the cure and relief of many of the diseases of children by lancing the gums. On the expectant method a child may, after many days of suffering, and after incurring perils in various organs, at last recover when the tooth has pushed its way into the light, but all the pain and jeopardy might have been spared by one or two free incisions." (p. 561.)

Now the whole hostile force of this illustration depends on the question of fact, whether the contrast which Dr. Symonds supposes to exist between a treatment directed with a view to "*aid the efforts of Nature*" and what he calls "*art*" is real or imaginary? If he is right in assuming (*which he does throughout his whole letter*) that a treatment conducted in accordance with the laws of Nature consists in *doing absolutely nothing*, or, as he phrases it, in "leaving diseases to follow their own course," then the above example is at once pregnant, instructive, and conclusive against us. But if, on the other hand, it is, *as we contend*, an utter confusion of language and ideas to speak of two things so essentially different as if they were one and the same, the example ceases to have either meaning or value, except as showing how much a man of strong sense may be blinded by a preconceived notion. In this very case of teething it so happens that the correct application of the principles of natural treatment would lead to the use of the very same means which Dr. Symonds states from experience to be alone appropriate. In teething, *Nature's efforts* are directed to effecting a passage outwards for the advancing tooth. If, from any circumstance, she proves unable for the effort, sound principle would assuredly direct us to *aid her* in it, and remove the obstacles which obstruct her progress; and I am at a loss to know what more rational or direct *aid* we could lend her in her own way, than making the opening through the gum, which she has failed to accomplish for herself! In the outset of the letter which Dr. Symonds criticises, I state that it is the business of the physician to "take special care neither to counteract her (Nature's) efforts, nor to substitute another *method of cure* for hers, *unless when we have positive evidence that the vis medicatrix*, JUDICIOUSLY AIDED BY US, *will prove unavailing*." (p. 506.) To apply this principle to the case before us, it seems to me to indicate that when Nature is perfectly able to make a passage for the tooth, we ought not to counteract or disturb her efforts, but that *as soon as we have evidence that her attempts will prove unavailing, and that danger will arise, we ought to come to her assistance* and divide the gum for her. In what respects Dr. Symonds's practical views differ from the above, he himself would be puzzled to tell.

Perhaps, however, the best way of exhibiting the real difference between a natural system of treatment and "leaving diseases to follow their own course," which Dr. Symonds seems to consider as identical with it, will be to take the example of a deep sabre cut, the progress and results of which are visible, and consequently admit of no dispute. If the wound be left to "*follow its own course*," and any large vessels happen to be divided, the efforts of Nature to arrest the hemorrhage by the retraction of the artery and the formation of a coagulum will prove unavailing, and the patient will bleed to death. If, however, only small vessels be implicated, the efforts of Nature will prove suc-

cessful, and after a time the bleeding will cease. But from the wound being left to follow its own course, it will remain with its lips apart, and if it heal at all it will be only by the slow and insecure process of granulation. Such is the one method. If, on the other hand, we act on our principle of *assisting Nature*, the moment we find her efforts to arrest the hemorrhage unavailing, we shall come to her aid either by compressing or tying the artery. Having previously studied the order of Nature in the healing of wounds, we know that if their cut sides be brought into even contact, and the part kept quiet, union by the first intention will follow in a few days, *because the conditions required by Nature will then have been fulfilled.* Such accordingly is the plan of treatment really indicated by our principles, and its success justifies the unanimity with which it is acted upon. Why then should Dr. Symonds frighten himself about a phantom when his realities are the same as our own?

In like manner, take a fracture as an illustration. From observing Nature, we know that when a bone is broken increased vascular action must take place in the injured parts before reunion can be effected; that if it be *defective* in amount, the broken ends may remain in contact for months without uniting; and that if *excessive*, suppuration may ensue and also retard the union. Guided by this knowledge, the surgeon endeavours to second Nature's intentions by securing the limb in a proper position, keeping it motionless, and using any means which may be required to keep the reaction *at its proper pitch.* Having done so, he leaves Nature to complete her own work, and in due time has his reward. When the local action is too feeble, he, with the like intent of *aiding her*, rubs the ends of the bone rudely together or introduces a seton, and thus often succeeds in rousing Nature to successful exertion. If again the inflammation runs so high as to threaten suppuration, the surgeon, knowing that its occurrence would defeat her object, uses means to allay it, and often succeeds. Why then does Dr. Symonds speak as if all this were the same as leaving the fracture to "follow its own course," when it is precisely the treatment which he himself would pursue?

It may seem supererogatory to adduce seriously so much evidence in support of a proposition, which when thus placed in its true light is almost self-evident. But the improvement of medicine is so much retarded by the prevailing but most unfounded assumption, that practice directed by the indications of and in accordance with the laws of Nature, necessarily excludes all active means, and consists literally in *doing nothing*, and practical men often attach so little importance to the advantages derivable from an intelligent co-operation with Nature, that it becomes indispensable to direct attention to the subject by every means in our power. So impressed is Dr. Symonds, in common with Dr. Mayo and many other men, with this most erroneous conviction, that it pervades almost every paragraph of his letter. Hence we find him saying, at p. 559—"But to come to the cases in which it is especially needful to have our minds made up as to the right course of practice.—I mean the acute visceral inflammations—when I become satisfied that these diseases *may be left to pursue their own way*," (always the same phantom,) "my belief will have suffered a more complete *bouleversement* that it has ever yet undergone." To me it seems strange that Dr. Symonds should have ever penned such a sentence; and had he not been almost haunted by the error of supposing a natural treatment to be identical with doing nothing, the inherent absurdity of the proposition that acute inflammations were to be left to pursue their own way would certainly have led him to suspect that he had fallen into some unaccountable mistake in believing it to be advocated in the articles on which he was commenting. A natural system of treatment does not by any means exclude activity of practice, *where it is clearly called for to promote recovery or relieve suffering*. All that it provides against is the *abuse* of active interference, where *assisted Nature* would have effected the object with greater safety to the constitution. It is, perhaps, in acute visceral inflammations, more than in other diseases, that

heroic treatment has been most indiscriminately used, and although a great improvement in their treatment has taken place within the last twenty years, it still remains for consideration, whether the activity of our interference may not be reduced still farther with advantage to the patient. Even in pneumonia, in which, till lately, every body believed very active treatment indispensable, we now find not only that a fair proportion of cures takes place under homœopathic treatment, but that, according to Dr. Balfour's statement in your last Number, the allopathic Skoda even "*considers the great advantage of not bleeding to be* THE SPEEDY *recovery.*" (p. 591.) In like manner, Dr. Marshall Hall, whom nobody will accuse of inert practice, when speaking of the treatment of pleurisy in the Parisian hospitals, which he at first ridiculed as "trifling," honestly confesses that "*where the treatment of pleuritis consists greatly in the application of mere cataplasms, a post-mortem examination in this disease is scarcely to be obtained, so generally do the patients recover.*" (Hall's Observations in Medicine, p. 68.) I do not say that these statements are sufficient to condemn active interposition *in all cases* of pneumonia and pleurisy, but they certainly tend to show that the necessity for its being carried so far as is usually done, is by no means so clear and urgent as represented by Dr. Symonds, and that bloodletting, for example, might sometimes be beneficially restricted in frequency and extent, and used with more discrimination and a sounder appreciation of Nature's requirements, than it is at present by the mass of practitioners.

From some of Dr. Symonds's general remarks as well as from his cases, there is reason to infer that, practically at least, his opinions are not so different as he imagines from those which we really advocate. After stating how strange it would be if there was no such thing as an Art of Medicine, (which on the *do-nothing* principle there could not be) he explains with equal acuteness and truth, that "*Art* after all is but *Nature in a new form—a fresh arrangement of the forces of Nature compelling them to work under new conditions.*" Now this is precisely the doctrine which we have been all along advocating; and all that we insist upon is, that we should carefully observe Nature for the purpose of discovering which mode of rearranging her forces is the best calculated to insure a speedy and safe recovery.—Here, then, we once more practically agree with Dr. Symonds.

Forgetting his own definition of Art and still haunted by the deceiving phantom, Dr. Symonds next refers to the cures effected by hygienic treatment without the aid of drugs. He admits that such things do happen, but says, "Allowing this to be true, we must nevertheless perceive that they *are interpositions of art.* If a patient is confined to bed, when, but for orders, he would be sitting up or walking about; if he is restricted to bread and water or milk, when otherwise he would be living as usual; if a particular temperature of his room is maintained &c. &c. and he gets well—his case surely ought not to be adduced as an example of the *curative powers of Nature.* There has been a decided *interference of Art.*" (p. 562.) Most certainly there has been both a decided and a wise interference of art, but of an art which was clearly our old friend "Nature in a new form," with her old forces "working under new conditions;" and the strange thing is, that Dr. Symonds should not recognize her features under so slight a disguise. His whole difficulty would vanish if he would keep in mind his own definition of art, and open his eyes to the fact that *even by his own statement*, the said hygienic treatment does certainly *not* consist in "leaving diseases to follow their own way." It is quite true that such cures as he supposes are not examples of the curative powers of *unaided* Nature, but the great aim of the treatment we advocate is to *aid her efforts as long as they promise to be successful when so aided, and only to substitute other methods when hers prove unavailing.* If true art is really Nature in another form, it becomes as clear as the sun at noon-day that recovery in the

circumstances he supposes is as directly an example of the *curative powers of Nature* as the union of the fractured bone after art has made a fresh arrangement of her forces, by placing their ends in contact and preventing all disturbing motion or inflammation. Surely it is *not* art that actually solders together the broken fragments by extraneous cement, and just as little is it art which removes an internal inflammation which it can neither see nor touch. In both instances, Nature is in truth the curative agent; and all that art can do, or ought to attempt, is as far as possible to fulfil the conditions which observation shows to be most favorable for the success of her efforts.

Dr. Symonds refers to the operation for strangulated hernia and the tying of arteries as conclusive "instances of *Art versus Nature*." But both are palpably examples of art acting successfully on the previous indications of Nature. If an artery is divided, and its natural power of retraction proves insufficient to arrest the hemorrhage, and permit a coagulum to be formed as a preliminary to the obliteration of its canal, what does the ligature do if not the very thing which unaided Nature tried in vain to accomplish? In like manner in hernia, Nature continues to propel the contents of the bowel and carry on its circulation and vital action till, in consequence of a physical obstacle which she cannot overcome, her efforts prove fruitless. But, as stated in my former letter, when an impediment or obstruction comes in her way, it is the direct business of the practitioner to remove it; and hence, so far from objecting to an operation *when milder means cannot be found*, he would, on the principles of the natural treatment, at once agree with Dr. Symonds in resorting to it. No doubt the surgeon's knife is the instrument by which the impediment to the return of the bowel is removed, but surely it has been used to fulfil the indications of Nature, and it is Nature under the new arrangement of her forces which completes the work. If the objectors to the natural system of treatment will only dismiss from their minds the false notion that it means "*doing nothing*," they will at once perceive that there is no incompatibility whatever between it and the employment of even the most active interpositions of art, when necessary to secure in any individual case that arrangement of Nature's forces which is most likely to lead to recovery. All that is required, is that such active means should be resorted to only *when, where*, and *in the manner, indicated by the close observation of Nature's operations*, and not, as so often happens, from mere habit, tradition, caprice, or—worst of all—in the form of medicines given as the means of securing an adequate remuneration for attendance.

Another reason which shows that Dr. Symonds is practically a greater believer than he imagines in the principles of the *natural treatment* of disease, will be found towards the end of his letter, where he says, "I am not fond of arguments from final causes; but can it be doubted that the various medicines we possess were, as such, *a part of the plan of the universe designed to have a relation to morbid states of living organisms* as much as esculent matters to healthy conditions?" To this query every rational mind will concur in answering, with Dr. Symonds himself, that no doubt whatever can be entertained on the subject. But just as little can it be doubted that, in accordance with the plan of Nature, such medicines should be used only in "those morbid states" to *which they have the designed relation*, instead of being given, as they *often* are, with no definite aim, and for no intelligible reason, except compliance with custom. If we take quinine as a fair specimen of this class of medicines, and as designed by Nature to "have a certain relation to the morbid state" of ague, is it not self-evident that its exhibition would be included under the head of a natural system of treatment of that disease, and that having due regard to the observation of Nature, we should further be prompted to discover and remove the local causes by which the ague had been produced, and by which recovery was still impeded? It is known, for instance, that removal to a healthy locality

will often suffice to put an end to the fits; and it may fairly be asked, would not removing the *cause from the patient* act in the same way as removing *the patient from it?* On the more common plan, however, of trusting too exclusively to medicines alone for the cure of disease, the continued operation of the cause, and the advantages which may be derived from a new and more salubrious arrangement of the forces of Nature in facilitating recovery and preventing the occurrence of disease, are apt to be too much overlooked.

"But," says Dr. Symonds, "what shall we say of fevers and the exanthemata? Only what nine-tenths of eclectics would affirm, viz., that the less heroically they are treated the better." Here then is still another proof that Dr. Symonds is not so wide apart from us as he imagined. *Fever and exanthemata are precisely the class of diseases the natural history of which is best known,* and on the right indications in which a sound physiology throws most light; and there is every reason to believe that in proportion as the natural history of other diseases becomes better understood, their treatment also will become less heroic and more accordant with the laws of Nature than at present, and, as a consequence, *more successful.*

I scarcely need say that in going into this subject at so much greater length than I intended, I am actuated by no feeling of disrespect towards Dr. Symonds, whose zeal and services and kindly intentions towards myself I fully appreciate. But I have given a prominence to his remarks because his character and position give them weight, and because, in this instance, he represents a large and influential class in the profession, whose objections to our views, although apparently formidable, are, from the very same cause, equally unreal as his own. In the 'Medical Gazette' Dr. Mayo, among others, took much trouble to refute errors, most of which consisted of misapprehensions of his own; and since the appearance of the articles in your Review, I have heard of various conversational *demolitions* of the supposed system of doing-nothing, by friends who, on farther inquiry as to the reality of the alleged opinions, found to their own surprise that they had expended their blows on the windmill when they believed they had been fighting a giant. Having adopted their own practical views merely as the results of experience or of previous teaching, they had failed to discover sooner that in their general bearing they were to a considerable extent coincident with those of a natural system of treatment; and hence, from pure misapprehension, they had attributed to me fancies which, in common with themselves, I wholly repudiated as too frightful to be believed.

If the subject under discussion had been one of a purely personal nature, I should not have occupied a single page of your Review with the correction of any of the mistakes which have been committed on either side. But where the application of an important scientific principle is concerned, and its soundness or unsoundness is calculated powerfully to affect the progress of an art bearing directly on human life and human happiness, personal considerations sink into insignificance, and it becomes a duty to point out every source of error, or of even apparent contradiction, which may come under our notice. It is with this purpose alone I have ventured to trouble you with these remarks, and I feel assured that Dr. Symonds will regard them as applied, not to himself, but to the subject. In justification of the misapprehensions into which he has fallen, I readily admit that in my former letter there are isolated passages which warrant the construction upon which he has commented, and that it would have been better had these been more carefully and correctly expressed. I admit, farther, that I omitted all detailed or special recommendation of active medical treatment, and might thus erroneously be supposed to disparage it, even in cases where it is most required. But as my object in writing was to urge attention to *the neglected portion* of treatment, and *not to that which is already too actively followed,* viz., the use or abuse of drugs, I naturally dwelt on the former, and did not feel called upon to enter upon the consideration of the

latter. Fearing, indeed, some such misconstruction, I warned you, towards the end of my former letter, that "I must trust to your good sense and right feeling not to give undue importance to any isolated or dubious expression which you may meet with, but *to adopt that meaning which is in harwony with the general spirit of my remarks.*" Tried by this test, I still think I shall be found not to have undervalued active treatment where it is really called for by the state of the patient, and where the indications presented are judiciously fulfilled. In this respect your views, I believe, entirely coincide with my own.

And now for a parting word on homœopathy. *Commenting on your statements* of the success attendant on the homœopathic treatment of even severe diseases,as a sufficient and reasonable ground for instituting an inquiry into the alleged facts, I recommended as the safest and easiest preliminary step, that experiments should be made on the alleged power of certain medicines to produce *in healthy persons* forms of disease analogous to those in which they are given to cure; and as an example, I said, "Let experiments be made on a *sufficient number of* HEALTHY PERSONS with quinine or any other drug, to ascertain whether it really has the power ascribed to it, of exciting certain groups of symptoms IN A SOUND CONSTITUTION, and after carefully varying and repeating the experiments, faithfully record and publish the results;" by which means the principle of *similia similibus* may be proved or disproved beyond the possibility of a doubt. Not adverting to the real meaning of this proposition, Dr. Symonds remarks that "Dr. Andrew Combe tells us that" (in testing homœopathy) "we must carefully distinguish the principle of *similia similibus* from that of *infinitesimal doses,* and he suggests that we should test the former separately. But to do this we must begin an entirely new line of observations. And *what is to induce us to administer a scruple of jalap for a looseness, and a grain or two of opium for a lethargy?*" Dr. Symonds follows up this question by sundry exclamations, which need not be quoted, on the absurdity of such a course; but if he had correctly apprehended my proposition, he would have seen that it was nearly the reverse of that which he ascribes to me. As really stated by me, the question to be tried is, whether the medicine which *cures* (not causes) a looseness or lethargy, *will produce* the one or the other respectively *when administered to a person in health and of sound constitution.* Now, as he does not affirm that jalap *cures* a diarrhea, nor opium a lethargy, it is for himself and not for me to explain *why* we should be induced to prescribe the one for a looseness or the other for a lethargy. I candidly own that I cannot give him the information he asks, nor would the inquiry proposed by me be likely to lead to it.

I trust that I have now said enough to satisfy Dr. Symonds that the question really before the profession is by no means so frightful as he represents it to be, and that, contrary to his assumption, there is something approaching to unanimity among medical men, as to the folly and wickedness of "leaving diseases to follow their own course," when Nature has put so many means within our reach, by which they may be guided more safely and surely towards a successful issue. But while I think that Dr. Symonds has been rash in ascribing to some of the communications in your Review a meaning not warranted by either their substance or their spirit, I scarcely regret the publication of his letter, or of the analogous articles in the 'Medical Gazette' and other journals, as the discussion they have elicited will lead to good, by stirring up reflecting readers to the *better observation of Nature in both the study aud the treatment of disease*—a path which still seems to me to lead more directly to the land of promised improvement in our art than any other yet discovered. But not to trespass further on your patience, I remain,

My dear Sir,

Very sincerely yours,

Andrew Combe.

II. NOTES OF SOME EXPERIMENTS, ILLUSTRATING THE INFLUENCE OF THE VIS MEDICATRIX, AND OF THE IMAGINATION, IN THE CURE OF DISEASES.

BY A NAVAL SURGEON.*

(*In a Letter to John Forbes, M.D., F.R.S.*)

[In our anxious desire to rouse the attention of the profession to a philosophical investigation of the real powers and actions of medicines, we willingly give a place in our pages to the present communication. Its bearing at once on the nonentities of homœopathy and the too strong realities of heroic medication, and also on the nature of the logic too current in the profession—is sufficiently obvious. We should think it would be equally obvious to every one who had taken the pains to read all our preceding papers on this subject, that, in giving publicity to such a document as this, we do *not* in any way advocate the propriety of leaving diseases to Nature, or of withholding any of the rational aids of ordinary medicine; much less, that we hold out to our readers our correspondent's plan of working with imaginary remedies, as one to be followed in ordinary practice. We here merely record the repetition, in an authentic form, of an old experiment, which we deem of some importance at the present time, and which the author meant simply as an experiment. Perhaps we should have taken this opportunity of offering, in our own name, some comments on certain misapprehensions of our views respecting the treatment of diseases recently advocated in this Journal, had not this been so admirably done for us by Dr. Combe, in the communication immediately preceding this. To this communication we request the best attention of our readers.

We must also say one word in reference to the cases of fever noticed in the present paper. Of course the number of instances given is infinitely too small to authorize any *general* inferences respecting treatment. The cases are, however, valuable *as instances*, as far as they go: *valeant quantum*.]

H.M.S. ——— Piræus of Athens, October 2d, 1846.

My dear Sir,—The correspondence, published in the July Number of your Journal, descanting on and highly approving of your invaluable article entitled Homœopathy, Allopathy, &c., which appeared in your January Number, has only just now met my eye. This correspondence has recalled to my mind the intention I had previously entertained, of offering to you my humble tribute of thanks, for having given to the world a paper the effect of which is likely to be attended with the most beneficial results, by promulgating doctrines and opinions of the highest importance to medicine; the truth of which has (at least partially) been long felt, although few have had the inclination, and none the courage, publicly to avow it. I now proceed to accomplish what I only before had purposed.

For a long period I have been getting more and more sceptical as to the curative effects of certain medicines in many diseases; while the use of the lancet, and other violent depressing means, have been almost totally omitted in my practice for nearly ten years. While on the coast of Africa, and in medical charge of the Island of Ascension, about ten years since, I fancied I saw great reason to alter the violent depleting mode of treatment then in vogue in those countries.

* The writer of this communication is an officer of long standing and much experience. His name and high character are known to the Editor.

In 1838, a bad attack of marsh fever broke out in the ship I was serving in, in the Mediterranean. My assistant was one of the sufferers. I was, consequently, left entirely to act on my own responsibility. Being in the greatest doubt as to what line of treatment was likely to be attended with the greatest amount of temporary alleviation of the distressing symptoms, as well as what was to tend most to the security of my patients against any permanent bad effects from the epidemic, I determined on running some risk with the first few, in the hope of being the better able to relieve the sufferings of those who came after. Acting upon this determination, I took the first twelve cases, officers and men, as they presented themselves. The fever in all these having assumed the ardent form, blood was abstracted from four, in quantities which, at the time, were considered sparing, as all were bled in the erect position, and from a free opening, and in two of these four the bleeding was repeated during the evening of the first day; laxatives were given, and the system attempted to be kept under the influence of antimony. Two were treated according to the mode then just made public, by M. Malliot—viz. by large doses of quinine from the very commencement. Two, after having had an emetic and purgative dose, were put under active mercurial treatment. Two had each a dose composed of the following ingredients: T. digitalis, T. opii, Vin. antimonialis, Vin. ipecacuanhæ, of each ʒiss. This dose was repeated after six hours; and after the profuse perspiration brought out by these draughts had in part subsided, the patients were simply watched, to see that they had effervescing or other palatable drinks in what quantity they chose. Two were put to bed, their skins freely sponged with vinegar, and watery drink given ad libitum, and so left, without even a dose of laxative medicine. Now for the result. Of the first four (the parties who were bled), two died, and one, subsequently, required to be invalided and sent home. Of the two who had ℈j doses of quinine, one died, and one subsequently required to be invalided. Of the two who were treated by mercury one died, and one had a most protracted convalescence, and died some two years after, without having ever fully regained his strength. The whole of the others were, after longer or shorter periods of convalescence, enabled to return to their duties, having, to all appearance, regained their original vigour. The fever continued in the ship until we were enabled to change our locality—viz. from the Gulf of Scanderoon (situated between Asia Minor and Syria) to the mouth of the Dardanelles. But as the whole of the remaining cases were treated in compliance with some special indication, I am inclined to consider the results as of less value. Need I add that since that period the patient labouring under fever, who has been solely under my charge, has never been bled, or that but very little medicine, in the treatment of either Mediterranean or West Indian fever, has been expended in my practice? And yet my returns, so far as I am aware, show no more fatal results than those of others similarly situated, but whose mode of treatment is much more active. So much for letting Nature have her way in those diseases which appear to arise from the absorption of specific poisons, as in the case of marsh and other fevers.

I am almost as sceptical as to the necessity of large bleedings, in inflammation of the contents of the larger cavities, as I am in fever. Convinced, in my own mind, that I had seen diseases of the chest more especially, not only kept up, but hurried on to a fatal termination by the large and repeated bleedings practised for their removal, I long ago began to be chary of following such practice. And here again I may mention that for years I have only once used a lancet, in the treatment of such inflammatory cases as have come under my care; and in that one (a case of inflammation of the bowels) I yielded with reluctance to the solicitations of my assistant, not from any impression of its absolute necessity. The man got well; but so do other men who

sustain no such loss. It must not, however, be imagined that I mean to convey the belief that I am prepared to doubt the efficacy of all curative means, or that I should be able to stand by and see active inflammation take its course, without making an effort to stop its progress. Very far from this. I only hold that in many cases where, in ordinary practice, the lancet is again and again had recourse to, the previous use of digitalis and antimony, prussic acid, opium, &c., will lead to results of a more satisfactory nature, than when the depleting system is practised to a large extent.

Since the year 1837, I have been in the habit of occasionally treating some few of my patients, whom I could neither dose nor reason into good health, by a different method still. And this method will, perhaps, be best illustrated by one or two examples.

Case I. A very intelligent officer had suffered for some years from violent attacks of cramp in the stomach. He had tried almost all the remedies usually recommended for the relief of this distressing affection; and for a short period prior to coming under my care, the trisnitrate of bismuth had been attended with the best results. The attacks came on about once in three weeks, or from that to a month, unless when any unusual exposure brought them on more frequently. As bismuth had been so useful, it, of course, was continued; but notwithstanding that it was increased to the largest dose that its poisonous qualities would justify, it soon lost its effect. Sedatives were again applied to; but the relief afforded by these was only partial, while their effect on the general system was evidently very prejudicial. On one occasion, while greatly suffering from the effect of some preparation of opium, given for the relief of these spasms, he was told that on his next attack he would be put under a medicine which was generally believed to be most effective, but which was rarely used, in consequence of its dangerous qualities; but that, notwithstanding these, it should be tried, provided he gave his consent. This he did willingly. Accordingly, on the first attack after this, a powder, containing four grains of *ground biscuit*, was administered every seven minutes, while the greatest anxiety was expressed (within the hearing of the party), lest too much should be given. The fourth dose caused an entire cessation of pain. Half-drachm doses of bismuth had never procured the same relief in less than three hours. For four successive times did the same kind of attack recur, and four times was it met by the same remedy, and with like success! After this my patient was ordered to join another ship, on a different station.

Case II. A seaman had suffered from four successive attacks of constipation. So far as could be detected, there was no organic disease to account for its occurrence. The symptoms were such as usually follow protracted constipation of the bowels; and on all four occasions large and repeated doses of the strongest purgatives (croton oil included), powerful enemata, cold affusion, and hot baths, had all been required to be persevered in to procure relief. On the fifth attack he was put under grs. ij of bread-pill every seven minutes; much anxiety being, of course, expressed to guard against any over-dose, as well as to watch the effect of what was thus given. Within two hours he became sick (*one of the symptoms expected from the medicine*); and his bowels were freely open almost immediately after; nor did they again become constipated, so far as I am aware.

Case III. In July, 1845, the company of H. M. S. ——— were attacked with an epidemic bowel complaint, terminating in simple diarrhea in some, but going on to dysentery in many. In every one of the latter cases tapeworms (whether a cause, or merely an effect, I am unable as yet to divine) showed themselves. Amongst others who suffered was H. B., a first-class petty officer, who had but a mild attack of dysentery, but who was much distressed towards the latter part of his attack by tapeworm appearing in considerable quantities. As the dysenteric symptoms disappeared, these worms

were attempted to be dislodged by every means that could be devised, and for a time it was supposed these means had been successful; but, as I feared, at too great a sacrifice, seeing that the pain arising (as I fancied) from the large doses of powerful medicine necessary to effect this difficult object, continued around the pyloric orifice of the stomach and upper portion of the small intestines, to be most distressing. Counter-irritants were applied until the skin became callous, sedatives administered until the man's senses became muddled, but no course of treatment seemed to afford the least relief This being so, I determined to try the effect of mental influence. Stating to him, as I did to the other men, that as his disease was most obstinate, so was it necessary to have recourse to desperate means to relieve it; that, with his sanction, I would therefore put him under a medicine which it was most necessary to watch with the greatest attention, lest its effects should prove most prejudicial, perhaps fatal, &c. Having by these statements made an impression, it became necessary to keep it up. This was done by repeated visits at all hours of the day and night, and by expressing on these occasions the most intense anxiety as to the effect of the very powerful and dangerous medicament. This was not a case in which a sudden effect could be expected to be produced, whatever might be the means employed. Symptoms of disease existed which bore too close a resemblance to those of an organic order, to admit of hope of a sudden, if even of tardy relief. Hence the pills (bread, of course) were given every sixth hour only. Within twenty-four hours the man's sufferings were decidedly less. Within four days he was almost free from pain. On the sixth day he was quite so; his pills were omitted, and at the end of a fortnight he was again at duty, with a clear eye, a healthy skin, and was rapidly regaining his flesh. Here, as in most cases where this method has been tried, the diet and drink have been left unrestricted. Occasionally, however, it became necessary to taboo some article, lest its coming in contact with the remedy might prove most destructive; in other words, articles are occasionally forbidden when the mind seems to be inclined to lose sight of what must be made the all-important subject of thought by night and day. The wonderful improvement in this man's state was frequently commented on by both officers and men, who, of course, were, and still are, as little acquainted with the means employed as the patient himself was.

It may be said that this case, as here given, goes for nothing, in so far as it does not show that the pains were anything but casual; in which case, any other mode of treatment, or very likely no mode at all, would have been equally successful; or it may be again, as it has before been said, that it was altogether feigned, and that the commanding officer would have made a better and quicker cure. I think not; and for the following reasons: The man's flesh had wasted, his eye became sunken, his skin sickly in hue, as well as in feeling, his sleep, when he had any, was of the most disturbed order. But, more than all, the pain after some weeks returned, and the other bad symptoms followed in its wake; yet both it and they were again relieved a second time by the same means. While suffering from a third attack, he was sent to the Royal Naval Hospital at Malta, and where, after much suffering, he brought up by vomiting a portion of the mucous membrane of one of the small intestines, distinctly marked by two, at least, of the valvulæ conniventes. I am assured by one of the officers of that establishment, that he most carefully examined this ejected matter, and that its characters were so marked that there could be no room for a doubt as to what it was. This being so, we have pretty clear proof that disease existed long before this slough was thrown off; and that even this organic disease was suspended on two occasions by mental influence only.

It is clear that there are great and evident objections to this mode of treatment becoming generally useful. The fact of its being based and built up on deception renders it an unworthy means for an intelligent practitioner to have

recourse to, even when his sole object is the relief of suffering. Nor is the fact that publicity would render it at once inutile, less certain of preventing it being followed. But though neither fit to be generally practised by others, or long followed by me, the truth which the above cases, and others like them, inculcate, ought to be made known; the more especially so at a time when the homœopathist is expending his powers in showing to the world how beautifully his infinitesimal doses act and react upon disease; as it may tend to convince some that if in many cases they would leave their doses, whether large or small, altogether alone, they might, perhaps, be equally near their purpose; or I ought rather to say, that their patients might, perhaps, be equally near a sound state of health.

I am, my dear Sir,
Yours faithfully,
* * * *

III. ON THE TREATMENT OF FEVER BY THE APPLICATION OF COLD WATER TO THE SURFACE OF THE BODY.

BY J. H. STALLARD, ESQ.,
Surgeon to the Leicester General Dispensary.

(*In a Letter to John Forbes, M.D., F.R.S.*)

[WE have great pleasure in laying the following communication before our readers, as the first fruits of our attempt to rouse anew the attention of the legitimate members of the profession to the great value of cold water as a therapeutic agent, and thus, if possible, to stimulate them to rescue its use from the hands of ignorant non-medical pretenders and charlatans. Mr. Stallard, like a man of sense, did not hesitate to apply what he thought would benefit his patients, because the mode of treatment might have first been used by one ignorant of all science, or since practised by some devoid of all honesty. He adopted the good as he found it; but, like a scientific practitioner, applied the remedy according to the rules of science,—at the proper time, in the proper cases, and in the proper manner. It is thus we would have the members of the profession to act generally, and so bring under the domain of scientific medicine, and within the pale of professional practice, what must now be considered as a weapon of power in the hands of our enemies;—as we may truly call those who profess to use the hydropathic or any other method of healing diseases, while ignorant of the knowledge on which alone rational practice can be based. To say that cold water employed in the mode lately introduced by Priessnitz is not a powerful therapeutic agent, is simply to express ignorance of a common and well-known fact; and to admit its potency as an agent, is equivalent to saying that, improperly administered, it is capable of doing mischief. In fact, we know that it is capable of doing great mischief, and believe that it has done great mischief; but it is also capable of doing great good; and we have called upon the members of the medical profession to destroy the evil, and cherish and promote the good, by taking the practice—or as much of it as is warranted by fair evidence—into their own hands, and modifying it in accordance with the laws of science, and in obedience to rational experience.

We are far from asking our readers to adopt hydropathy in an exclusive form, much less to become professed hydropathists; on the contrary, we ask them to assist us in destroying hydropathy as it is now practised, and in routing the whole phalanx of trading water-doctors, by cutting from under them the ground

on which rests their strongest hold on public patronage—viz. the pertinacious and scornful rejection by the profession of certain modes of treatment employed by them, which the public have found to be useful, and choose to patronize accordingly.

In making the attempt to awaken attention to this subject, at the time and in the manner we made it, we were well aware that we should excite opposition not merely from the ignorant and self-interested, but even from some who were qualified to judge of the question, both as a matter of science and as a matter of professional propriety, but who had not had any experience of the facts, or well considered the subject in a scientific point of view. But here, as in the previous discussions in the Journal, on Homœopathy, Mesmerism, and Phrenology, it required no effort on our part to meet any opposition that might be excited, or to tolerate with perfect calmness any criticisms that might be addressed to us: the importance of the object to be attained—the singleness of our aim—the consciousness of proper motives—were sufficient to render us perfectly satisfied with the result, whatever it might be, and to keep us steadily and quietly advancing in the humble path which we had deliberately chosen. Our first object in what we have done, and are doing, is the attainment of truth; our next is the maintenance and promotion—if it may be—of the honour, and dignity, and true interests of the medical profession. In the attainment of these objects, or in the endeavours to attain them, we trust we shall never so far forget ourselves as to prefer, at any time, the wrong because it is fashionable or safe, or to abandon the right because its advocacy may expose us to some inconvenience, to the imputation of unworthy motives, or even to the charge of a dereliction of professional duty. He is a poor politician and a poorer philanthropist who thinks only of the present, and cares only for the pleasing. We would fain hope that we of the British and Foreign are made of sterner stuff.]

Leicester, Nov. 18, 1846.

My dear Sir,—Although personally unknown to you, I have ventured to send you the inclosed paper, containing notes of some cases of fever treated by me in the Leicester Temporary Fever House by the application of cold water. During the autumn, fever has raged here to a degree far greater than usual, and the board of guardians have established a temporary fever house, in which I acted as resident surgeon for the space of one month. It was during this residence that the last number of your Journal, containing your article on hydropathy, and remarks on the treatment of fever pursued by Dr. Currie, came under my notice. This so engaged my attention that I requested my father, one of the union medical officers, to place his share of cases under my charge, in order that I might give cold water a fair trial. The cases I have reported are not *selected* cases; they are those which presented themselves as being most proper for the trial; and are all which came under my *entire* observation in the fever institution. I may, however, remark that the same plan has since been carried out by my father in all recent cases with the most decided success, and I myself have frequently employed it with like benefit in private practice. The mode of proceeding was as follows:

The patient was stripped naked, enveloped in a *cold* wet sheet, and covered with a blanket. After remaining in this situation from ten to fifteen minutes he was, without being dried, immediately wrapped in a blanket thoroughly heated before the fire, and thus removed to another bed and covered over with bedclothes. The effect produced by the wet sheet is, first, a sensation of great cold, accompanied with sighing; but this is almost immediately succeeded by an agreeable sensation of coolness and comfort; the sheet then begins to grow warm, and when the heat of the skin had been previously very great, the

blanket reeks with steam. Shortly after the patient is removed to the warm bed he begins to perspire, his headache and muscular pains cease, and he sinks into a calm and undisturbed sleep, from which he awakes still perspiring, but painless, refreshed, and occasionally well.

With respect to the character of the fever which has so extensively prevailed in this neighbourhood, I believe it to be the common typhoid fever of this country. Its type has varied considerably; for the most part its tendency has been enteric; but during a few weeks, and that especially during the observations I have detailed, there was a marked tendency to pneumonic implication, chiefly of an asthenic character. With a few exceptions, I have not observed much cerebral disease. The following extracts will show the general features of the disorder:

LEICESTER UNION FEVER INSTITUTION.

Number of patients admitted from Sept. 12th to Nov. 10th	187
Remaining at time of the Report	30
Discharged	157
Of whom died	16
Cured	141

Average deaths, 10·1 per cent.

Time the patients remained in the institution:

Longest	50 days
Shortest (case of Smith)	2 ,,
Average	12 ,,

Time of residence of the fatal cases:

Longest	50 days
Shortest	1 day
Average	12 days

LEICESTER COUNTY FEVER INSTITUTION.

Patients admitted from Aug. 1st, to Oct. 31st	99
Remaining at the time of report	14
Discharged	85
Cured	70
Died	15

Average deaths, 15·2 per cent.

Time the patients were resident in the institution:

Longest	83 days
Shortest	4 ,,
Mean	25 ,,

Time in the institution of the fatal cases:

Longest	39 days
Shortest	4 ,,
Average	16 ,,

Duration of illness in fatal cases prior to admission.

Longest	28 days
Shortest	1 ,,
Mean	11 ,,

It will be seen by these tables that the deaths in both institutions resulted principally from the admission of hopeless cases. The form of complaint experienced in the County Fever House was more severe, and longer in its duration, than in the sister institution—no doubt arising from the neglect which country people more frequently suffer before admission; whereas the Union Fever House being placed under the care of their own medical officers, the latter were enabled to remove every case as it appeared; thus very materially diminishing the average residence of the patients, and also the per centage of deaths. On the whole, the mortality of the town has considerably exceeded

the usual average. The deaths, as far as my own examinations tell, have resulted either from ulcerations of the small intestines, or congestive or typhoid pneumonia.

REMARKS ON THE CASES.

In the first case, the patient had a slight cough before the cold application, and it appeared as though the cold had rather increased the internal congestion; but the general state of the boy clearly showed that the constitution was entirely relieved by the profuse perspiration. The cough did not return with the second application of cold.

Case II was one in which I hesitated to use the cold, fearing lest the internal organs, which were already seriously affected, should suffer by it; but the disease seemed so really serious, the skin was so remarkably harsh and dry, no perspiration having ever taken place since his admission, that I determined to try it. The effect was most gratifying; the perspiration seemed at once to unload the system, to restore the functions of the lungs and alimentary canal to something like order, and the stimulants ordered at the same time aided, I have no doubt, in bringing about this desirable and, to me, unexpected result.

The third case was equally successful. Although the patient had been under the ordinary treatment five days without real benefit, he was at once relieved by the cold sheet. The application in this case, as in most others, caused *immediate costiveness.* This is remarkable; for the general type of the epidemic has been characterized by the frequent occurrence of diarrhea.

Of the following cases I shall only notice the eighth and tenth; the former being by far the most striking cure resulting from the cold-water treatment—a single application, without any medicine, having restored the healthy condition of the body.

The last case, although terminating fatally, does not furnish a single argument against the use of the cold sheet. The death was, no doubt, caused by sudden congestion of the lungs, which fever patients are especially prone to, and the tendency to which was probably augmented by the dose of morphine administered over night to allay the excessive restlessness. The application of the cold itself was followed by the best result, viz. free perspiration; which I am afraid was checked by the restlessness of the patient causing the bedclothes to be thrown off and the body to be exposed. I have related it with the rest; and, although I should, perhaps, hesitate again to employ the sheet in the very advanced stage of fever, especially where there is great irritability of the nervous system, I think its use must be highly advantageous in debilitated persons when first seized, as it enables the practitioner to employ beef-tea, wine, and other stimulants, whilst, at the same time, means are being used to restore the action of the skin and the healthy condition of the blood.

I shall not tire your patience with remarks on the pathology of fever, or the theory of its *cure;* but, before concluding, I shall offer one or two observations which have occurred to me during the epidemic of the present autumn. At its commencement I very frequently employed an emetic at the first onset of the febrile attack, and with very varied success. In many cases, free vomiting was accompanied by profuse perspiration, in a few by purging also; and these cases, without exception, got well immediately; but in other instances, and those not a few, no perspiration ensued, and in these as constantly did the tongue become dry and foul, the thirst and fever increase. I think that this observation, together with the evidence afforded by the cases I have recorded, clearly shows that, in this epidemic at least, a fever may be cured, and that not by a rash, uncertain, and empirical mode, but by a remedy which coincides with and favours the efforts of Nature. This I think the cold sheet, as applied by me, does, first, by relieving the system of caloric which it is endeavouring to get rid of by over-action of the skin and lungs; secondly, by reducing the temperature of the surface, and so enabling the skin to resume

its healthy excretory action; and, thirdly (through the natural stimulus given to that healthy action), by depurating the blood of those effete matters which the checked excretion of the skin had locked up in it.

Should you think the inclosed cases likely to be of any public use, I shall feel a pleasure in having recorded them; remembering, as I do, how deeply I am indebted to you for the instruction and benefit I have always received from the perusal of your valuable Journal.

I remain, dear sir, yours, most truly,

J. H. Stallard.

Notes of Cases of Fever treated in the Leicester Temporary Fever Institution, October 1846.

Case I. Coleman, Henry, æt. 10, admitted October 5th, from a close and most unhealthy yard. He complained of rigors; skin very hot and dry; bowels quite open; slight cough. He was ordered simple febrifuge mixture, with low, simple diet.

October 6th. The skin very hot and dry; the tongue red and very foul; bowels open; he complains of slight cough and headache. There is no apparent congestion of the lungs.

He was ordered to be surrounded with a cold wet sheet, to be placed in a blanket, and to remain in it for ten minutes.

Vespere. About half an hour after the cold application he broke out in a profuse perspiration, which continued during the whole day. He appears much better this evening, is more lively, and his head is relieved; still slight cough. Contin. med.

7th. Tongue cleaner, moist; bowels open; complains of headache. His nose has bled during the night. Cough as before. Slight crepitation observed at the base of the left lung, but he has perspired freely through the whole night.

To have cold water applied to the head, and to continue the fever mixture.

8th. Much better; crepitation gone; no recurrence of fever. Ordered to get up.

9th. There was a slight recurrence of fever last night, and he was again placed in the wet sheet; perspiration was induced, and it continued during the whole night. His tongue clean, and bowels regular; cough gone. To have rice pudding, and omit medicine.

10th. Continues improving. To have beef-tea. 11th. Discharged cured.

Case II. Tomlinson, Spring, æt. 26, admitted October 2d, consequently had been ill a week when I first saw him on Oct. 9th. He had been taking simple fever mixture, and had had diarrhœa and pneumonia. His tongue was dry and foul; bowels still purged; cough; mucous râles were heard over both sides of the chest, at the back part; pulse very feeble, upwards of 100; skin had never been moist since his admission, and is now remarkably harsh and dry. Ordered the wet sheet to be applied as above, and to have $1\frac{1}{2}$ oz. of port wine every four hours, with a little beef-tea. No medicine.

10th. Much relieved. Shortly after the removal of the wet sheet he broke out into a most profuse perspiration, which has not yet entirely gone off. Tongue is now clean and moist; his bowels have been open twice. He had no beef-tea yesterday, from an error of the nurse, but it was again ordered.

11th. Skin is quite wet; his cough is gone; his tongue is much improved; and his bowels have been costive since yesterday's visit. To have ol. ricini. ℥ss.

12th. Better. Bowels opened by the oil; he coughs occasionally; but a thick whitish phlegm is easily expectorated. To have meat diet [too soon?]

13th. Tongue dry; bowels open twice; no thirst; his skin is quite moist; and otherwise he is much better. To omit meat, and recur to beef-tea.

14th. Tongue is to-day clean and moist.

He continued to improve, and was discharged on the 16th.

CASE III. Thomas Harris, æt. 14, admitted October 3d. He had been ill seven days previous to his admission, but had so far improved that he was ordered to get up on the 7th. On the 8th I found him with a foul tongue, very red at the tip and margin; bowels open once in twelve hours; great thirst; aching of limbs, back, and head; a frequent pulse; and his skin very hot and dry. He was ordered the wet sheet and no medicine.

9th. He perspired freely after the cold application, and the perspiration continued all night, but had entirely ceased at the time of my visit, 9 a.m. The tongue is still very foul and red. To have the wet sheet again this evening.

10th. Skin is now quite wet with perspiration, which has been the case ever since the cold application; the tongue is not improved; the bowels are costive; the pulse is much more feeble; and he complains of weakness, but neither of fever nor headache. To have ol. ricini ℥ss; to be repeated in four hours if necessary. The cold application to be repeated if the fever should return.

11th. Better. Bowels open; tongue cleaner. The skin preserved its moisture all day yesterday, so that the wet sheet was not employed; but as the skin is now hotter and drier than at my visit yesterday, it was ordered to be used to-day. The pulse is more feeble, and he was ordered half a pint of beef-tea in the twenty-four hours.

12th. Very much improved. The tongue is clean; the bowels are open; perspiration occurred after the cold sheet, and has continued ever since; the skin is now quite cool; he complains of weakness.

13th. Better; and he was ordered meat diet.

The convalescence was at once established, and he was discharged on the 15th.

CASE IV. Joseph Timson, æt. 23, married, admitted October 8th. He had been seized with shivering on the 4th, and had had an attack every day since. He complained of muscular prostration, pain of head, limbs, and back, great thirst, and loss of appetite; his tongue was foul; his skin hot and dry; and his bowels costive. He was ordered the wet sheet immediately.

9th. Almost well. He perspired freely after the application of the cold sheet, and the perspiration still continues. His pain of head and limbs is gone; the tongue is foul, and his bowels are costive, which makes him feel uneasy. To have ol. ricini ℥j, and wet sheet if necessary.

10th. Perspiration has continued. The skin is quite cool and moist; the tongue is much cleaner; and the bowels are quite open. He was ordered meat diet, and to get up.

11th. Skin still continues moist. He feels much better; his tongue is not quite so clean; and his bowels have not been moved since yesterday. Repeat ol. ricini.

12th. Same as yesterday. Bowels still costive. Repeat ol. ricini.

13th. Feels quite well; his appetite is good; he has no return of shivering or fever; his bowels are freely open; and he wishes to go home. Discharged.

CASE V. John Foreman, æt. 16, admitted October 5th; but the wet sheet was not applied until the 8th. His state was little better than when he was

admitted; his bowels were purged; his skin was very hot and dry; and his tongue very foul.

9th. He is greatly relieved, having perspired freely ever since the application. He was ordered to have it repeated if the fever should return.

10th. Much the same as yesterday. The sheet was not again applied.

11th. Much the same. There was a return of fever last night, and his skin is not very moist, and rather hot. The wet sheet was administered.

12th. Has perspired freely all night; his tongue is clean; bowels open. Ordered to get up; to have rice pudding.

13th. Says he feels quite well. His discharge was delayed until the 15th.

Case VI. Foreman, Mary Ann, sister of the above, æt. 20, single, was attacked with shivering fourteen days back, but continued her work until the 14th of October. She was admitted on the 16th, late in the evening.

17th. Had a shivering fit after her admission last night, and when reaction came on she had the cold sheet applied as in the former cases. I found she had perspired freely all night, but her skin, though still moist, was very hot; her tongue was foul and red; her bowels open. She complained of great prostration and headache. She was ordered the liq. ammon. acet. and camphor mixture every four hours, low diet, and the wet sheet at night.

18th. She has been in a comfortable perspiration all night. The tongue is much improved; bowels are costive; headache much relieved. Cont. med.; omit the sheet.

19th. Bowels still costive, but tongue nevertheless cleaner. She has perspired freely, and her skin is now cool and moist. Omit medicine. To take ol. ricini ℥ss.

20th. Convalescent. Her discharge was delayed two days, on account of her peculiar work.

Case VII. Thomas Kean, æt. 19, admitted October 9th, from a dirty yard, where fever has been very severe. Had been seized with shivering on the 7th, and when seen was in a high state of fever, and was specially noted as promising to be a very bad case. I sent him immediately to the fever house, with orders to place him in the wet sheet immediately. This was done for ten minutes, and about twenty minutes after the sheet was removed he began to perspire.

10th. Relief is very marked. He has had a good night, and his head and back aches are gone, whilst his skin continues quite wet with perspiration; his bowels are costive. To have low diet, and ol. ricini ℥ss.

11th. His skin became hotter last night, and his pain and thirst slightly returned, and the wet sheet was therefore repeated. He is not much better. His tongue is clean, though red; bowels have been relieved four times. He was to get up, and have rice pudding.

12th. Bowels costive. Complained of headache towards the afternoon. Ordered to have the sheet if necessary, and ol. ricini ℥ss directly.

13th. Tongue less red; bowels open. Did not require the sheet; headache relieved.

14th. Better. He was discharged cured the day following.

Case VIII. Jane Smith, æt. 42, mother of six children, was admitted October 11th. She had been seized with shivering fourteen days before her admission, and had felt chills ever since. She was taken much worse on the 9th, so as to be unable to attend to her family. Her bowels were very costive, and on the morning of the 9th she took salts and senna, which have acted very violently. She now complains of intense headache, and aching of the back

and limbs, with thirst and great prostration. She was ordered the wet sheet and low diet; no medicine.

12th. Perspiration came on after the cold application, and has continued all night most profusely. The headache and lumbar pain are quite gone, and she complains only of slight weakness. To have meat diet, but to remain in the fever house.

13th. No return of shivering or fever. Her skin is moist, and tongue clean; feels very feeble, but is anxious to go out, as she thinks she can attend to her family. Discharged.

CASE IX. T. Cooper, æt. 30, living in a dirty, ill-ventilated house, was seized with shivering on the 9th of October, and has been unable to do anything since, the shivering having occurred every day, accompanied with the usual symptoms of the epidemic. When admitted on the 12th, he complained more especially of thirst, sore throat, and back-ache. His tongue was foul; his skin hot and dry; and his bowels reported open. Ordered to have the wet sheet.

13th. Has been in a good perspiration all night. Tongue is much cleaner; his bowels are costive. Altogether he feels much better, but he still complains of his throat. The fauces were red and swollen. Ordered ol. ricini ʒvj, and a mustard sinapism to the throat.

14th. Bowels not well relieved; skin cool and moist; tongue cleaner. To repeat the ol. ricini.

15th. Much better. Bowels freely open; no headache. To have meat diet.

18th. Discharged cured.

CASE X. Elizabeth Holmes, æt. 12, had been confined to bed a fortnight, admitted on the evening of October 16th, and first seen on the morning of the 17th. She had had a very restless night, and had moaned frequently. She was quite sensible, and complained of no pain. Her tongue is dry, brown, and cracked; the skin hot, very harsh, and dry. She has had frequent sickness, which is always removed by the exhibition of the medicine which she had been taking under the direction of her previous medical attendant. Bowels have been moved three times since her admission; there is no pain or tympanitis; pulse very feeble; the second stroke of the heart very indistinct; no cough or congestion of the lungs. To take potass. bicarb. gr. vj, ex aquâ 4tis horis; to have wine and beef-tea, and to have the wet sheet in the evening.

18th. She perspired after the cold application, and the perspiration continued until 1 a.m. to-day; after this she was very restless, and crying. Bowels are open; tongue same as yesterday; sickness ceased after the exhibition of the medicine; urine moderate in quantity high-coloured, clear; Cont. Mist. To have morphiæ acet. gr. $\frac{1}{8}$ horas omni. Diet as before. The sheet not to be repeated.

19th. She slept a little after the draught, but again became very restless, About 1 a.m. dyspnœa came on, which increased until 11 a.m., when she died. I saw her about 8 a.m., when the powers were failing, and the lungs were much congested. No post-mortem examination was allowed.

PART FOURTH.

Original Reports and Memoirs.

REPORT ON THE PROGRESS OF PRACTICAL MEDICINE,

IN THE DEPARTMENTS OF

MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN,

IN THE YEARS 1845-6.

By Charles West, M.D.,

Member of the Royal College of Physicians, Physician-Accoucheur and Lecturer on Midwifery to the Middlesex Hospital, and Senior Physician to the Royal Infirmary for Children.

This Report refers to a period of fifteen months extending from the 1st of July, 1845, to the 30th of September, 1846; and its general arrangement differs in no respect from that followed in the two previous Reports.

1. On the Progress of Midwifery.

A reprint of Dr. Murphy's Lectures on Parturition, which were originally published in the 'Lancet,' is the only new work that has appeared on this subject. Dr. Rigby's Lectures on Midwifery are now in course of publication in the 'Medical Times,' and Dr. J. H. Davis has contributed to the 'Lancet' very numerous papers on the management of difficult and preternatural labour.

PREGNANCY.

Signs of Pregnancy. Dr. Mikschick* has been led, by the examination of the urine of 50 pregnant women, to the same conclusion as many other investigators have already arrived at, with reference to the little value to be attached to the presence of kysteine in the urine as a sign of pregnancy. He found that in the majority of cases, an opalescent membrane formed on the surface of the urine after it had been allowed to stand for several days, but the same appearance was observed in many other instances independent of either lactation or pregnancy.

Disorders of Pregnancy. Mr. H. B. Lane† relates a case of that rare occurrence, *anteversion of the pregnant uterus.* The patient was a woman 35 years old, who fell down stairs in the 6th week of her pregnancy, striking her back and hips. Soon afterwards she began to suffer from tenesmus, dysuria, and frequent desire to pass water, and a varicose condition of the veins at the entrance of the vagina came on. When her pregnancy was 3½ months advanced, her state became much aggravated, and she could no longer pass water except in the horizontal posture. The os uteri could not be reached,

* Oesterr. Med. Jahrb., Dec. 1845.

† Lancet, Sept. 27, 1845.

but a tumour was felt through the superior vaginal wall, encroaching on its cavity, and so firmly fixed as to be quite immoveable. The use of the catheter afforded much relief, and though an attempt at the reposition of the uterus failed, yet the organ two days afterwards returned to its proper position, and the patient went to the full term of her pregnancy without suffering any further inconvenience.

Dr. Skae* has recorded an instance of *inversion of the uterus*, occurring 10 days *after abortion* at the 4th month of pregnancy. Considerable hemorrhage had occurred on the third day after the miscarriage, in consequence of the patient attempting to move about; but the uterus became inverted during an attack of vomiting, which was attended with a sensation of something falling down within her, and was followed by prostration of strength, bearing down, and flooding.

She was seen 12 hours after the accident, when the os tincæ was open to the width of two inches, and dilatable, and a tumour passed through it into the vagina. After two efforts, each continued for 15 or 20 minutes, the tumour was returned within the os uteri; but the fundus of the organ was not thoroughly reverted till the following day. The patient had since menstruated naturally, and continued well. [A somewhat similar case is related by Lisfranc, 'Clin. Chirurg.' iii. 380; but the inversion of the uterus, which had probably existed for five years, was not discovered until after the patient's death.]

Extra-uterine Pregnancy. Dr. Cogswell† has related a case of supposed *ovarian pregnancy*, in which the symptoms subsided between the 2d and 3d month, but the woman subsequently conceived and gave birth to two living children. In her second labour, it became necessary to puncture a soft tumour, situated between the vagina and rectum, which refilled, and, having been again tapped, continued to discharge a dark fluid, until the patient died exhausted, three months after delivery. The sac was found to be formed by the enlarged left ovary, which contained a pint of fluid besides some fœtal hair and bones. The case described as ovarian pregnancy by Dr. Harris‡, though the account given of it is very imperfect, may yet be decided not to have been an instance of extra-uterine pregnancy, but of ovarian disease, the right ovary being dropsical; hair and a portion of bone having been formed in the interior of the left.

A case of *Fallopian pregnancy* is related by Mr. Allport,§ which terminated fatally by hemorrhage, consequent on rupture of the tube in the 5th month of pregnancy; and another is related by Dr. Oldham‖ in which the patient died from the same cause at the 3d month. The uterus was in both cases lined with decidua.

Dr. Oldham's second case appears to have been one of *interstitial pregnancy*, the ovum having been developed in the uterine substance just behind the end of the tube. The tube was impervious, both above and below the supposed situation of the ovum, which appeared to have occupied a cell in the uterine substance large enough to contain a horse chesnut. Some doubt, however, is thrown on the real nature of the case, by the circumstance that the ovum, which had probably escaped through the ruptured walls of the cell, could not be found, and that the corpus luteum, though distinctly marked, was found in the ovary opposite to that side of the uterus where the supposed ovum was situated.

References are given below to several cases of *abdominal pregnancy*.¶ In

* Northern Journal of Medicine, July, 1845.

† Boston Medical Journal, July, 1845.

‡ Southern (American) Journal, July, 1846.

§ Lancet, Oct. 18, 1845.

‖ Guy's Hospital Reports, 1845.

¶ Grossi, Annales de la Chirurgie, Sept., 1845; Jobert et Dubois, Gaz. des Hôpitaux, July 5, 1845; Stevens, Amer. Journal of Med. Science, July, 1845; Yardley, ibid., April, 1846; Whinery, ibid., April, 1846; Craddock, American Medical Examiner, May, 1846; Mason, ibid., Jan., 1846; Carganico, Med. Zeitung, Aug. 13, 1845; Cerise, Gaz. des Hôpit., Jan. 12, 1846; McCulloch, British American Journal, Oct., 1845; Götz, Oesterr. Med. Jahrbüch, April, 1846.

the case reported by Dr. Grossi, the patient was still living, and the symptoms were by no means conclusive, while it is to be regretted that no attempts were made to determine, by means of the stethoscope, whether certain movements perceived through the abdominal walls were really due to the presence of a fœtus. A somewhat similar doubt attends the case which was under the care of MM. Jobert and Dubois, though the occurrence of pains resembling those of labour at the end of the 9th month, affords a presumption in favour of conception having taken place, which did not exist in the case reported by Dr. Grossi. It seems somewhat doubtful whether the case of Dr. Yardley's patient was one of extra-uterine pregnancy, or of rupture of the womb towards the end of gestation, from external violence, with escape of the fœtus into the abdominal cavity. After 6 months of severe suffering, the patient regained her health; she gave birth to a dead child four years afterwards, and subsequently miscarried thrice. During almost the whole of this time, the woman's health was indifferent, and her sufferings were severe, till at the end of 15 years the fœtal bones made their escape through the rectum, after which, in the course of the ensuing five months, she completely recovered. The account given by Dr. Craddock, refers to the dissection of a woman, in whose abdomen an extra-uterine fœtus had resided for 22 years without impairing her health; death taking place eventually from pneumonia. Dr. Carganico's patient died of hemorrhage, produced by rupture of the cyst at 5½ months. The anatomical details are very incomplete, but it is stated positively that the placenta was attached to the peritoneum in the pouch between the uterus and rectum, and not to the ovary. Dr. Mason's patient died exhausted, 4 months after the natural end of pregnancy; a communication having formed between the cyst and intestines, as well as an opening through the abdominal walls. The woman, whose history is recorded by M. Cerise, died after 15 hours of pain like that of labour, occurring at the full term of pregnancy. The anatomical details of the case are very imperfectly given, and the insertion of the placenta is not stated. In the cases of Drs. McCulloch, Whinery, Stevens, and Götz, gastrotomy was performed. More than 18 years had elapsed since the natural termination of pregnancy in the first case, more than 4 years in the second, more than 10 years in the third, but in the fourth case, the operation was performed at the end of 9 months, and a child was extracted, which survived for two hours. The placenta was large, and so firmly attached to the fundus uteri, that it was thought imprudent to attempt separating it. The patient went on well for a few days, but the placenta becoming partially detached, fatal hemorrhage took place on the eighth day after the operation. The insertion of the placenta was found to have been to the right ovary, and the right side of the fundus uteri. [These cases substantiate the general opinion that gastrotomy, at the natural end of pregnancy, is attended with great peril, while if restricted to patients in whom nature is, after the lapse of some time, endeavouring to get rid of the fœtus through the abdominal parietes, a successful issue may often be expected.]

NATURAL LABOUR.

Dr. Simpson* expresses the opinion that *galvanism does not exert any influence, either in originating or in increasing uterine action.* The experiments which led him to this conclusion were made on six women, in whom he carefully noticed the duration of the labour-pains, and of the intervals between their recurrence. He next repeated his observations with all the apparatus for galvanism prepared, but without establishing the contact, he then established contact, and lastly, renewed his observations after removal of the wires. From these experiments he infers that when uterine action has seemed

* Monthly Journal, July, 1845.

to be excited by means of galvanism, this has either been a mere coincidence, or has resulted from the impression made on the mind, or been produced by the mechanical irritation of the os uteri, or of the surface of the abdomen by the conductors. [These observations appear to have been made with great care, but can hardly as yet be allowed to outweigh the results arrived at by Reil and Carus in their experiments upon animals, and the recent evidence in favour of the reality of the influence of galvanism afforded by some of the cases which Dr. Radford has recorded.]

Plurality of Birth. Mr. Pretty* relates a case in which a woman gave birth in the 7th month of pregnancy to three male children, of whom only one was living. Cases in which four children were born in one labour are recorded by Dr. Beatty†, and Dr. Migliavacca‡. Three of the children in Dr. Beatty's case were born alive; one, an acephalous monster, was still-born. In the other case, the sex of the children is stated; three were girls, the fourth was a boy. They were all born alive, but all died speedily, the boy, who survived the longest, living only nine days.

PRETERNATURAL LABOUR, FROM CAUSES DEPENDING ON THE MOTHER.

Malformation of the Pelvis. Dr. Kirchoffer§ has described the case of a young woman who was delivered of her first child by the Cæsarean section, in consequence of the want of capacity of her pelvis, and died of hemorrhage five days afterwards. Her pelvis (of which a very interesting cast has been published) was found to be perfectly healthy, but presenting a remarkable similarity to the quadrilateral pelvis described by Robert of Marburg, of which mention is made in the first of these Reports. Like it there is defective development of the alæ of the sacrum, and anchylosis of both sacro-iliac synchondroses.

The distance between the two anterior superior spines of the ilium was only	7 inches
Antero-posterior diameter of the brim	4½ ,,
Transverse	3 ,,
Antero-posterior diameter of the outlets	4 ,,
Transverse	1 ,,

Dr. Kirchoffer subscribes to Naegele's theory of the congenital origin of this malformation, an opinion which is supported by Dr. Moleschott‖, in a well-written essay on the subject, though he does not adduce any new facts or arguments in its support.

Professor Martin, of Jena,¶ has revived the arguments on which he insisted some years ago, in order to prove this deformity not to be congenital. The history of a case recorded by Danyau, in which hip-joint disease, that had terminated by anchylosis of the head of the femur into the acetabulum, existed on the contracted side of the pelvis, is regarded by him as affording a confirmation of his views. The left hip-joint had been diseased, the left sacro-iliac synchondrosis was completely ossified, the left half of the sacrum was narrower than the right, as was also the left ramus of the pubes, and the whole of the left os innominatum was less perfectly developed than the right. The left oblique diameter of the pelvic brim was 4 inches, 4 lines, and the right, 3 inches, 10 lines. [This case might be regarded as affording support to Martin's theory, that the deformity is the result of an ulcerative process, in the course of which the bone becomes absorbed, and the disease finally cured by anchylosis of the joint, if we did not know that Vrolik's researches** have proved the effect of

* Med. Gazette, Dec. 5, 1845.
† Dublin Med. Press, Dec. 10, 1845.
‡ Gaz. Med. di Milano, May 9, 1846.
§ Neue Zeitschrift f. Geburtsk., xix, 3tes Heft.
‖ Zeitschrift für die gesammte Medicin, April, 1845.
¶ Neue Zeitschr. f. Geburtsk., xix, 18tes Heft.
** Essai sur les effets produits dans le corps humain par la luxation congénitale, etc., du Fémur. 4to. Amsterdam, 1839.

hip-joint disease to be a widening rather than a contraction of the affected side of the pelvis. A preparation in the museum of the Middlesex Hospital bears witness to the correctness of Vrolik's views, as probably do specimens in other anatomical collections. We must then conclude that the combination was in this case purely accidental.]

Dr. David and Mr. Gibson* have each related a case in which the *pelvis* was *narrowed by fracture of the sacrum.* In Dr. David's patient the accident took place in the fourth month of her fourth pregnancy, notwithstanding which she went to her full time, but delivery was then found to be impracticable, and consent not being given by her to the performance of craniotomy, she died undelivered. Mr. Gibson's patient was twice delivered by craniotomy after the occurrence of the accident, [but no reason is assigned for the induction of premature labour, with the view of saving the children, not having been attempted.]

Morbid states of the uterus. Cases of the *successful incision of the os uteri* in persons whose delivery was rendered impracticable by its occlusion, are related by M. Laborie, Mr. Davis, and Dr. Y. Ona.† One of M. Laborie's cases is of importance, since it illustrates an occasional source of danger from the operation; hemorrhage so profuse having followed the incisions as to place the patient's life in danger.

Dr. Lever‡ relates a very interesting case of *separation of the lower segment of the uterus* in a case where labour was obstructed by insuperable rigidity of the os uteri. The patient died on the 11th day after the occurrence, puerperal affection of the joints having come on the day following her delivery.

M. Devilliers§ has related the history of a lady who conceived after five years of childless marriage; an impediment to conception having existed in a *congenital constriction of the vagina*, which narrowed the passage so considerably as scarcely to allow the finger to pass beyond it. When labour came on, however, no interference was needed, the constriction yielding readily, and allowing parturition to be accomplished without difficulty. From this fact, he deduces the inference [which many cases already on record would substantiate] that congenital narrowing of the vagina may almost always be left to nature, although the acquired narrowing which results from inflammation and other similar causes generally needs interference.

Dr. W. Lange‖ mentions an instance of *persistence of the hymen*, rendering its division necessary during labour. [It seems likely, however, from the situation of the membrane, 1½ inch from the vulva, that it was not the hymen, but a secondary membrane situated beyond it.]

M. Danyau¶ describes a case in which labour was complicated by the presence of a large fibrous polypus connected with the anterior lip of the uterus, and which was forced by the labour-pains beyond the vulva. The woman had been delivered by a midwife, who turned the child before M. Danyau saw her; he, however, divided the pedicle of the tumour, which was two fingers' breadth in diameter, and no serious symptom followed the operation. In some observations appended to the case, he points out the danger of inducing abortion by meddling with such growths before pregnancy is terminated, but he does not think it necessary any longer to postpone operating after labour has taken place.

Inversion of the uterus. Dr. Fisher** describes a case of the spontaneous inversion of the uterus during labour at the 8th month; the fœtus and pla-

* Lancet, May 16, 1846; ibid., June 13, 1846.

† Gaz. Méd., Jan. 31, 1846; Med. Gazette, March 20, 1846; Amer. J. of Med. Science, Oct., 1845.

‡ Guy's Hospital Reports, 1845. § Revue Médicale, Août, 1845.

‖ Oesterr. Med. Wochenschr., Oct. 4, 1845.

¶ Gaz. Méd., Oct. 3, 1846, from Journal de Chirurgie, Juin, 1846.

** American Journal of Med. Science, May, 1846.

centa having been expelled together at at the moment when the accident happened. The cord, in this instance, was only eight inches long, and in a case related by Mr. Smith,* its length did not exceed six inches. The placenta was, in this instance, found partially detached from the inverted womb, immediately after the birth of the child, but after its complete separation the womb was returned to its proper position with but little difficulty. The uterus was likewise easily reduced in Dr. Fisher's case, and in neither was the accident attended by serious hemorrhage. A third case is related by Dr. Christie,† in which the patient died of hemorrhage, the uterus having been inverted, and the placenta in part detached as the result of traction at the funis by an ignorant midwife.

The note‡ contains references to several fatal cases of *rupture of the uterus.* The history of Dr. Hersing's patient strikingly illustrates the bad results of neglecting enbryotomy, and of trying to turn at all hazards, when the liquor amnii had long escaped, the uterus was acting spasmodically, and when, moreover, the prolapsed umbilical cord was felt to be pulseless. The accident occurred to Dr. Bona's patient, during an attempt to drag a child by means of the forceps through a pelvis with a conjugate diameter of two inches. Mr. Tyte, after allowing fifteen hours to elapse from the occurrence of symptoms of rupture of the uterus before he interfered at all, then applied the vectis, and afterwards the forceps, and finally delivered by craniotomy. In Dr. Reid's patient the accident occurred in the course of a natural labour, which had not lasted above 4 hours, the liquor amnii having escaped only half an hour, and the os uteri being dilatable. The case is interesting, moreover, from the absence of those symptoms of great nervous depression which usually come on immediately after the occurrence of laceration. The patient died in 36 hours, with symptoms of uterine inflammation, which had appeared to call for active treatment. Mr. Jackson's patient had been for 12 hours in her 4th labour, her former labours having been tedious, owing to slight contraction of the pelvis, when the uterus gave way, and the child escaped into the abdominal cavity. Gastrotomy was performed about three hours after the occurrence of the accident, and a dead female child extracted. The patient went on tolerably well for the first three days, when symptoms of extreme exhaustion came on, and on the 8th day she died. From a comparison of this case with thirteen others that came under his notice, Mr. Jackson draws a conclusion favorable to gastrotomy, whenever a considerable time has elapsed since the escape of the fœtus into the abdominal cavity; for in those cases in which either turning was performed, or the patient was left to nature, death always took place within three days, while the woman on whom gastrotomy was performed survived for 8 days.

Two instances of recovery after rupture of the uterus are recorded; the one by Dr. Williamson, the other by M. Robiquet.§ In the first case the feet of the child presented, one of which, during a violent pain, perforated the lower segment of the uterus, about an inch from the edge of the os. Immediate suppression of the uterine action followed the accident, but the foot being returned, labour was completed by the natural powers. The absence of bad symptoms in this case might be in some measure accounted for by the comparative slightness of the injury. In M. Robiquet's patient, the uterus gave way spontaneously after about 24 hours of tedious but not severe labour, and no very grave symptoms succeeded the occurrence. After the delivery of the child, which was effected by the forceps, a portion of omentum and a coil of small intestine prolapsed at the vulva. Soon after their return, however, the

* Monthly Journal, May, 1846. † Ibid.

‡ Hersing, Med. Zeitung, 30 Juli, 1845; Bona, ibid., Nov. 12, 1845; Tyte, Lancet, Feb. 7, 1846; Reid, Med. Gazette, Aug. 15, 1845; Jackson, Prov. Med. Journal, Sept. 3, 1845.

§ Northern Journal, Sept., 1845; Bull. de l'Acad. de Médecine, April 30, 1846.

uterus contracted well, no bad symptom supervened, and on the 17th day the patient was quite convalescent.

In a clinical lecture, by M. Roux,* on *lacerated perineum*, that surgeon expresses himself as decidedly in favour of operating some considerable time after delivery, rather than immediately or shortly afterwards. He considers the operation at the late period to be attended with much less danger of the supervention of erysipelas, and more likely to lead to the complete closure of the wound. In 13 out of the 15 cases on which he operated, his success has been complete, but in the remaining two cases the patients died. M. Roux employs the quilled suture, having previously pared the edges of the wound, and inserts only three stitches. For the first few days he keeps a catheter in the bladder, but does not consider it necessary to maintain a constipated state of the bowels after the 6th day, and so soon as they have acted freely he cuts out the stitches. The consolidation is quite firm, and there does not appear to be any increased tendency to laceration in a subsequent labour, but rather the reverse.

LABOUR PRETERNATURAL, FROM CAUSES DEPENDING ON THE CHILD.

Of this but few cases have been related. Two instances are recorded,† in which the *spontaneous evolution* of a second twin took place; the child in the former case being still-born, in the latter born alive.

Mr. Elton‡ has described a case of twin labour, in which the birth of the first child, which presented by the feet, was prevented by its head and that of the second child becoming locked together, just as in the case reported by Dr. Walter, and mentioned in the last Report. Mr. Elton decapitated the first child, and then extracted the second by the forceps, the liquor amnii having escaped so long before that he feared making any attempt to disengage the two heads. The second child was still-born as well as the first.

OPERATIVE MIDWIFERY.

Dr. Hoffman's report of the lying-in hospital at Würzburg§ is of much value as an unintentional illustration of the dangers of that over-fondness for manual interference that characterizes many of the continental practitioners, and especially of the hazard attending the *injudicious employment of the long forceps.* Dr. Hoffman applied the forceps 20 times in 637 labours, or about once in 30 cases. Dr. Ramsbotham once in 729 cases. The deaths after application of the forceps in Dr. Hoffman's practice were 35 per cent.; in Dr. Ramsbotham's practice 6·12 per cent. As illustrative of the dangerous force that may be exerted by this instrument, the case may be mentioned in which Dr. Hoffman having applied it while the head was at the brim of the pelvis, succeeded in extracting the child, but in so doing tore asunder all the joints of the pelvis, and lacerated the urethra and vagina, of which injuries the woman died on the 18th day. In a second case, the application of the forceps to extract a putrid child was followed by the mother's death in 29 hours. In another instance where the cord was prolapsed and pulseless, a dead child was extracted by the forceps, and the mother died 4 days afterwards of puerperal fever. In a fourth case, the long forceps was applied to draw a hydrocephalic head into the pelvis, and traction was made with the blades unlocked; though on a reapplication of the instrument, some time afterwards, it was found possible to lock it. Craniotomy was at length performed, but the woman died 24 hours afterwards, her vagina being extensively lacerated.

Craniotomy. M. Reali ‖ has related another instance of that horrible oc-

* Gaz. des Hôpitaux, Oct. 7, 1845.

† Hersing, Med. Zeitung, Dec. 3, 1845; Dr. Reid, Med. Gazette, Aug. 15, 1845.

‡ Med. Gazette, July 24, 1846

§ Neue Zeitschr. f. Geburtsk., xx, 1stes und 2tes Heft.

‖ Gaz. Méd., Sept. 27, 1845.

currence, the survival of a child for two hours after it had been extracted by means of Levret's *tire tête*, and quite a fourth of the brain had escaped during the operation. [It must be borne in mind that the instrument employed is a branched crotchet, which is applied without previous perforation of the skull.]

*Cæsarean section.** Four cases are related of the performance of this operation, with a favorable result both to mother and to child; 2 in which the mother survived, 6 in which the life of the child was preserved, and 2 in which neither life was saved. The history of the mother, in Mr. Goodman's case is not carried beyond the third week, at which time, however, she was doing well. In the patient on whom Dr. Meyer operated, the uterus contracted around the neck of the child so firmly after the body was extracted, as to render it necessary to enlarge the incision, an accident which illustrates the advantage of extracting the head first whenever that is possible. Mr. Lyon's case presents many points of interest. The operation was rendered necessary by the presence of a tumour blocking up the pelvis, and which, from its position behind the rectum, as well as from its firmness, was taken for an osteosteatomatous tumour of the pelvis. It turned out, however, to be the left ovary, enlarged and converted for the most part into an adipocire-like substance. [The case derives great importance from being almost, if not quite, the only instance of an ovarian tumour getting behind the rectum, and it illustrates the necessity of making an experimental puncture or incision of such tumours through the vagina, before exposing a patient to the dangers of the Cæsarean section.] In the case reported by Mr. Aitken, the uterus had given way before the patient's admission into the hospital, so that the Cæsarean section, which the extreme contraction of the pelvis rendered necessary, could not be regarded as the sole cause of her death. [The statistics of the operation at present yield the following results. It has been performed in 378 cases, of which trustworthy accounts have been given. In 145 of these cases the women recovered, in 233 they died; or the recoveries were in the proportion of 38 per cent., or as one in 2·6 cases. The fate of 318 children is mentioned, of whom 219 were saved, 99 were lost, or the child survived in 68 per cent., or in rather more than 2 cases out of 3.]

UTERINE HEMORRHAGE.

Professor Simpson† has published some remarks on the anatomical source and pathological nature of post-partum hemorrhage, which have a direct and very obvious bearing on the opinion he has expressed with reference to the source of the hemorrhage in placenta prævia, and the treatment applicable to some cases of it. He notices the fact that hemorrhage sometimes takes place after delivery, notwithstanding the existence of an average amount of uterine contraction, while, on the other hand, hemorrhage does not always follow the expulsion of the placenta, though the uterus is imperfectly contracted. Bleeding is prevented, not merely by the degree, but also by the equability and uniformity of the uterine contraction, while other means besides muscular contraction concur in producing the same effects. Hemorrhage from the detachment of the placenta is never arterial, but always takes place from the veins; the blood that ought to flow onwards towards the periphery of the

* Both lives saved—Dittmar, Gaz. Méd. de Strasbourg, and Dublin Journal, Nov., 1845; Long, Gaz. Méd., Sept. 13, 1845; Künsemüller, Neue Zeitschr. f. Geburtsk., xix, p. 384; Steinbrenner, Gaz. des Hôpitaux, Sept. 12, 1846. Mother survived—Goodman, Med. Gaz., Dec. 26, 1845; Meyer, Med. Zeitung, Sept. 10, 17, 1845. Child survived—Lyon, Monthly Journal, Dec., 1845; Jungmann, two cases, Oesterr. Med. Jahrb., Sept., 1845; Kirchoffer, Neue Zeitschr. f. Geburtsk., xix, 3tes Heft; Balfour, Northern Journal, May, 1846; Aitken, Lancet, June 13, 1846. The last two cases occurred at Vienna, and are merely reported by Messrs. Balfour and Aitken. Neither life saved—Künsemüller, loc. cit.

† Northern Journal, January, 1846.

uterus, and the ovarian and hypogastric trunks, regurgitating towards the cavity of the organ. Bleeding from the lacerated openings in the veins is checked as well by a peculiarity in the structure and arrangement of these vessels as by the constriction of their orifices by the uterine fibres. This peculiarity, noticed by Mr. Owen, in vol. iv, of the collected edition of Hunter's Works, and more recently by Mr. Goodsir, consists in the arrangement of the veins in successive tiers or planes, the veins of the lower tier communicating with those of the upper, by very oblique openings in the floor of the latter, while the opening into the lower vein is partially covered by a semilunar projection, formed by the lining membrane of the two venous tubes as they meet together at a very acute angle. It is probable, moreover, that these semilunar processes act something in the manner of the Eustachian valve, and thus further contribute to prevent the regurgitation of blood from above downwards. Another circumstance, which is not without its influence in diminishing the tendency to hemorrhage after the expulsion of the placenta, is that the derivative power which attracted the blood downwards into the placental cells no longer exists; and this having ceased, the blood will be more likely to flow in the onward current, and by direct channels, than to pass through the less free communications which exist between two different tiers of veins. The formation of coagula in the collapsed veins, and the presence of tufts of fœtal vessels, or remnants of decidua blocking up their openings, likewise contribute to the same result. It is obvious that these facts, assuming them to be correctly stated, lend much support to Dr. Simpson's opinion, which was also entertained by the late Dr. Hamilton, that the main bleeding in cases of placenta prævia takes place not from the uterine surface, but from the placental orifices of the lacerated veins, from which the blood would escape in its natural onward course, unchecked by any contractile power in the veins themselves, or in the tissue by which they are surrounded.

Dr. Radford,* Dr. Ashwell,† and an anonymous correspondent of the 'Medical Gazette,'‡ writing from Glasgow, under the signature of J. B., attack this theory of the source of the hemorrhage in placenta prævia as being physiologically impossible. Whether correct or not, however, this theory involves no contradiction of ascertained anatomical facts; its alleged incompatibility with the account given by Weber, Wagner, and others, evidently rests on a misunderstanding of Dr. Simpson's statement.

The pamphlet of M. Négrier,§ though written with the express object of explaining the causes of presentation of the placenta, and of laying down rules for the management of cases of hemorrhage from that source, contains little either new or valuable. The supposition that the placenta becomes attached to the cervix uteri, in consequence of the ovum having entered the womb while the decidua was as yet unformed, and having consequently gravitated to the lowest part of its cavity, has often been entertained, though now regarded as an inadequate, probably, an altogether incorrect explanation of the occurrence. The fact, too, that the fibres of the cervix are differently arranged from those of the body of the uterus, and that the cervix is likewise possessed of a smaller amount of contractile power, have both been long known, although brought forward as novelties by M. Négrier.

The papers on the subject of placenta prævia, by Drs. Simpson and Lee,‖ are occupied chiefly with the exposure of statistical errors into which each conceives the other to have fallen, and leave the main question of the propriety of detaching the placenta before the birth of the child, much as it stood last year. The interest of the subject, however, has led to the publi-

* Med. Gazette, Nov. 14, 1845. † Ibid., Nov. 7, 1845. ‡ Ibid., Nov. 21, 1845.

§ Recherches et Considérations sur la Constitution, et les Fonctions du Col de l'Uterus, &c. 8vo, Paris et Angers, 1846.

‖ Med. Gazette, Sept. 19, Oct. 10, 24, and Nov. 7, 1846.

cation of cases in which the placenta was spontaneously expelled before the child, as well as to its artificial detachment in various instances, where the experiment was conceived to be justifiable. Eleven cases are mentioned,* in which the spontaneous expulsion of the placenta occurred, the accident having happened twice to the same woman. All the mothers survived, except one, who died of the effects of the loss of blood on the 8th day. Nine of the children were still-born, one was very feeble at birth and died in a few hours, and one survived. The interval between the expulsion of the placenta and the birth of the child is stated in 7 cases. Thrice the interval did not exceed a very few minutes, once it was ten minutes, once half an hour, and once three hours. The hemorrhage appears to have ceased in every instance on the expulsion of the placenta, but in both instances in which the child was born alive its birth followed almost immediately on the expulsion of the placenta.

Seventeen instances have been recorded in the English Journals† during the past fifteen months, of detachment of the placenta before the birth of the child, in cases of placenta prævia. In the case recorded by Dr. Simpson, to whom it had been communicated by Mr. Cripps, the placenta was removed by an ignorant midwife, and 10 hours elapsed before the child was born, during which time, however, no hemorrhage took place. In 16 out of the 17 cases the bleeding is said to have ceased immediately on the detachment of the placenta, but Dr. Everitt mentions that although the flooding abated on the separation of the placenta, it did not entirely cease until after the application of cold externally; and he insists on the fact as proving that in cases of this kind, the hemorrhage comes from the uterine as well as the placental ends of the lacerated veins. The life of the mother was preserved in every case but one, and then the previous hemorrhage had been so profuse as almost to exhaust the patient, who died 3 hours after delivery. All the children were still-born, except in the case related by Mr. Stickings. [As far as the well-doing of the mother is concerned, the results of these cases must be regarded as favorable; but, on the other hand, the lives of 17 out of 18 children were sacrificed, at least half of whom would probably have been saved by the ordinary practice. In many instances, too, there appears to have been no reason why the child was not turned and extracted first; the os uteri having been well dilated, or yielding and dilatable. In such cases it seems not unfair to assert that the child's life was sacrificed to the desire of performing a new operation. Several of the cases are so loosely worded that little can be gathered from them, while some have either been so carelessly observed, or so incorrectly related as to render them quite untrustworthy.]

The opinion expressed by some practitioners, and acted on by more, that the new mode of treatment is generally applicable, and to be preferred to the ordinary practice, is opposed both by Mr. Crowfoot‡ and Dr. Radford,§ the latter of whom disclaims having recommended the detachment of the placenta before the birth of the child except under special conditions, these being the death of the child, the existence of so great a degree of exhaustion from loss of blood, as to render the ordinary mode of proceeding impracticable, or

* Goddard, Lancet, Dec. 3, 1845; Parker, Prov. Med. Journ., Sept. 24, 1845; Favell, ibid., Aug. 20, 1845; Reid, Med. Gazette, Nov. 25, 1845; Tweed, Lancet, Jan. 3, 1846; Ley, Prov. Med Journal, April 22, 1846; Russell, Ed. Med. Surg. Journal, July, 1846, p. 52; and Burwell, Amer. Journ. of Med. Science, April, 1846.

† Wilkinson, Prov. Med. Journal. July 23, 1845; Walker, ibid., Sept. 3; Greenhow, ibid., Sept. 10, 1845; Maclean, Northern Journal, Aug., 1845; Radford, Med. Gaz., Oct. 24, 1845; Jones, Lancet, Sept. 27, 1845; Wells, ibid., Nov. 8; Brown, ibid., Dec. 27, 1845; Simpson, Med. Gazette, Oct. 10, 1845, p 1011, note; Hutchinson, Prov. Med. Journal, Oct. 15, 1845; Houghton, Lancet, Jan. 24, 1846; Stickings, Med. Gazette, May 8, 1846; Wales, Prov. Med. Journal, April 8, 1846; Bryan, ibid., June 17, 1846; Jay, Med. Gazette, Aug. 21, 1846; Everitt, Prov Med. Journal, Sept. 30, 1846.

‡ Prov. Med. Journal, Nov. 12, 1845.

§ Ibid., Aug 13, 26; and Med. Gaz., Nov. 21, 1845.

the presence of some mechanical obstacle to the extraction of the child. He further condemns the employment of a sound, or of any instrument for detaching the placenta, and urges that the operation should be undertaken only when it can be accomplished by the finger, and when the introduction of the hand into the uterus has become practicable. Previous to this, the application of cold, but above all, the employment of the plug must not be omitted. These limitations would reduce the cases suited for the new plan of treatment to a very small number, but it must be borne in mind that Dr. Simpson* himself is very far from recommending it as always applicable, but confines it to those cases where rupturing the membranes is insufficient, and where turning is either impracticable or extremely dangerous. The cases in which he would practise it, are those in which the os uteri is rigid and undilated, the uterus too contracted, or the woman too exhausted for turning to be safely performed, or where the pelvis is contracted. Further, he would adopt it when the child is premature, or has been ascertained to be dead, and also in the greater number of cases of first labour, or of labour coming on before the 7th month.

Professor Osiander, of Göttingen,† is the only continental writer who has noticed this question, and he objects most strongly to the practice of detaching the placenta before the child, on the ground of its necessarily involving the death of the child, while he doubts, though apparently not on the ground of facts observed by himself, whether the practice be a certain means of arresting hemorrhage.

Many isolated cases are related of the successful treatment of placenta prævia by turning the child. References are not made to them in this Report, because the mere fact of that plan being adequate in the majority of instances is generally admitted, while nothing but a most elaborate analysis of cases of both kinds would render any inferences as to the comparative value of the two modes of treatment, drawn from their statistics, at all trustworthy. This is well illustrated by a statement of Mr. Russell's,‡ that he saved all the mothers, and 6 of the children, in 7 cases of placenta prævia that occurred in his private practice, while he lost 5 women out of 29 to whom he was called in consultation. The different results here depended not on the treatment, but on the time of its adoption.

Mr. Dorrington§ has related a case of placenta presentation in which he employed galvanism with apparent advantage, but the patient had not presented any very alarming symptoms, and galvanism was not employed alone, but in conjunction with rupture of the membranes, so that it is not easy to determine to which of these measures the excitement of vigorous uterine action, and consequent arrest of the hemorrhage, are to be attributed.

Dr. J. H. Davis‖ animadverts on Dr. Simpson's omission of all mention of the plug, as a means to be employed in the management of cases of placenta prævia, and insists on its extreme value in controlling hemorrhage until the state of the os uteri is such as to allow of turning. M. Négrier,¶ likewise, extols the plug, but at the same time he absolutely condemns the rupture of the membranes, since the cervix uteri being possessed of but little contractile power would not, in his opinion, close the bleeding vessels. In cases of central insertion of the placenta, he advises that the hand be introduced through it rather than to one side, since, by the former plan, the mother will lose less blood, while the fœtus will generally be born alive, if delivery be expeditiously accomplished. He recommends, moreover, the forcible dilatation of the os uteri, in all cases where women have previously given birth to children, since he thinks that in them it seldom offers any considerable resistance. [M.

* Med. Gaz., Oct. 10, 1845, p. 1013.
† Neue Zeitschr. f. Geburtskunde, xix, 3tes Heft.
‡ Loc. cit.
§ Prov. Med. Journal, March 11, 1846.
‖ Lancet, Nov. 8, 1845.
¶ Lib. cit., p. 133-170.

Négrier's own results, (he lost 4 patients out of 8) form the best comment on doctrines, which, but for the reputation of the author, would not have been noticed.]

A controversy has been carried on in Italy, which does not seem to have attracted much attention in other parts of the continent, between Dr. Bellini, of Florence, and some of his countrymen, with reference to the treatment of placenta prævia.* Dr. Bellini advocates the making incisions into the os uteri, in cases of hemorrhage from this source, in order to allow of the early introduction of the hand, and consequent early delivery of the patient. He condemns the use of the plug in these cases as being a means either wholly inefficient, or at best, suppressing the bleeding only for a short time, while it favours the occurrence of internal hemorrhage. On the other hand, incisions have often been made into the os uteri for the express purpose of enabling the practitioner to deliver his patient immediately, and no bad results have followed from this proceeding, even in cases where the use of the forceps has been needed to accomplish delivery. The four instances in which Dr. Bellini resorted to incision of the os uteri were not cases of placenta prævia, and it has accordingly been objected by Ciniselli, that the state of the os uteri, when the placenta is attached around it, differs from its condition in ordinary labour, it being thick and vascular, and consequently likely to bleed dangerously if incised. The other arguments for and against incision of the os uteri, and in favour or in dispraise of the plug present nothing remarkable.

Mr. Dorrington† relates two cases, and Dr. Radford‡ one, of the successful employment of galvanism to excite uterine action in accidental hemorrhage. In each instance the agent seems really to have had the effect attributed to it; but Dr. Radford's case is the most conclusive, since rupture of the membranes had been previously resorted to without the uterine action being in the least degree excited by it. Another case is recorded by Mr. Johnson§ of its employment to check dangerous hemorrhage from the uterus, a month after miscarriage.

A very interesting case is related by Dr. Pagan,‖ in which hemorrhage, after the expulsion of the placenta, appears to have been kept up by that body having been partially developed within the fallopian tube. The placenta was disrupted, and the hand, when introduced into the uterus, removed a portion, 2 inches long by half an inch broad, and expanding into a surface 2½ inches in breadth from one side of the fundus of the organ. He supports his opinion as to this deviation from the natural seat of the placenta being an occasional cause of flooding, by the detail of another case in which the patient died of peritonitis, she having had hemorrhage after delivery, for which the placenta was extracted, though lacerated in so doing. After death a portion of placenta, 3½ inches long, was found at the orifice of the tube, attached around its margin, and projecting far into its cavity. Some similar observations have been made by Riecke and d'Outrepont, to which Dr. Pagan refers.

Mr. Adams¶ has written some essays on floodings after delivery, and their treatment, which are remarkable for their opposition to all hitherto received opinions on the subject. He asserts that in the majority of cases hemorrhage after delivery does not result from want of uterine contraction; that the blood does not proceed from the interior of the uterus at all, but from the rupture of vessels about the os tincæ, or more frequently about the vulva, during the passage of the child.

* An account of part of this controversy is given by Schreiber, Neue Zeitschr. f. Geburtsk., xvii, 2tes Heft; besides which there are, an essay by Bellini, in Gaz. Med. di Milano, Nov. 15, 1845, and a defence of the use of the plug by Barbieri, in the same number of the journal; and by Casazza, ibid., Aug. 15, 1846.

† Prov. Med. Journal, March 11, 18, 1846.

‡ Med. Gazette, Jan. 2, 1846.

§ Prov. Med. Journal, March 25, 1846.

‖ Monthly Journal, Nov., 1845.

¶ Med. Gazette, Aug. 29, 1845, Jan. 23, 1846.

He concludes, therefore, that the introduction of the hand into the uterus is not only useless, but mischievous; that the local application of cold, gentle compression of the abdomen, and the administration of small doses of opium are usually all that is needed; and that if these means should fail, the bleeding vessel at the vulva must be sought for, and secured by ligature.

These extraordinary assertions are refuted by Dr. Ramsbotham* and Mr. Copeman,† who ably vindicate the usual mode of practice.

Dr. Beattie‡ has made some remarks on the value of the ergot of rye as a means of preventing hemorrhage after delivery, in cases where there may be reason to dread its occurrence. With this view, in cases where hemorrhage has followed the expulsion of the placenta in previous labours, or where the incomplete contraction of the uterus causes him to dread its coming on, he administers a dose of ergot immediately on the birth of the child. He conceives that the remedy acts both by lowering the action of the heart, as well as by inducing contraction of the uterus. He mentions, moreover, that he has found it of great service when administered in this way, as a means of preventing those intensely severe after-pains which some women suffer, and which opium fails to relieve. He conceives that in these cases it acts by producing a complete contraction of the uterine fibres, and thus preventing the formation of clots in the interior of the organ, by which spasmodic action is excited and kept up.

M. Négrier§ is one of a small number of practitioners who place confidence in the plug, as a means of arresting hemorrhage after delivery. He regards it as very useful in exciting uterine action, while he conceives that by pressure upon the womb, in the direction from above downwards, the danger of internal hemorrhage may be avoided. Two cases of its successful employment after delivery are related by Dr. Barbieri,‖ but in one of these cases 10, and in the other, 9 days had intervened between the woman's confinement and the commencement of flooding.

Mr. Pretty¶ suggests a modification of the pad and tourniquet, which have been used by different practitioners to suppress uterine hemorrhage after delivery. This modification consists in the introduction of two lateral pads to compress the sides of the uterus, in addition to the larger one, which presses it from before backwards.

Mr. Brown** has related a case where, exhaustion having come on after craniotomy, although it does not appear that the quantity of blood lost was very considerable, he performed transfusion, and succeeded in restoring the patient by the injection of ℥iv of blood.

THE PUERPERAL STATE.

Puerperal Convulsions. Dr. Mickschick,†† to whose investigations on the subject of kysteine reference has already been made, tested the urine of 26 women in labour, with the view of ascertaining the correctness of Lever's statements as to the presence of albumen in the urine in cases of convulsions. He detected it in the urine of 5 patients, one of whom was dropsical. Two women were attacked with convulsions, but albumen was found in the urine of only one of them. M. Labat‡‡ has reported an interesting case where puerperal convulsions came on in a highly anasarcous woman, who at length sank into a comatose condition. By degrees, as the anasarca diminished under the influence of remedies, the albumen, with which the urine had been

* Med. Gazette, Sept. 12, Oct. 24, 1845.
† Ibid., Nov. 7, 1845.
‡ Dublin Quarterly Journal, May, 1846.
§ Lib. cit., pp. 152-171.
‖ Gazetta Medica di Milano, Nov. 15, 1845.
¶ Med. Gazette, Jan. 16, 1846.
** Northern Journal, Dec., 1845.
†† Loc. cit.
‡‡ Gaz. des Hôpitaux, May 30, 1846.

loaded, disappeared, and the intellectual powers returned. Dr. Landsberg* has published a very prolix essay on the subject of puerperal convulsions, the chief aim of which is to point out that the danger attending them arises, not from the convulsions, but from the accompanying congestion of the brain, and that free depletion must, therefore, always be practised, while forcible delivery is not only useless, but dangerous.

Puerperal Fever. MM. Bidault and Arnault have given an account† of a very fatal epidemic of puerperal fever, which they observed among the lying-in patients at the Hôtel Dieu and Hôpital St. Louis, in the years 1843-4. Its greatest prevalence was during the colder months of the year, namely, from September to March, and its fatality was such, that of 34 who were attacked, only 2 recovered; and these 34 cases occurred among 156 women. The general characters of the disease, were those of the epidemic gastric fever of Locock. The veins of the uterus were unaffected, but its lining appeared, covered with a gray sanious false membrane; the uterine lymphatics, especially near the appendages of the organ, were full of pus and peritonitis, or purulent effusion into the peritoneum was present in every case. Among those who died at the Hôtel Dieu, there was likewise found an enlarged and diseased condition of Brunner's glands. The treatment consisted in the employment of depletion and mercurials; and the empirical administration of the tincture of aconite in some cases, among which were the two that recovered. From an editorial article in the 'Gaz. Médicale' of Feb. 28, 1846, the disease seems to have been extensively prevalent during the past winter, especially among the inmates of the hospitals. The atmosphere of the hospitals appears to have had a great share in producing it, for at the time when it was most prevalent, many women who were received into those institutions for various diseases, and who had been delivered some months previously, or who were menstruating, were attacked by inflammatory affections of the generative system. During the autumn of 1842, and till late in the spring of 1843, puerperal fever was very prevalent in Würzburg and its vicinity, and attacked 30 of 129 women who were delivered during that period in the lying-in hospital of the city, 14 of whom died. The uterine appendages, the peritoneum, and the membranes of the chest and head, were the parts most frequently affected, while the substance of the uterus was seldom implicated. The course of the disease was generally extremely rapid, and patients were attacked by it after the easiest labours, as often as after labours that had been tedious or difficult. Premature labour occurred in many cases, as though the seeds of the disease had been sown in pregnancy; a supposition which seems the more probable since uterine hemorrhage was likewise of frequent occurrence, and was often followed speedily by the outbreak of the fever. Although apparently originating under atmospheric influence, and appearing simultaneously in different parts of the city, there were yet many instances in which it was fairly traceable to contagion.‡ The contagious nature of puerperal fever, forms the subject of an elaborate compilation by Dr. Kneeland,§ and in a second similar communication, he endeavours to illustrate the connexion between it and epidemic erysipelas, which he believes to be produced by the same morbid poison. This supposition is supported by the interesting cases related by Dr. Keiller.‖ The essay on puerperal fever by Dr. Flint¶ is an attempt to arrive at useful conclusions, by the application of the numerical method to the analysis of 11 cases of puerperal fever, observed not by the writer himself, but by seven different physicians, who communicated the particulars more or less completely to him.

* Zeitschr. f. d. gesammte Medicin, July, Aug., 1846. † Gaz. Méd., Aug. 2, 1845.

‡ Hoffman, Neue Zeitschr. f. Gebnrtsk., xix, 2tes Heft, p. 194.

§ American Journal of Medical Science, January and April, 1846.

‖ Monthly Journal, Feb., 1846. ¶ New York Journal, July, 1845.

Mr. Bell* has reported eight well-observed cases of *pelvic inflammation ending in abscess.* Six of these cases occurred as the immediate sequelæ of parturition; the remaining two happened in women neither pregnant nor recently delivered, but who were exposed to cold while menstruating. There is nothing, however, particularly novel in Mr. Bell's remarks on the affection.

Lactation. Two cases of supernumerary nipples are related by Dr. Ashley, and Mr. Garthorn.† In the former instance, they were situated in each axilla, and were connected during suckling with a small tumour the size of an egg, which always disappeared after the woman had weaned her infant. After the patient's third delivery, milk occasionally flowed from these supernumerary nipples. In the other case, both breasts were well formed, but two nipples were situated on the left, about an inch apart, and milk flowed from each of them during suckling.

Midwifery Statistics, Reports of Hospitals, &c. The number of deaths in childbirth, as compared with deaths from other causes in this country, appears to be as 1:94.‡ Data do not exist, however, for a statement of its causes, while the great disparity between the returns of different districts, as 1:137, in the South Eastern Division, and 1:65 in the Northern Division, naturally leads to the inference that the returns of death under these circumstances are at present very imperfect.

The note§ contains references to the reports of different hospitals, dispensaries, &c., but all, with the exception of the reports of Dr. Reid and Mr. Earle, and of Professors Götz and Jungmann, refer only to a few hundred cases. Dr. Reid's report contains the result of 1771 cases, and the other three of about 4000 cases each. Their contents, however, are not of such a nature as to allow of abstract within the space of this Report.

II. On the Progress of Knowledge with Reference to the Diseases of Women.

Comparatively little has been done during the period embraced by this Report in this department of medicine. No new work treating of it has been published, but two more parts of Meissner's elaborate work have appeared. Dr. Rigby's reports on the diseases of women, have been continued in the 'Medical Times,' and many clinical lectures by Dr. Lever have appeared in the 'Medical Gazette;' and some very interesting cases are detailed by Dr. Mickschick‖ in his account of that division of the hospital at Vienna appropriated to the diseases of women.

DISORDERS OF MENSTRUATION.

Amenorrhea. Cases of amenorrhea, the result of an imperforate condition of the hymen, and cured by dividing it, are related by Drs. Barclay and Metcalf.¶ Dr. Krocker, jun.** operated on a girl in whom the menses had never appeared, in consequence of congenital imperforation of the vagina. Peritonitis followed, and proved fatal in 60 hours. It was found on a post-mortem examination that the cervix uteri had become greatly distended, forming a pouch $4\frac{1}{2}$ inches long, by $3\frac{3}{4}$ broad; but that though no separation existed between the neck of the uterus and its body, that part of the organ was very little, if at all, enlarged, and measured only 2 inches 3 lines in length.

* Med. Gazette, Dec. 12, 1845, Jan. 9, 16, 1846. † Lancet, Aug. 22; ibid., Sept. 12, 1846.

‡ Med. Gazette, Nov. 21, 1845.

§ Mickschick, Oesterr. Med. Jahrb., Oct., 1845; Siebold, Neue Zeitschrift f. Geburtsk., xix, 1stes Heft; Hohl, ibid., xix, 2tes Heft; Hoffman, ibid., 1stes und 2tes Heft; Waddy, Lancet, June 20, 1846; Campbell, Northern Journal, May, 1846; Reid, Med. Gazette, Aug. 15, and Nov. 28, 1845; Earle, Prov. Med. and Surg. Journal, June 10, 1846; Götz, Oesterr. Med. Jahrb., April, 1846; Jungmann, ibid., July, August, September, 1845, and July, August, 1846. ‖ Ibid., Nov., Dec., 1845.

¶ Prov. Med. Journal, Dec. 10, 1845; American Journal of Med. Science, July, 1846.

** Casper's Wochenschr., Oct. 18, 1845.

Two cases of the successful performance of the operation for congenital imperforation of the vagina are recorded by MM. de Bal and Kluyskens.* In each instance, relief was afforded by the first operation, and one of the patients had subsequently menstruated 7 times, while the other, since whose cure many years have elapsed, had married, but never had any family. The writers state that this operation has been performed with success by M. Amussat, as well as by M. Villiaume and M. Manoury, though Boyer and Dehaen both failed, having wounded the bladder instead of opening a passage to the uterus.

Dysmenorrhea. The valerianate of zinc, a remedy recently introduced, and reputed to be of great service in some hysterical and neuralgic affections, has been much extolled by Dr. Aldridge† as a cure for certain cases of dysmenorrhea. The dose of the preparation is gr. ¾ to gr. j two or three times a day, in the form either of pill or of solution.

Menorrhagia. Mr. Lane‡, who introduced the oxide of silver into medical use, records some cases illustrative of its power in checking hemorrhage from the uterus, whether menorrhagic, or resulting from malignant disease of the organ. The remedy is employed in doses of gr.ss. or gr.j. every 4 or 6 hours, and hemorrhages of a passive nature appear to be those most likely to benefit by it. A similar class of cases appear, from the experience both of Dr. Sée,§ and Dr. Ebers‖, to benefit by the use of the ergotine, in gr.ij. doses every 4 hours. The existence of febrile disturbance contraindicates its employment, the stomach under such circumstances often rejecting it. The ergotine is said to be preferable to the ergot of rye, of which it is the essential principle, not merely from its being a much less bulky medicine, but because its long-continued use is found not to be followed by those unpleasant nervous symptoms which have sometimes resulted from perseverance in the administration of the ergot.

DISEASES OF THE UTERUS.

Means of investigating them. Mr. Fergusson, of King's College,¶ has constituted a speculum which has all the advantages of the ordinary glass speculum, together with others peculiar to itself, while it is quite free from the danger of breaking, which has been urged as an objection to that instrument. It consists of a glass speculum, coated on the outside with silver leaf and varnish, then with cotton cloth, to prevent its breaking, and then with caoutchouc, which gives a smooth external surface. This instrument, which is by no means expensive, presents a very brilliant reflecting surface, not liable to tarnish.

Displacements of the Uterus. Dr. Edwards** relates an instance of that rare form of displacement of the uterus, *anteversion.* The patient in whom it existed was not pregnant, and the symptoms, which had come on without any evident cause, had gradually increased in severity. There was no considerable difficulty in defecation, but both pain and difficulty in passing water, attended with very frequent desire to void it. The os uteri could not be brought into view by the speculum, but a large tumour was felt towards the pubis, and it was only by pushing up this body that the os uteri could be brought within reach of the finger; the neck of the womb was large, and directed backwards.

After some preliminary local depletion, and emptying the rectum by means of an enema, the position of the uterus was easily rectified by pushing up the fundus uteri with the index-finger of one hand, and at the same time drawing down the cervix with the fore and middle fingers of the other.

Mr. Gregson†† has added another to the list of successful removals of the

* Gaz. Méd., March 28, 1846.

† Dublin Hosp. Gaz., Aug. 15, Sept. 1, 1845.

‡ Med. Gazette, May 1, 1846.

§ Gaz. Méd., Aug. 8, 1846.

‖ Med. Gazette, Jan, 9, 1846.

¶ Ibid., Dec. 19, 1845.

** Lancet, Feb. 7, 1846.

†† Med. Gazette, Feb. 20, 1846.

inverted uterus by ligature. The inversion had existed since the patient's delivery two years before, and for the last 18 months she had suffered from constant and profuse hemorrhage. The ligature, which included the whole body and cervix of the uterus, came away on the 9th day, and 3 months after the operation, the patient continued perfectly well.

Inflammation and ulceration of the os and cervix uteri. Dr. H. Bennett* has published a series of cases, some of them of considerable interest, illustrative of the views on ulceration of the cervix uteri contained in his recent work on that subject; and some papers on similar topics have been published by MM. Boys de Loury and Costilhes†. M. Pichard‡ attacks what he regards as the too general employment of local cauterization for the cure of various diseases of the os and cervix uteri. The train of argument which he pursues, is to the effect that the cervix uteri being composed of various dissimilar tissues, while the effect of caustics is not confined to the one supposed to be diseased, but extends to all, changes in all result, and any tendency that may exist to cancerous degeneration is thus called into activity. [It cannot be denied that cauterization of the os uteri is sometimes improperly resorted to, and that in other cases a milder treatment would have sufficed for the cure of the patients' ailment. M. Pichard, however, though he speaks of a "host of observations," does not adduce them in confirmation of his statements that cancerous degeneration is often induced by this treatment. In the only case which he relates, where the death of the patient succeeded to cauterization of the cervix, he did not see the patient during the last month of her life, no examination of the body was made after death, and she is reported to have died of carcinoma, only on the evidence of her daughter as reported to M. Pichard by a third party. Hence, although treating of a subject that well deserves careful investigation, the book is in the highest degree unsatisfactory. The remarks on amputation of the cervix contain nothing new.]

Dr. Roberts,§ in two very elaborate essays, treats of *leucorrhea*, and of the importance of employing the speculum in cases where it is present, since it is not a mere increase of the vaginal discharge from debility, but is almost always symptomatic of uterine disease, especially of inflammation of the os and cervix, or more rarely of the lining membrane of the womb. The facts on which his conclusions rest, contain nothing new, but afford a confirmation of the statements of most other writers who have made frequent use of the speculum. M. Gibert|| strongly recommends an alcoholic tincture of tannin, mixed with seven parts of water as an astringent injection in leucorrhea, and in cases of ulceration of the os uteri. Dr. Mitchell¶ speaks of the employment of the actual cautery to the spine as a means of relieving that extreme pain in the back, which attends some of those cases of leucorrhea where there is very great tenderness of the cervix uteri. It does not appear that he employs the cautery so as to produce a slough, but that he uses it as a mild counter-irritant, similar in its operation to the moxa, though far less severe.

Polypus uteri. A paper on this affection has appeared from the pen of Dr. Montgomery,** containing much valuable information. He notices the frequent occurrence of very small uterine polypi, which may not merely escape detection on a vaginal examination, but may even fail to be discovered by the speculum, owing to their being situated between the lips of the os uteri. Even these small polypi, however, are a common cause of ulceration and menorrhagia, the cure of which requires, as a preliminary step, the removal of the polypus. These small polypi derive additional importance from the circum-

* In the Lancet for the autumn of 1845, and summer of 1846.

† Gaz. Méd., June, July, August, and September, 1845.

‡ Des Abus de la Cautérisation, et de la Résection du Col dans les Maladies de la Matrice. 8vo. Paris, 1846.

§ New York Journal, April, July, 1845.

|| Gaz. Méd., Aug. 9, 1845.

¶ Dublin Medical Press, Oct. 7, 1846.

** Dublin Quarterly Journal, August, 1846.

stance that they are seldom solitary, but for the most part, associated with other kinds of polypi, and especially with fibrous tumour, and that when met with in women at an advanced age, they are often the precursors of some malignant uterine disease. He points out two sources of error, into which the practitioner is likely to fall; the one is the mistaking a tumefied and somewhat elongated condition of the extremity and inner surface of the anterior lip of the os uteri (which sometimes exists in cases of long continued ulceration) for a polypus; the other is the taking the pain and other symptoms attending a polypus for the indications of cancer of the womb; an error in which the practitioner may be confirmed, if he make a hasty examination at a time when a large polypus is in the act of passing through the os uteri. Dr. Montgomery prefers the ligature of polypi to their excision, since, though slower in its action, it usually has the immediate effect of restraining the morbid discharges and alleviating the symptoms, while the hemorrhage, often dangerous after the excision of a large polypus, is sometimes troublesome even when the growth removed is small and its pedicle slender. He adds one important caution, with reference to the management of cases of long standing polypus, attended with copious discharges, which has not been given by previous writers. It is to the effect that after their removal, a condition of the system requiring precaution against determination of blood to the head is likely to follow the suppression of the hemorrhage, to which the patient had become as it were habituated.

Dr. J. H. Bennett* repeats a caution [already given by Lisfranc, 'Clin. Chirurg.' t. iii, p. 210] with reference to the tendency of the wound left after either the ligature or excision of a polypus to degenerate into a troublesome ulceration, and insists on the importance of examining in such cases with the speculum after a patient has recovered from the operation, in order to ascertain whether or no any such ulceration has been left behind. Dr. Montgomery† notices the same fact, but is disposed to attribute the ulceration to the polypus itself, rather than to the means used for its removal, A clinical lecture by M. Lisfranc,‡ on occasion of the death from peritonitis of a patient affected with polypus uteri, in whom the growth had with much difficulty been forced by the uterine efforts through the mouth of the womb, contains some interesting observations. M. Lisfranc remarks that in any case where the symptoms of uterine engorgement, after having been apparently cured, return without evident cause, again disappear under treatment, and once more causelessly return, there is reason for fearing the presence of a polypus. He mentions having seen two or three instances in which the violent expulsive action of the uterus, that is usually regarded as indicative of the presence in its cavity of some body of which it is endeavouring to get rid, occurred although it was quite empty. Lastly, he debates on the conduct to be pursued while the womb is endeavouring to get rid of its contents, which he decides should be merely palliative and expectant; while in those exceptional cases, only three of which have come under his notice where peritonitis followed the expulsion of the polypus from the uterine cavity, all surgical interference must be postponed until the peritonitis is cured. His chief reason for preferring the excision of polypi to their removal by ligature is that the pedicle often being in part composed of uterine substance, there is greater danger of inflammation of the womb following the use of the ligature than of the knife. That the danger of inflammation, however, is not always avoided when the knife is used is shown in the history of a woman aged 42, from whom M. Corni§ removed a large polypus by incision. A severe attack of metro-peritonitis came on on the fifth day, from which the patient recovered only after the employment of very active antiphlogistic measures.

* Lancet, July 19, 1845.
† Loc. cit., p. 33.
‡ Gaz. des Hôpitaux, Sept. 15, 1846.
§ Gazetta Medica di Milano, March 21, 1846.

Dr. Herrich* has endeavoured to improve the operation of excision by inventing a new knife for the purpose, he having been deterred from the use of the ligature as a means of extirpating uterine polypi by a case where the growth being of remarkably dense texture the ligature failed to divide its pedicle, but uterine inflammation was induced, which terminated fatally on the ninth day. He proposes to employ a knife, the stem of which is curved in correspondence with the size of the polypus, while the blade, of a semilunar shape, is placed at right angles to this stem. The blade does not taper towards its extremity, and its only cutting edge, which corresponds to its concavity, is concealed by the same kind of arrangement as that of an ordinary *bistoire caché*, till the knife has been brought to that part of the pedicle which it is intended to divide. The sheath being then withdrawn, and one finger being placed on the extremity of the blade to retain it in the right direction, a slight sawing movement suffices to divide the pedicle. This instrument has been used by Dr. Herrich in two cases, in both of which the polypus had passed through the os uteri, and the scissors might probably have been employed with success. He considers it, however, to be applicable even to cases where the finger cannot be introduced to guide the incision, and the rather, since the cut will always be made at right angles to the body of the polypus and not obliquely, as must be the case when the scissors are employed.

Malignant diseases of the uterus. In Dr. Montgomery's paper already referred to, he mentions that a patient from whom he removed a cauliflower excrescence of the uterus by ligature, and whose case he reported in the 'Dublin Journal' for Jan. 1845, still continues well after the lapse of nearly three years.† He likewise relates a second case, where he removed a similar growth by the same means from a patient aged 60. In five days, the growth was detached; the discharges ceased, and the patient's health became perfectly re-established. She continued quite well for four months after the operation; the discharges then began to return, and at the end of another three months the growth had regained its former size, but the patient would not consent to a repetition of the operation. A case is related by Dr. Bodenstab,‡ in which he extirpated the uterus by Langenbeck's operation, by which the peritoneum is not opened. The loss of blood was inconsiderable, but the patient fainted towards the close of the operation, which lasted nearly half an hour, and though she rallied for a short time, syncope came on again, and ended in death, about three quarters of an hour after its completion.

DISEASES OF THE UTERINE APPENDAGES.

Diseases of the ovaria. Dr. Bennett, of Edinburgh,‖ has made some observations on the anatomy and pathology of encysted tumours of the ovary. He notices the doubts that have existed with reference to the source of the fluid found in the abdominal cavity in many cases of this disease, in which it is evidently neither a simple passive effusion, nor the result of inflammatory action. He conceives that in many instances this fluid has originally formed within the ovarian cyst, but escaped from its cavity through some of those ulcerated openings in its walls, that result from the distension of the sac by the accumulation of fluid. Sometimes the adhesions of the cyst to the abdominal walls prevent this occurrence, in which case the only change that takes place is the progressive breaking down of the septa between the different cysts, by which process the dropsical ovary comes in course of time to be composed of one or two large sacs, instead of a number of small cysts either altogether separate from each other, or communicating but very par-

* Ueber Gebärmutter-Polypen, und deren Ausrottung. 8vo, Regensburg, 1846.

† This case is referred to in last year's Report, where, by a typographical error, it is stated that two months had then elapsed since the performance of the operation, instead of 21 months.

‡ Neue Zeitschr. f. Geburtsk., xviii, 2tes Heft. ‖ Edinburgh Med. Surg. Journal, April, 1846.

tially. It is not until the morbid growth has reached this stage that any considerable impairment of the general health begins to take place, an occurrence which he believes to be often symptomatic of the commencement of inflammation and suppuration in the interior of the cyst. He assigns as a reason for postponing tapping as long as possible, the importance of giving time for the occurrence of those changes in the cyst by which it is reduced from a compound to a simple one, since there may then be some ground for hoping that the employment of pressure after tapping may give rise to adhesive inflammation of its walls. He attaches much importance to the uterine sound as a means of discovering the real seat of abdominal tumours, and points out the importance of microscopic examination of the fluid removed from a dropsical ovary as furnishing a clue to the nature of the disease by which it is affected.

A case of permanent cure following the accidental rupture of an ovarian cyst as large as the first is related by Dr. White.* Severe peritonitis followed the accident, and the patient's convalescence was very tedious, but on her recovery the tumour was found to have completely disappeared.

Mr. J. B. Brown and Mr. Hunt,† each relate a case of ovarian dropsy treated by compression, combined with the administration of mercurials and diuretics. The patients are stated to have eventually recovered, though the treatment adopted appears to have placed the lives of both in imminent hazard. In both the mercurial produced most distressing ptyalism, and the diuretics excited painful and almost incessant vomiting. The extremely tight bandaging after tapping induced in Mr. Brown's patient suppuration of the cyst, from which on a repetition of the tapping ten pints of pus escaped. The life of the patient was for many weeks in great danger, but when she got well, the size of the abdomen was found to be much diminished, and the cyst was felt collapsed, hard, and painless. In Mr. Hunt's case the medicines disagreed, the tight bandaging after tapping caused symptoms which could not be relieved except by the removal of the bandage; a step, however, which was not taken until after symptoms of inflammation and fever with delirium had come on. Tapping was performed on Nov. 12; from Nov. 14 to Dec. 3 the patient was very dangerously ill; but on Dec. 9, when she was pronounced convalescent, we are informed that the tumour had entirely disappeared. The results of vaginal examination are not stated, an omission the more to be regretted, since the presence of this tumour in the recto-vaginal pouch two years previously had led Dr. Blundell to sanction the induction of premature labour.

Dr. B. Alison, of Indiana,‡ relates the history of a patient, who having suffered long from ovarian dropsy, and been frequently tapped, was losing her health very rapidly, when he injected a solution of iodine into the sac from which the discharge had for some time been kept constantly flowing by means of a kind of tent that was removed at pleasure. Neither the quantity nor the strength of the solution is stated, but the symptoms which followed its injection are said to have been truly alarming. On their subsidence, however, the patient's health improved greatly, though a little pus still followed the daily withdrawal of the tent. [The introduction of the tent to allow of the daily withdrawal of the fluid resembles the proceeding of Ollenroth mentioned in the Report for 1842-3. The injection of stimulating fluids, suggested by the late Dr. Hamilton, of Edinburgh, though abandoned by him from its bad results, has since been tried by many English and continental surgeons, though generally with unfavorable consequences.]

Mr. Southam and Mr. Dickins§ have extirpated a dropsical ovary with success; the operation not having been succeeded by bad symptoms in either case. Mr. Southam's patient walked a distance of three miles on the nine-

* American Journal, April, 1846, p. 547.

† Lancet, Jan. 10; ibid., Jan. 24, 1846.

‡ Medical Examiner, June, 1846.

§ Prov. Med. Journal, Sept. 10; ibid., Oct. 1, 1845.

teenth day, and Mr. Dickins's patient was up and walking about at the end of three weeks. In both instances the remaining ovary is said to have been quite healthy. Mr. Southam likewise states that the patient on whom he operated in 1843 continues quite well.

Dr. Hayny, Mr. Solly, and Dr. Handisyde,* operated without success. In one of Dr. Hayny's patients it was found, after the abdomen was opened, that the tumour had contracted such extensive adhesions as to render its removal impracticable. In the other the operation was completed, but it was found necessary to remove with the tumour a portion of omentum that adhered to it. The first patient died, apparently exhausted, on the fourth day, the other had several attacks of peritonitis, but lingered on for six weeks, when she died. [In neither instance was any attention paid to the regulation of the temperature at the time of the operation, or to those other points in the general management to which so large share in producing the favorable issue of Dr. Clay's, Mr. Walne's, and other English cases seems attributable.] Mr. Solly's patient died of internal hemorrhage eleven hours after the operation, and Dr. Handisyde's, whose progress was at no time satisfactory, died seventy days after the removal of the ovary from strangulation of the small intestine by a band of lymph encircling it, and which appeared to have been thrown out during the course of three inflammatory attacks after the operation, of which the pelvic viscera showed ample evidence.

DISEASES OF THE VAGINA AND EXTERNAL ORGANS.

Vesico-vaginal fistula. Dr. Zechmeister† relates a case, in which the very frequently repeated introduction of the catheter appears to have been followed by the cure of a fistula of a year's standing. The instrument was at first introduced every hour, afterwards every two or three hours, thus gradually increasing the intervals between the times of employing it. M. Tripet‡ gives a minute detail of M. Bérard's operation for the cure of vesico-vaginal fistula (mentioned in the last Report), by inducing occlusion of the vagina. From the history which he gives of the patient's fatal illness, her death would seem to be scarcely due to the effects of the operation. A new operation has been suggested for the cure of this accident by M. Jobert§ Its peculiarity consists in detaching a small portion of the vagina by a transverse incision from the cervix uteri before inserting the sutures into the edges of the fistulous opening. The result of this is that the edges are very readily brought into contact, and that all stress upon the sutures is prevented It is said to have succeeded in the three cases in which it has hitherto been tried.

Dr. Oldham|| has published a fuller account of that *follicular disease of the vulva,* concerning which he contributed a brief notice to Dr. Ashwell's work on diseases of women. [The situation to which he states the disease to be generally limited, namely, two symmetrical strips of mucous membrane at the posterior half of the entrance of the vagina, within the nymphæ, together with the symptoms of itching, and pain on walking, with a thick discharge, seem to point to Duverney's glands as the probable seat of the disease, though Dr. Oldham does not appear to have investigated their condition. The late Dr Fricke, of Hamburg, was the first to call attention to this affection at the scientific congress at Hamburg, in 1838.¶

III. On the Progress of Knowledge with reference to the Diseases of Children.

A new edition has appeared of Dr. Underwood's well-known work on the diseases of children, edited by Dr. H. Davies, many of whose notes contain

* Oesterr. Med. Jahrb., August, September, 1845; Med. Gazette, July 10, 1846; Edinburgh Med. Surg. Journal, April, 1846.

† Oesterr. Med. Wochenschrift, August 16, 1845.

‡ Arch. Gén. de Méd., September, 1845.

§ Gaz. des Hôpitaux, Jan. 24, 1846.

|| Med. Gazette, May 15, 1846.

¶ A very brief notice of his statements is contained in Rust's Magazin, Bd. xxxiii, p. 143.

very useful practical information. Dr. Coley* has likewise published a work on the same subject, which, while it contains some interesting observations derived from his own experience, is for the most part a compilation from the writings of others. Dr. Friedberg's† treatise on the diagnosis of children's diseases, is entirely a compilation, generally well executed. M. Legendre's‡ volume of essays is a work of much value, consisting indeed chiefly of papers already published, but containing some new contributions, all of which are evidently the fruit of careful and patient observation. The essays which have formerly appeared, are those on hydrocephalus, cerebral hemorrhage, fœtal condition of the lung, and the simultaneous development of variola and vaccinia. The other articles will be noticed in the course of the Report.

I. DISEASES OF THE FŒTUS.

Dr. Beatty§ has described a fœtus expelled at the fourth month of pregnancy, the left arm of which was nearly severed by the umbilical cord twisted around it, as has been noticed in other instances of *spontaneous amputation of the limbs in utero.*

Dr. Lasserre‖ has related the history of two still-born children, in one of whom *meningeal apoplexy* existed; in the other, which was expelled at five and a half months, there was copious *hemorrhage into the ventricles.* This last fœtus is said to have been dead nearly a fortnight.

Dr. Pastorello¶ saw a child born alive, and living for 2½ hours, whose *hands and feet* were entirely *destitute of epidermis;* the true skin of those parts looking like that of a dead and already putrifying child. The mother is said to have been healthy, and to have previously given birth to healthy twins.

Dr. Götz** describes a case of congenital disease of the skin, which appears to have been the *ichthyosis intra-uterina*, of which instances were mentioned in the last two Reports. The child was a female, born at the full time, of a healthy mother, aged 25 years, in whose pregnancy nothing remarkable had occurred, except that she was much frightened in the 8th week. The fissured condition of the skin, and the eversion of the eyelids were at first very remarkable, and the fissures for some time reappeared after each desquamation. After each, however, the state of the surface improved, and at the time of the child's death, completely atrophied, when 5 weeks old, the skin was merely covered with white scales.

Dr. Schneider†† mentions having twice met with *congenital bronchocele* in children otherwise healthy. In one instance the tumour was as large as a goose's egg, in the other its size was somewhat smaller. Inunction with a weak iodine ointment caused the swellings to disappear in less than 3 weeks. He adds references to similar cases, mentioned by other writers, and extracts from the 'Transactions of the Academy of Sciences at Bologna,' the history of a still-born fœtus, in which an enormous tumour occupied the whole neck, extending downwards towards the sternum, and likewise reaching upwards, especially on the left side, so as greatly to disfigure the face. The tumour was found by M. Mondini, who observed the case to be formed by the enlarged thyroid gland, part of which presented a cellular structure, like that of ordinary bronchocele, while the lower part of the growth more nearly resembled fungus hæmatodes in its characters.

Dr. Hermann‡‡ describes *appearances* which he found *in the lungs* of a fœtus

* A Practical Treatise on the Diseases of Children. 8vo, London, 1846.

† Diagnostik der Kinderkrankheiten. 8vo, Berlin, 1845.

‡ Recherches Anatomo-Pathologiques et Cliniques, sur quelques Maladies de l'Enfance. 8vo, Paris, 1846.

§ Dublin Med. Press, Dec. 24, 1845.

‖ Monthly Journal, April, 1846.

¶ Annali Universali, July, 1845.

** Oesterr. Med. Jahrb., June, 1845.

†† Casper's Wochenschr., July 18, 1846.

‡‡ Oesterr. Med. Wochenschr., Feb. 21, 1846.

still-born at the 7th month, resembling what have been described by some writers as *the results of inflammation*, an opinion in which Dr. Hermann coincides. The substance of both lungs was solid and of a reddish brown colour. On their surface, as well as in their substance were numerous small patches, varying from the size of a lentil to that of a bean, of an irregularly round form, and a dirty grayish tint, exceedingly firm and dense, presenting an indistinctly granular structure when divided, and infiltrated with a gray, adhesive matter. [Similar appearances are enumerated at p. 166 of Graetzer's work on 'Diseases of the Fœtus,' who is disposed to regard them as the consequences of inflammation.]

2. GENERAL OBSERVATIONS ON THE DISEASES AND MANAGEMENT OF INFANCY AND CHILDHOOD.

Diet in infancy. Dr. Klencke* calls attention to the important deterioration which the milk of stall-fed cows undergoes, and is inclined to attribute the production of scrofula in children in many instances, to its direct transmission through the the medium of that fluid. Although the direct production of scrofula by the contagious properties of the milk is assumed rather than proved in this pamphlet, still the fact is very important that stall-fed cows often become tuberculous, and that their milk loses much or even the whole of its sugar, that the butter and casein diminish, while albumen is found, sometimes in as high a proportion as 15 per cent., and elain in the proportion of 1·4 per cent., and that in some cases lactic acid is likewise present.

In a well-written paper on the subject of diet in children, Dr. Marotte† draws attention to the error often committed in placing infants on a spare diet, who have been observed not to thrive at the breast, but to suffer from diarrhea and to lose flesh. The real means of cure would consist in obtaining a wet-nurse for the child, and thus providing it with a more instead of a less nutritious food. Many instances of gastro-intestinal disorder in childhood depend, in his opinion, on the want of a more highly animalized diet. It is therefore, as a general rule, undesirable to dilute the milk of the herbivora, already poor in animal constituents; while in those cases where it is necessary to supply deficiency in the nutritive qualities of the nurse's milk, chicken or other broth, either alone or mixed with milk, should be used for that purpose.

Infantile therapeutics. A manual on this subject has been published by MM. Berton and Schulz.‡ It is decidedly inferior to Dr. Ure's little work on the same subject, which appeared in this country some years ago.

In a paper on the use of opium in childhood, Dr. Sobotka§ takes what seems to the writer of this Report, a very exaggerated view of the dangers of its administration, and expresses opinions which, if generally received, would banish this drug from practice in the cases of children. In many of the cases that he relates as illustrative of the mischievous results produced by opium, diarrhea had existed for some time, and it may be questioned whether the head symptoms were not the consequences of that, rather than of the administration of opium. A very interesting case is related by Dr. M. Barry,|| of an infant aged 9 months, which had been poisoned by 30 drops of laudanum, and was not seen till seven hours afterwards, when in a state of profound coma. From this state it was roused by the employment of electro-magnetism. At first, when the current ceased for a moment, the child sank into a profound sleep, and there was no marked amendment until the means had been continued for three hours; and four hours and three quarters had passed before

* Ueber die Anstuckung und Verbreitung der Scrofelkrankheit bei Menschen durch den Genuss der Kuhmilch. Leipsig, 1846; and an abstract of it in J. f. Kinderkr., June, 1846.

† Journal de Médecine, August, 1845; and J. f. Kinderkr., March, 1846.

‡ Formulaire Thérapeutique, et Matière Médicale, concernant les Maladies de l'Enfance. 12mo, Paris, 1846.

§ J. f. Kinderkr., Dec., 1845.

|| Northern Journal, June, 1846.

it was thought prudent to discontinue their use. The child, however, then recovered without any further head symptoms.

3. DISEASES OF EARLY INFANCY.

Asphyxia Neonatorum. M. Depaul* has written a very elaborate paper on the subject of artificial respiration, as a means of resuscitating still-born children. He instituted a series of experiments on the dead subject, with the view of determining the amount of danger of injuring the lungs by the insufflation of air. He satisfied himself that this danger is almost an imaginary one, since, even after the lungs were removed from the body, it required several most forcible insufflations, far stronger than would ever be made in the case of a still-born child, to produce rupture of the pulmonary vesicles. On the other hand, he was struck with the great force needed thoroughly to inflate the lungs, while their resiliency was sufficient to expel the greater part of the air. He found, moreover, in many cases where children had died suddenly after breathing for several hours or days, no other morbid appearance than an unexpanded condition of a large portion of the lungs. With reference to the mode of practising artificial respiration, he condemns the mere blowing into the mouth as inadequate, and recommends the use of a tracheal tube. He is of opinion that there is more danger of failing from imperfect insufflation than of doing harm by its too forcible performance. It is of importance, likewise, that it should not be suspended on the first sign of breathing, but continued until the child cries loudly, and respires well.

Dr. Götz† relates a case of the *spontaneous fracture of the left parietal bone* during a natural but tedious labour, in which the head was five hours in the pelvic cavity, although the pelvis was well formed. There were three fissures in the bone; one running into the sagittal suture, one to the anterior inferior angle, and the other to the middle of the anterior edge of the bone. The child was still-born; much blood was effused under the scalp, but none into the skull.

Mr. Close‡ quotes from a manuscript of Dr. W. Hunter's, which he says was published fifteen years ago, an account of *cephalæmatoma*, so complete as to leave nothing to be added to it. Dr. Hunter notices the deceptive sensation of the bone being perforated. He cautions, likewise, against the needless cruelty of opening the tumour, which he says he leaves alone, and it disappears of itself.

Trismus. Dr. Sims§ calls attention to the intense vascularity of the vessels of the spinal cord, and the effusion of blood around it, often met with in cases of trismus, and observed by him in the instance of which he has recorded the dissection. He endeavours to account for the spinal apoplexy, by assuming that the new-born child lying on its back, with a hard support beneath its yielding skull, the edge of the occipital bone is driven up under the parietal bones, and the cerebral circulation is thus interrupted. [It is scarcely necessary to remark, that any such mechanical theory of the disease is contradicted by the remarkable influence of climate in the production of the disease, and by such facts as its extreme frequency at one time in the Dublin Lying-in Hospital, and its almost complete disappearance after an efficient system of ventilation had been adopted.]

Spina bifida. Dr. L. de Thinnecour|| relates the history of the successful treatment of a case of this affection in a child aged two months, in which a tumour of the size of an infant's head grew from the junction of the lumbar and sacral vertebræ. Instead of employing a common circular ligature, he used an instrument that acted on the principle of Dupuytren's *enterotome*, then punctured the sac, and afterwards laid it open through its whole extent. In ten days the communication with the vertebral column was closed, and he

* Journal de Chirurgie, May, June, 1845; and J. f. Kinderkr., März, 1846.
† Oesterr. Med. Jahrb., April, 1846.
‡ Med. Times, Sept. 20, 1846.
§ American Journal of Med. Science, April, 1846.
|| J. f. Kinderkr., May, June, 1846.

now removed the instrument, and placed a common circular ligature round the pedicle. The wound granulated and healed kindly, and at nine months old the child was perfectly well, and had had no return of the disease. The advantages of this mode of operating he conceives to be, that the membranes of the spinal cord are secured from the access of air after the puncture, that the two rods on each side of the spine form a substitute for the arches of the vertebræ, and favour the union of the serous surfaces, while they also help to diminish the space between the arches themselves. Dr. Beaunier† employed the common ligature in a child ten days old, with a spina bifida proceeding from the third cervical vertebra. Having tied it, he punctured it twice. Having tightened the ligature after the second puncture, the cyst began to become gangrenous, whereupon he cut it off. The wound healed, and four months afterwards the child was well.

Dr. Williamson‡ has described a very interesting case of *imperforate anus,* in which the rectum terminated in the urethra, the child, nevertheless, living for 8 months, and for 5 months of this time passing its fæces with moderate ease by the urethra. About that time, however, the child began to take other food besides the mother's milk, its fæces became more solid, and escaped with greater difficulty. Attacks of constipation of increasing severity now began to recur frequently, and in one of these the child died. The communication was found to exist at the membranous portion of the urethra, by means of a canal a quarter of an inch in diameter, and half an inch long, while the gut terminated in a blind pouch. Dr. Williamson made an unsuccessful attempt during the life of the child to open the rectum, but was not allowed to repeat it. He adds a caution against carrying the trocar too far backwards in operating in cases of this kind, since by so doing, the instrument may pass behind the rectum instead of puncturing it.

Dr. Thore's‡ observations on *peritonitis* in new-born children contain much valuable information. The general characters of the disease appear to be the same as it presents in the fœtus, and the same absence of puriform fluid in the abdominal cavity is noticed here. A dirty serous fluid, with fibrinous flocculi floating in it, is often observed, and layers of pale membrane cover the intestines, and are especially abundant about the spleen and liver. In one third of the cases, 63 in number, pleurisy was found associated with the peritonitis, another point in which it resembles the disease as it occurs in the fœtus. It appears, moreover, that this affection is most frequent during the first fortnight of existence, while after the first month it is very rare; a circumstance which suggests the doubt whether it does not, in some instances, commence even before birth. In several cases, however, this certainly was not so, for M. Thore found that the season of the year had much to do with the prevalence of the disease, and that it is most frequent during the spring and summer season, partly, perhaps, owing to the wards of the hospital being then most crowded. He inclines also to the opinion that those conditions which favour puerperal fever likewise increase the frequency of peritonitis in the infant; and he establishes, conclusively, the existence of a relation between infantile erysipelas and peritonitis, since 17 of 26 cases of erysipelas were combined with peritoneal inflammation, and a similar relation may be noticed between peritonitis and phlebitis of the umbilical vein.

A sudden, tympanitic swelling of the abdomen is often the first symptom of the disease, and is soon associated with vomiting of a greenish matter, which phenomenon, however, is seldom of long continuance. The bowels are generally constipated throughout, the respiration and pulse become accelerated, the heat of the skin is increased, and the child evidently suffers pain in the abdomen. As the disease advances the countenance alters, the skin grows cold, and the pulse feeble, the child dying, in the great majority of cases, in

* J. f. Kinderkr., July, 1846. † Med. Gazette, May 1, 8, 1846.
‡ Arch. Gen. de Méd., Aug. and Sept., 1846.

less than twenty-four hours, and no instance having ever come under his notice where the symptoms ran a chronic course. All the cases that M. Thore saw terminated fatally, his recommendation, therefore, of depletion and calomel rests on theoretical grounds, not on any well-ascertained beneficial results of their employment.

4. DISEASES OF SUBSEQUENT CHILDHOOD.
DISEASES OF THE BRAIN, NERVOUS SYSTEM, ETC.

An interesting case of the occurrence of *apoplexy,* in a healthy boy, aged 11 years, is recorded by Mr. Worthington.* The right ventricle was full of extravasated blood, and the brain around lacerated to a considerable extent, but no ruptured vessel was distinguishable. When first seen after the attack, the child was extremely faint and exhausted, but paralysis was not present, though the right pupil was extremely dilated, and the left much contracted. General convulsions occurred every ten minutes for the last ten hours of the child's life, and he died in one of these fits, about twelve hours after the seizure.

Inflammatory affections. A work has been written by Dr. d'Alnoncourt,† addressed to the public at least as much as to the profession, in which he combats the generally-received opinion of the frequency of inflammatory affections of the brain, especially at the time of dentition, and refers almost all the diseases of childhood to impairment of nutrition and disorder of the digestive organs. The book abounds in exaggerated statements, and declamations against physicians, but contains no new facts, and no record of observations.

Dr. Mayne‡ has given a brief sketch of an epidemic of *cerebro-spinal arachnitis,* which has recently prevailed in the Irish workhouses, and in some of the Dublin hospitals. It resembled, both in its symptoms and in the morbid appearances to which it gave rise, the affection that was epidemic in many parts of France, from the year 1840 to 1842. The arachnoid was found in every case extensively inflamed, and lymph was poured out beneath it; the arachnoid of the spinal cord being in every instance much more severely affected than that of the brain, while the nervous substance was comparatively seldom involved, and never in any considerable degree. In France the young conscripts were most frequently attacked, and in Ireland its most frequent subjects were boys under 12 years of age, while there, as on the continent, females very seldom suffered from it. Its attack was generally very sudden, and its course often extremely rapid, some patients dying in twenty-four hours, while few survived the 4th day. Severe pain in the abdomen, attended with vomiting and purging, and a condition of general collapse, marked the outset of the disease. A stage of reaction followed this in a few hours, the surface becoming hot, the pulse full, and its frequency varying from 120 to 140, while the face assumed a tetanic expression, and the head was retracted and firmly fixed. General convulsions or coma succeeded to this condition, and failure of deglutition, and a slow and laboured pulse soon followed as the immediate precursors of death. Dr. Mayne does not state the exact number of cases that came under his care, but he mentions that he, in common with other physicians, often found treatment of no avail, though he seems to have employed depletion and mercurials with great decision in those cases which he saw early.

Acute hydrocephalus. In a paper on the premonitory symptoms of this disease, M. Rilliet§ expresses his dissent from the opinion of those writers who have attributed them either to the occurrence of small effusions of fluid into the ventricles, or to a congested state of the brain, or to a chronic meningitis ushering in the more acute affection. He regards the so-called premoni-

* Prov. Med. Journal, April 22, 1846.

† Die Gehirnaffectionen der Kindern in der Dentitionsperiode, &c., eine Täuschung der Aerzte. 8vo, Leipsic, 1846.

‡ Dublin Quarterly Journal, Aug., 1846.

§ Gaz. Méd., Jan. 1846.

tory symptoms of the disease as the index of the general tuberculization, rather as the sign of the local deposit of tubercle in the membranes of the brain. The child shows signs of ill health when the lungs and bronchial glands are becoming the seat of the morbid deposit, and it does not always happen that these premonitory signs are followed by the outbreak of the acute disease, for sometimes they pass into confirmed phthisis, while in other instances they altogether disappear under appropriate treatment. He states, moreover, that the general deposit of tubercle is more abundant, and it is found to have reached a more advanced stage directly in proportion to the longer duration of the premonitory symptoms, while, on the other hand, their duration and that of the meningitis are in an inverse ratio to each other.

A series of articles have appeared from the pen of Dr. Bierbaum,* on the diagnostic value of the different symptoms of acute hydrocephalus. They are by no means destitute of merit, though far inferior to the pamphlet on the same subject, published some years ago by Dr. Wolff, of Bonn.

Chronic hydrocephalus. Dr. Spengler† has published an analysis of the fluid from a chronic hydrocephalus, from which he concludes that at any rate, in this instance, it was not the product of inflammatory action, but was the result of the morbid accumulation of the cerebro-spinal fluid.

Dr. Edwards‡ mentions the case of a child who recovered from chronic hydrocephalus after a single tapping. This proceeding was adopted when the child was 14 months old, after six months of unsuccessful treatment. Eight ounces of a rose-coloured fluid were drawn off, and the child recovered, and continued well at the end of seven years. Dr. Götz and Mr. Chater§ have punctured the brain unsuccessfully. Both children were 5 months old, and in both the disease was congenital. In the former case three punctures were made within eighteen days; temporary amendment succeeding to each, but the fluid collected again rapidly, and death in convulsions took place five days after the last puncture. Mr. Chater modified the operation by introducing two or three threads into the puncture in order to act as a syphon, and thus keep up a constant drain from the brain. By this plan the head was reduced 5½ inches in circumference in five weeks; the child then suddenly grew comatose and died. The brain was in this case so extensively disorganized, as to have rendered cure quite impossible, but the practice of Mr. Chater deserves another trial. [There are now 63 authenticated cases on record in which puncture of the brain was performed; and in 18, or 28·5 per cent. of these the child recovered.]

DISEASES OF THE ORGANS OF RESPIRATION.

A paper has been published by Dr. G. A. Rees,‖ 'On Carnification of the Lungs in Infants,' which might be dismissed without further notice, if it were not that in it he asserts a claim to have been the first who described this condition, and insinuates a charge of plagiarism against MM. Rilliet and Barthez, who, he says, in their work on Chidren's Diseases almost quote his own words. [The title of the paper on which Dr. Rees founds his claim, is 'On deformity of the chest in young children from disease of the lungs;' and it was published in the 'Medical Gazette' for January 12, 1839. It is fortunately unnecessary to analyse this paper in order to vindicate MM. Rilliet and Barthez from this singular accusation, since the article pneumonia in their large work on Children's Diseases, is little else than a reprint of their monograph on that subject which appeared in 1838.¶ In that monograph will be found the particulars of 11 observations of the condition which they designated as carnification of the lungs, as well as the very words between

* J. f. Kinderkr., July, Aug., Sept., 1846. † Oesterr. Med. Wochenschr., July 12, 1845.

‡ Monthly Journal, June, 1846.

§ Oesterr. Med. Jahrb., June, 1846; Prov. Med. Journal, Oct. 1, 1845. ‖ Lancet, July 11, 1846.

¶ The title of this work is, Maladies des Enfans. Affections de Poitrine. Première Partie—Pneumonie. Par MM. Rilliet et Barthez. 8vo, Paris, 1838.

which and his own expressions, published some months afterwards, Dr. Rees professes to discover so remarkable a coincidence.]

DISEASES OF THE ORGANS OF DIGESTION, AND ASSIMILATION, AND THEIR APPENDAGES.

Dr. Panck* has described an epidemic of *cynanche parotidea*, that prevailed in the Alexandrine Orphan House at Moscow in the commencement of the year 1840, and affected 162 out of 300 inmates between the beginning of January and the beginning of April. Both sexes appeared to be equally liable to the disease, but young persons about the age of puberty were most frequently attacked by it, while children under 7 years old had simple febrile seizures, without any swelling of the glands. It was quite evident that some epidemic influence was concerned in its production, and that it did not arise simply from cold; neither did cold appear to be the principal cause of metastasis of the swelling from the parotids to other parts, though it did sometimes seem to cause a relapse in patients who had appeared convalescent.

Dr. Duncan† relates several cases of *ulcerous stomatitis*, connected with diarrhea and entero-colitis, which he observed among the children of the South Dublin Workhouse. He gives a brief but good account of the disease, and dwells at some length on the evidence of its not being dependent on the administration of mercurials, though it would seem, from his remarks, that he regards it as identical with cancrum oris, an opinion to which the writer of this Report cannot by any means subscribe.

A very interesting account has been given by Dr. Daviot of an epidemic of *diphtheritis*, which prevailed in the department of the Saône and Loire from 1841 to 1844.‡

He states that the disease never occurred sporadically in this district, and that no case of it had been observed since 1809; though between 1782 and 1809, it had been four times epidemic.

The pharynx was the part most frequently affected by it, and next to that the skin; then the mucous membrane of the trachea and larynx, and lastly, that of the mouth; but it frequently attacked many parts simultaneously. The affection of the skin often came on with intense redness, followed by ulceration; or the skin appeared excoriated, and these excoriations then became covered with lymph. In other instances, an eruption resembling scarlatina appeared over the whole surface, although not followed by any diphtheritic affection of the skin.

The course of the disease was very rapid. It reached a high degree of severity in from 36 to 48 hours, and its fatal termination took place in from 7 to 10 days; the patients dying asphyxiated. Sometimes, after apparent recovery, bronchiopneumonia supervened, and led to a fatal result.

Dr. Daviot bled both generally and locally at the onset of the disease. In the first stage he employed alum locally, but as the disease advanced, he had recourse to the nitrate of silver, believing it to be more energetic than hydrochloric acid. Rubefacients were frequently applied to the throat with advantage, but blisters were avoided, on account of the tendency to diphtheritic affection of the skin.

Diarrhea. The work of M. Legendre§ contains an essay on this subject, founded on the observation of 28 fatal cases. He ascertained that in some instances no alteration whatever of the intestinal mucous membrane could be found, while in the majority of cases enlargement of the intestinal follicles, or their more or less extensive ulceration, constituted all the morbid appearances. These changes of the follicles, too, appear always to precede any alteration of the mucous membrane itself. From these facts he concludes

* Zeitschr. f. d. gesammte Medicin, Sept., 1845. † Dublin Journal., Sept., 1845.

‡ The writer has been unable to obtain this pamphlet, which was published at Autun, in 1845; but an abstract of it is given in the J. f. Kinderkr., June, 1846. § Op. cit., pp. 363-418.

that the diarrhea of early childhood is at first merely an excessive secretion, and not the result of any appreciable morbid change, and that the anatomical alterations of the digestive canal are the consequences of the diarrhea, not its cause, whence it happens that their extent is usually proportionate to the severity and continuance of the flux. The grand object of this essay is to combat the opinions of the disciples of Broussais, and by disproving the inflammatory origin of diarrhea, to explain the frequent failure of antiphlogistic treatment, and to justify a return to the practice of the old physicians, and the use of evacuants and absorbents.

Dr. G. Bird* has made an *analysis of the green evacuations of children.* He finds that they give no indication of containing an excess of bile, as has generally been supposed, but conceives that their colour is due to the presence of altered blood, and that the state in which they are produced is analogous to that which exists in melæna, the portal system generally being in a congested state. In support of his views, he mentions that stools originally of a yellow or orange colour, often turn green under exposure to air, an occurrence very unlike what takes place with matters containing bile, though it is an ascertained fact that blood under the influence of some oxidating agents acquires a green colour.

Two cases of fatal *intussusception of the intestines* are related; the one by Dr. Boyer, the other by Mr. Markwick,† in each of which the characteristic symptoms were present.

Dr. Duclos‡ relates two cases of *fissure of the anus* in infants, treated by M. Trousseau. The symptoms seem to be the same as in the adult, namely, pain and spasmodic contraction of the sphincter, betokened by a cry when the bowels act, and the escape of a drop or two of blood. M. Trousseau treats these cases successfully with enemata of one part of extract of krameria to 100 of water.

M. Morand§ recommends the extract of belladonna as a valuable remedy for *nocturnal incontinence of urine,* in those cases of it which seem to be associated with a state of general debility. For children from 4 to 6 years old, he begins with a pill containing gr. $\frac{1}{4}$ of the extract twice a day, increasing the dose to gr. j in the course of 14 days. He suspends the medicine if symptoms of narcotization come on, but otherwise continues it for 2 or 3 months so as to effect a perfect cure.

FEVERS.

Ague. Dr. Petzold,|| who inhabits a malarious district, describes the peculiarities which this disease presents in early childhood. Its characters are on the whole less marked, so that there is some danger of mistaking the nature of the affection. The shivering fit is less severe, and neither the hot nor the sweating stage is so well marked as in the adult. The intermissions, likewise, are less complete, the child being manifestly out of health between the paroxysms, while there is often a very great tendency in the attack to anticipate, so that the periodicity of its return may easily be lost sight of.

When it occurs in children of only a few months old, the cold stage usually sets in very suddenly, attended with great depression, or sometimes it comes on with convulsions, and manifest cerebral disturbance, so that when the hot stage has succeeded, the case may be taken for one of inflammation of the brain. The tranquil sleep, however, into which the child falls as the hot stage passes off, will serve to guard from this error. It is of importance to recognize the disease early in the infant, as well as in the aged, since the attacks of ague exhaust the strength very rapidly, while quinine, which is of the most marked benefit while the disease retains the intermittent type, seems to lose much of its efficacy so soon as the fever has assumed the continued form.

* Med. Gaz., Sept. 5, 1845. † Casper's Wochenschr., March 10, 1846; Lancet, July 18, 1846.
‡ J. f. Kinderkr. June, 1846. § Ibid., Sept. 1845. || Ibid., Sept. 1845.

Typhoid Fever. Dr. Löschner,* physician to the Children's Hospital at Prague, has written a paper on this subject, the materials for which are drawn from the observation of 104 cases that came under his notice among a total of 6500 children, at a time when fever was not epidemic. He ascertained the ages from 5 to 9 to be those during which the disease is most prevalent, while it attacks boys more frequently than girls. The mortality among the cases that he observed was 8 in 104. He notices a greatly enlarged and highly injected state of the mesenteric glands as having been much more constant than ulceration of Peyer's glands; and on this tendency to affection of the glands, he builds the hypothesis that what is called typhoid fever, is a kind of acute scrofula, though he adduces no other fact in support of this theory.

Measles. An epidemic of this disease that prevailed in the neighbourhood of Glogau, in the spring of 1843, is described by Dr. Posner.† The disease did not cause any remarkable mortality, and presented nothing unusual in its course.

Dr. Battersby‡ has related some very interesting cases illustrative of the complications and sequelæ of measles, as he observed them during an epidemic in the autumn of 1844. The affection presented much of an asthenic character, and was often associated with diarrhea and dysentery, and with inflammation of the mouth and pharynx. In two or three cases also, where the powers of life were much exhausted, sloughing of the cornea took place. Unlike the affection of the eyes which comes on in phlebitis, it was attended with but very little increase of vascularity; [and it seems questionable whether it was not due rather to the general impairment of nutrition than to any specific influence of the poison of measles.]

Scarlatina. The pamphlet of Mr. J. B. Brown§ on this subject has been so generally noticed in the various medical journals, as to render any further mention of it in this Report unnecessary.

Dr. Merbach‖ has described the *dropsy that succeeded to scarlatina,* in an epidemic at Dresden, and which sequela appears to have been extremely fatal, causing the death of nearly 1 in every 3 who suffered from it. The treatment adopted, which consisted chiefly in the administration of stimulant diuretics, with the neglect of depletion and of all decided antiphlogistic means, will probably in some measure account for this mortality. Dr. Merbach confirms the statements of previous observers with reference to the characters of the urine, and the fluctuations in the quantity of albumen it contained without any apparent cause. He notices, moreover, that the diminution in the quantity of urea was always in direct proportion to the abundance of the albumen, but that the increase in the quantity of the former always took place more slowly than the diminution of the latter. The work of M. Legendre¶ contains a valuable essay on the *anasarca and the œdema of the lung* which occasionally succeed to scarlatina. He first notices the frequency with which the eruption of the scarlet fever is overlooked, in consequence of its being but very temporary, and insists on the importance of making very minute inquiries and examining the surface very carefully in all the febrile affections of childhood, in order to ascertain whether the rash is or has been present. He next lays down rules for the hygienic management of children during their convalescence from scarlatina. Lastly, he inquires into the nature and causes of the dropsy, which he regards as the simple result of the action of cold, and not as the consequence of renal disease; the albuminous state of the urine being in his opinion produced by a simple nephritis or even by a congested state of the kidney, and not the token of an incipient stage of Bright's disease. In support of this opinion he appeals to

* J. f. Kinderkr., Dec. 1845. † Ibid., Sept. 1846. ‡ Dublin Journal, Sept. 1845.

§ On Scarlatina and its successful Treatment by the Acidum Aceticum Dilutum of the Pharmacopœia. 8vo, London, 1846. ‖ J. f. Kinderkr., May, 1846. ¶ Op. cit., pp. 305-362.

the connexion between the quantity of albumen and that of blood in the urine, and to the simultaneous diminution in the two as the patient approaches towards convalescence. The number of cases on which M. Legendre's remarks are based was only 14; and he does not seem to have had the opportunity of watching any patients who died after the disease had reached a chronic stage, so as to determine whether any tendency to granular degeneration of the kidney is induced by the previous scarlatinal dropsy. [The correctness of his views, however, is borne out by the observations of cases where the dropsy has existed unconnected with albuminous urine, as in the epidemic at Berlin, in the spring of 1840,* as well as by the results of recent microscopic investigations, such as those of Henle, Eichholtz, and Dr. G. Johnson.]

The concluding part of his essay gives an account of that œdema of the lung which comes on as a sequela of scarlatina, for the most part in cases where general anasarca is present or has previously existed. He describes the sudden manner in which its symptoms often appear, while though the dyspnœa that attends it is extremely urgent, there are no auscultatory signs of the affection of the lungs. The chief point in the paper, however, is the anatomical description of this condition, which is an œdema of the interlobular celluar tissue, compressing the air-cells in greater or less degree, and thus differing from the œdema of Laennec, in which the fluid is supposed by that author to be contained within the pulmonary vesicles.

Variola. An elaborate paper on the *anatomy of the smallpox pustule* has been written by Dr. Simon,† of which only brief mention can be made here. He states that the pustule does not always owe its central depression to the presence of a hair-follicle pinning down the epidermis, but that in parts where no hair-follicle exists, the appearance is probably owing to the rapid desiccation of the exudation first poured out, while fresh matter is afterwards effused around it. The white membraniform layer immediately below the surface of the pustule is not in reality a false membrane, but is chiefly made up of the lower desintegrated stratum of epidermis, and the cellular structure of the pustule is produced by this stratum remaining in connexion with the cutis at some points, while at others it is detached from it.

The utility of the application of *mercurial ointment* or plaster, as a means of producing the *abortion of the smallpox pustule*, and thus preventing pitting, and diminishing the danger of the disease, is confirmed by the experience of MM. Goblin, Charcellay, and Briquet,‡ and M. Thielmann and Dr. Panck§ state that they have obtained equally favorable results from the frequent use of a solution of the corrosive sublimate. M. Thielmann employed it of the strength of gr.j to ℥ij. Dr. Panck used it of about half that strength.

M. Tardieu‖ has related a case of the *simultaneous existence of variola and vaccinia* in a man aged 18, who was vaccinated on the day on which the eruption of smallpox had made its appearance. The variola ran its course with its characters modified, and after the desquamation of its pustules an irregular eruption of cowpox appeared. From this case he concludes that we may vaccinate with the hope of doing good, not merely during the preliminary fever of variola, but even after the outbreak of the eruption.

The work of M. Steinbrenner,¶ who, with M. Bousquet and M. Fiard, has shared the prize of the French Academy for the best essay on *vaccination* and *its influence on smallpox*, will be found to contain a great amount of information on the subject, collected with the most laborious industry. In reply to the five questions proposed by the Institute, M. Steinbrenner decides—

* Described by Dr. Philip, in Casper's Wochenschr., Aug. 29, 1840.

† Müller's Archiv, 1846, ii.

‡ Revue Méd., and Oesterr. Med. Wochenschr., September 20, 1845; Bull. de l'Acad. Roy. de Méd., April 15, 1846; Gazette des Hôpitaux, September 19, 1846.

§ Gazette des Hôpitaux, April 16, 1846; Oesterr. Med. Wochenschr., September 20, 1845.

‖ Gaz. Med., November, 1845.

¶ Traité sur la Vaccine. 8vo, Paris, 1846.

1st. That the preservative power of vaccination is almost always permanent, and that when it is not so the period of immunity varies greatly according to individual peculiarities. 2d. That the vaccine virus does undergo a positive deterioration by transmission through successive individuals. 3d. It is therefore desirable to obtain fresh lymph frequently, which might be done by taking it annually from the cow, by which we should be much more sure of succeeding than by retrovaccination or any similar means. 4th. There is no necessary connexion between the intensity of the local phenomena of vaccination and its preservative power, but there is such a relation between the preservative power and the amount of constitutional disturbance. 5th. Revaccination is desirable not because it is always necessary, but because we have no means of distinguishing the cases where it is needed from those in which it is superfluous.

A very elaborate collection of statistics, intended to illustrate the same questions as are treated of by Dr. Steinbrenner, has been made by Dr. Lane,* but is not of a kind to admit of abstract.

In an account of an *epidemic of smallpox at Heidelberg*, and of revaccinations which he practised there, Dr. Hoefle† makes an assertion which is opposed to general experience. He asserts that he found the pustules of revaccination bear to those of primary vaccination just the same relation as those of a second attack of variola bear to those of a first attack. He states, moreover, that he observed this modification, although he never employed revaccine lymph, and though he always vaccinated directly from arm to arm.

M. Legendre‡ has related the particulars of some *chronic affections of the skin* which were *cured by* the appearance of the *eruption of smallpox*. The cases which underwent improvement were either papular, vesicular, or pustular, while an eruption of porrigo favosa of the scalp was not in the least benefited by a copious eruption of smallpox.

DYSCRASIÆ, ETC.

Gangrene. Dr. Battersby§ describes a case of gangrene of the skin in a female child, aged 10 months. The disease began with the appearance on the limbs of several vesicles, a good deal like those of varicella, but larger, the cutis beneath some of them being black and gangrenous. The child lived for a fortnight, during which time no attempt took place at separation of the dead parts, and the gangrene extended from the thigh to the vulva, and partly up the abdomen. Dr. Battersby mentions a similar case recorded by Dr. Hutton in 'Dublin Journal,' xvii. p. 485. [A case is mentioned by Rilliet and Barthez, 'Maladies des Enfans,' ii. p. 195; references to others are given by Richter, 'Ueber den Brand der Kinder,' pp. 9-12; and one instance of it came under the notice of the writer of this Report, in which the skin of the face was affected.]

Scrofula. Mr. Phillips's Treatise on Scrofula ‖ contains a very large amount of valuable statistical information. The grand objects of the work, however, is to prove the non-identity of phthisis and scrofula. He confesses the apparent identity of the deposit, however tested; but in scrofula inflammatory change in the gland precedes the deposit; while the lung is unaltered around a simple deposit of tubercle. Further, the two diseases are not prevalent in the same districts, nor in the same sex, nor at the same age, and 18 out of 20 phthisical patients show no sign of scrofula; from all which facts taken together Mr. Phillips draws the conclusion that though allied they are not identical diseases.

* American Journal of Med. Sci., July, 1846.
† Gaz. Méd., April 25, 1846.
‡ Op. cit., p. 439-449.
§ Dublin Hospital Gazette, March 15, 1846.
‖ Scrofula,—its Nature, its Causes, its Prevalence, and the Principles of Treatment. 8vo, London, 1846.

ON A NEW MEANS OF RENDERING SURGICAL OPERATIONS PAINLESS.

Just as our last proof was passing through our hands, we received from our medical friends in Boston the account of a matter so interesting to surgeons, and indeed to every one, that we take the opportunity of introducing it here. We know nothing more of this new method of eschewing pain than what is contained in the following extracts from two private letters, kindly written to us by our excellent friends, Dr. Ware and Dr. Warren, of Boston—both men of the highest eminence in their profession in America—and, we may truly say, in Europe also. It is impossible however, not to regard the discovery as one of the very highest importance, not in the practice of operative surgery only, but also, as Dr. Ware suggests, in practical medicine also. We trust our friends will forgive us for putting into print their private communications. The importance of the subject and the necessity of authenticating the statements, are our excuses. The authors of the discovery are Dr. C. T. Jackson and Dr. Morton.

"Boston, November 29, 1846.

"I found, on my arrival here, a new thing in the medical world, or rather the new application of an old thing, of which I think you will like to hear. It is a mode of rendering patients insensible to the pain of surgical operations, by the inhalation of the vapour of the strongest sulphuric ether. They are thrown into a state nearly resembling that of complete intoxication from ardent spirits or of narcotism from opium. This state continues but a few minutes—five to ten—but, during it, the patient is insensible to pain. A thigh has been amputated, a breast extirpated, teeth drawn, without the slightest suffering. The number of operations of various kinds, especially those in dentistry, has been very considerable, and I believe but few persons resist the influence of the agent.

"The effect is not exactly the same on all. In some, the insensibility is entire, and the patient is aware of nothing which is going on; in others, a certain degree of the power of perception remains, the patient knows what the operator is doing, perceives him for example, take hold of a tooth and draw it out, feels the grating of the instrument, but still has no pain.

"There are no subsequent ill effects to detract from the value of this practice, none even so great as those which follow a common dose of opium. One person told me she had some unpleasant sensations in the head for a short time, and was weak, languid, and faintish through the day, but not more so than she ordinarily was from having a tooth drawn. Another told me that he experienced something of the same kind and in addition that his breath smelt very strongly of *ether* for forty-eight hours, and was indeed so strongly impregnated with it as to affect the air of the room in which he sat, so as to be disagreeable to others.

"One of our best operative surgeons informs me that he regards it as chiefly applicable to cases of the large and painful operations which are performed rapidly, and do not require any very nice dissection, but that for the more delicate operations, which require some time, he would prefer to have the patient in his usual state. But it is impossible at present to judge what will be the limits to the application of such an agent. Objections may arise of which we do not dream, and evils may be found to follow, which we do not now perceive. Still it certainly promises much in surgery, and perhaps may be capable of application for other purposes beside the alleviation of pain. Would it not be worthy of trial in tetanus, in asthma, and in various cases of violent internal pain, especially from supposed spasms?

"It was brought into use by a dentist, and is now chiefly employed by that class of practitioners. He has taken out a patent for the discovery, and has despatched persons to Europe to secure one there also; so you will soon hear of it, and probably have an opportunity of witnessing its effects. Faithfully yours, John Ware."

"Boston, November 24th, 1846.

"You may have heard of the respiration of ether to prevent pain in surgical operations. In six cases I have had it applied with satisfactory success and no unpleasant sequel. "I remain, &c., John C. Warren."

Since the above was in type we have seen a more extended communication on the same subject, published by Dr. Bigelow in the 'Boston Medical and Surgical Journal.' The more material parts of this we hasten to extract:

"It remains briefly to describe the process of inhalation by the new method, and to state some of its effects. A small two-necked glass globe contains the prepared vapour, with sponges to enlarge the evaporating surface. One aperture admits the air to the interior of the globe, whence, charged with vapour, it is drawn through the second into the lungs. The inspired air thus passes through the bottle, but the expiration is diverted by a valve in the mouth-piece, and escaping into the apartment, is thus prevented from vitiating the medicated vapour.

"A boy of 16, of medium stature and strength, was seated in the chair. The first few inhalations occasioned a quick cough, which afterwards subsided; at the end of eight minutes the head fell back, and the arms dropped, but owing to some resistance in opening the mouth, the tooth could not be reached before he awoke. He again inhaled for two minutes, and slept three minutes, during which time the tooth, an inferior molar, was extracted. At the moment of extraction the features assumed an expression of pain, and the hand was raised. Upon coming to himself he said he had had a 'first-rate dream—very quiet,—'and had dreamed of Napoleon—had not the slightest consciousness of pain—the time had seemed long;' and he left the chair feeling no uneasiness of any kind, and evidently in a high state of admiration. The pupils were dilated during the state of unconsciousness, and the pulse rose from 130 to 142.

"A girl of 16 immediately occupied the chair. After coughing a little, she inhaled during three minutes, and fell asleep, when a molar tooth was extracted, after which she continued to slumber tranquilly during three minutes more. At the moment when force was applied she flinched and frowned, raising her hand to her mouth, but she said she had been dreaming a pleasant dream, and knew nothing of the operation.

"A stout boy of 12 at the first inspiration coughed considerably, and required a good deal of encouragement to induce him to go on. At the end of three minutes from the first fair inhalation, the muscles were relaxed and the pupil dilated. During the attempt to force open the mouth he recovered his consciousness, and again inhaled during two minutes, and in the ensuing one minute two teeth were extracted, the patient seeming somewhat conscious, but upon actually awaking, he declared 'it was the best fun he ever saw,' avowed his intention to come there again, and insisted upon having another tooth extracted upon the spot. A splinter which had been left afforded an opportunity of complying with his wish, but the pain proved to be considerable. Pulse at first 110, during sleep 96, afterward 144, pupils dilated.

"These cases, which occurred successively in about an hour, at the room of Dr. Morton, are fair examples of the average results produced by the inhalation of the vapour, and will convey an idea of the feelings and expressions of many of the patients subjected to the process.

"The inhalation, after the first irritation has subsided, is easy, and produces a complete unconsciousness at the expiration of a period varying from two to five or six, sometimes eight minutes; its duration varying from two to five minutes, during which the patient is completely insensible to the ordinary tests of pain. The pupils, in the cases I have observed, have been generally dilated; but with allowance for excitement and other disturbing influences, the pulse is not affected, at least in frequency. The patient remains in a calm and tranquil slumber, and wakes with a pleasurable feeling.

"It is natural to inquire whether no accidents have attended the employment of a method so wide in its application, and so striking in its results. I have been unable to learn that any serious consequences have ensued. One or two robust patients have failed to be affected. I may mention, as an early and unsuccessful case, its administration in an operation performed by Dr. Hayward, where an elderly woman was mdae to inhale the vapour for at least half an hour without

effect. Though I was unable at the time to detect any imperfection in the process, I am inclined to think that such existed. One woman became much excited, and required to be confined to the chair. As this occurred to the same patient twice, and in no other case, as far as I have been able to learn, it was evidently owing to a peculiar susceptibility. Very young subjects are affected with nausea and vomiting, and for this reason Dr. Morton has refused to administer it to children. Finally, in a few cases, the patient has continued to sleep tranquilly for eight or ten minutes, and once, after a protracted inhalation, for the period of an hour.

"The following case, which occurred a few days since, will illustrate the probable character of future accidents. A young man was made to inhale the vapour while an operation of limited extent, but somewhat protracted duration, was performed by Dr. Dix upon the tissues near the eye. After a good deal of coughing, the patient succeeded in inhaling the vapour, and fell asleep at the end of about ten minutes. During the succeeding two minutes the first incision was made, and the patient awoke, but unconscious of pain. Desiring to be again inebriated, the tube was placed in his mouth, and retained there about twenty-five minutes. the patient being apparently half affected, but, as he subsequently stated, unconscious. Respiration was performed partly through the tube and partly with the mouth open. Thirty-five minutes had now elapsed, when I found the pulse suddenly diminishing in force, so much so that I suggested the propriety of desisting. The pulse continued decreasing in force, and from 120 had fallen to 96. The respiration was very slow, the hands cold, and the patient insensible. Attention was now, of course, directed to the return of respiration and circulation. Cold affusions, as directed for poisoning with alcohol, were applied to the head, the ears were syringed, and ammonia presented to the nostrils and administered internally. For fifteen minutes the symptoms remained stationary, when it was proposed to use active exercise, as in a state of narcotism from opium. Being lifted to his feet, the patient soon made an effort to move his limbs, and the pulse became more full, but again decreased in the sitting posture, and it was only after being compelled to walk during half an hour that the patient was able to lift his head. Complete consciousness returned only at the expiration of an hour. In this case the blood was flowing from the head, and rendered additional loss of blood unnecessary. Indeed the probable hemorrhage was previously relied on as salutary in its tendency.

"Two recent cases serve to confirm, and one I think to decide, the great utility of this process. On Saturday, the 7th November, at the Massachusetts General Hospital, the right leg of a young girl was amputated above the knee, by Dr. Hayward, for disease of this joint. Being made to inhale the preparation, after protesting her inability to do so from the pungency of the vapour, she became insensible in about five minutes. The last circumstance she was able to recal was the adjustment of the mouth-piece of the apparatus, after which she was unconscious until she heard some remark at the time of securing the vessels—one of the last steps of the operation. Of the incision she knew nothing, and was unable to say, upon my asking her, whether or not the limb had been removed. She refused to answer several questions during the operation, and was evidently completely insensible to pain or other external influences. This operation was followed by another, consisting of the removal of a part of the lower jaw by Dr. Warren. The patient was insensible to the pain of the first incision, though she recovered her consciousness in the course of a few minutes.

"I am as yet unable to generalize certain other symptoms to which I have directed attention. The pulse has been, as far as my observation extends, unaltered in frequency, though somewhat diminished in volume, but the excitement preceding an operation has, in almost every instance, so accelerated the pulse that it has continued rapid for a length of time. The pupils are in a majority of cases dilated; yet they are in certain cases unaltered, as in the above case of amputation.

"The duration of the insensibility is another important element in the process.

When the apparatus is withdrawn at the moment of unconsciousness, it continues upon the average two or three minutes, and the patient then recovers completely or incompletely, without subsequent ill effects.

"But if the respiration of the vapour be prolonged much beyond the first period, the symptoms are more permanent in their character. In one of the first cases, that of a young boy, the inhalation was continued during the greater part of ten minutes, and the subsequent narcotism and drowsiness lasted more than an hour. In a case alluded to before, the narcotism was complete more than twenty minutes; the insensibility approached to coma.

"The process is obviously adapted to operations which are brief in their duration, whatever be their severity. Of these the two most striking are, perhaps, amputations and the extraction of teeth. In protracted dissections, the pain of the first incision alone is of sufficient importance to induce its use; and it may hereafter produce a narcotism of an hour's duration. It is not unlikely to be applicable in cases requiring a suspension of muscular action; such as the reduction of dislocations or of strangulated hernia; and, finally, it may be employed in the alleviation of functional pain, of muscular spasm, as in cramp and colic, and as a sedative or narcotic."

P.S. Dec. 22.—Yesterday we had ourselves the satisfaction of seeing this new mode of cheating pain put in practice by a master of chirurgery on our own side of the Atlantic. In the theatre of University College Hospital, Mr. LISTON amputated the thigh of a man previously narcotised by inhalation of the ether vapour. Shortly after being placed on the operating table, the patient began to inhale, and became apparently insensible in the course of two or three minutes. The operation then commenced; and the limb was removed in what seemed to us a marvellously short space of time—certainly less than a minute; the patient remaining, during the incisions and the tying of the arteries, perfectly still and motionless. While the vessels were being secured, on being spoken to he roused partially up (still showing no signs of pain), and answered questions put to him in a slow, drowsy manner. He declared to us that at no part of the operation had he felt *pain*, though he seemed to be partially conscious; he heard some words, and felt that *something* was being done to his limb. He was not aware, till told, that the limb was off, and, when he knew it, expressed great gratification at having been saved from pain. The man seemed quite awake when removed from the operation-room, and continued so. Everything has since proceeded as usual, and very favorably.

Mr. LISTON afterwards performed one of the minor but most painful operations of surgery—the partial removal of the nail in *onychia*—on a man similarly narcotised, and with precisely the same result. The patient seemed to feel no pain, and, upon rousing up after the operation, declared that he had felt none.

In these cases the ether vapour was administered by means of an ingenious apparatus extemporaneously contrived by Mr. Squire, of Oxford street. It consisted of the *bottom part* of a Nooth's apparatus, having a glass funnel filled with sponge soaked in pure *washed* ether, in the upper orifice, and one of Read's flexible inhaling tubes in the lower. As the ether fell through the neck of the funnel, it became vaporized, and the vapour being heavy, descended to the bottom of the vase, and was thence inspired through the flexible tube. No heat was applied to the apparatus or the ether.

The momentous details given above suggest many remarks which we have no room to record. We are only able to observe that if the new process shall supersede that employed, with a like object, by the mesmerists, we must concede to them that it supplies, from analogy, additional reasons for believing in their statements in regard to the production by their process, of insensibility to pain. (See Art. X, in our last Number.) The readers of two articles in our last and present Numbers, will also observe the bearing of some of the present results on the semi-psychical discussions contained in them. J. F.

THE
BRITISH AND FOREIGN
MEDICAL REVIEW,
FOR APRIL, 1847.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *A Dictionary of Practical Medicine.* By JAMES COPLAND, M.D., F.R.S. Part X. Article "*Pestilence Choleric.*" —*London,* 1846.
2. *Notes on the Epidemic Cholera.* By R. H. KENNEDY, M.D., late Physician-General and President of the Medical Board, Bombay. Second edition.—*London,* 1846.
3. *Report on the Epidemic Cholera, as it has appeared amongst the Native Troops of the Madras Army on the Line of March from one Station to another, from* 1820 *to* 1844 *inclusive.* Drawn up by ALEXANDER LORIMER, M.D. Published by Order of Government.—*Madras,* 1846.
4. *Medical Notes and Reflections, &c.* By HENRY HOLLAND, M.D., F.R.S. Chapters, "*On the Method of Inquiry as to Contagion*" (pp. 276), and "*On the Hypothesis of Insect Life as a Cause of Disease*" (pp. 567). Second edition.—*London,* 1840.

THE investigation of the rise, progress, and nature of the disease commonly called "Asiatic Cholera," is one of the great medical problems of our time. Our interest in the subject is testified by the variety of works which have been published upon it, and by the labour which has been spent in examining it. And to those who are acquainted with the literature of cholera it will appear that a considerable amount of real knowledge has been attained. The observation of nearly thirty years has given us a mass of evidence in a great measure yet unanalysed, from which future inquirers may venture to make a safe and tolerably complete generalization. Still it cannot be denied that at present great uncertainty and obscurity exist, with regard to many of the most important points, and that the opinions of men of eminence are widely at variance. In some instances, this is probably inevitable from the difficulty of the subject, but in other cases there can be little doubt that erroneous conclusions have

been arrived at, from a want of due care in the examination of evidence and of recorded observation.

Between the history of cholera and that of previous epidemics there exists a wide and singular difference. Never before have we had an opportunity of obtaining an authentic record of the very commencement and origin of a pestilence. The ravages of the "Black Death" and of the "Sweating Sickness" occurred in a comparatively unenlightened age, and first made their appearance among rude and semi-barbarous nations. Their origin was concealed by the clouds of geographical ignorance, their nature defied the inquiry of an unformed physiology, and an imperfect chemistry. From the scattered notices and records of contemporary writers modern industry has compiled only an imperfect and disjointed narrative of their progress.

The history of cholera as an epidemic is, on the contrary, the completest in the records of medicine. Two hundred years ago, and it would have swept over the earth and again have disappeared. uninvestigated and misunderstood. But the remarkable position of the English in India, has afforded the opportunity of observing the apparent commencement of this disease in 1817, in the hot and humid atmosphere of Bengal, and of tracing with great accuracy each step of its variable and capricious progress.

It might have been anticipated that this minute investigation would have thrown no little light on the obscure subject of epidemic diseases generally. We might have imagined that some clue would be given us as to the mode in which the great subject of the spread of contagious and infectious diseases should be studied. But, on the contrary, it has increased the difficulty of the subject; another problem is given to the inquiry; another complex affection is added to the list of those diseases, dissimilar in their origin, their nature, and their treatment, but which are connected together by the one single tie of universality of prevalence.

The whole subject of epidemic diseases is, indeed, one of peculiar difficulty. The subtle agents active in their production, are for the most part unrecognized by science. The various conditions in which the bodies receiving these agents are placed, either by changes in themselves, or by physical alterations in surrounding media, are undetermined or but vaguely guessed at. The division which has been made of them into contagious and non-contagious has simplified the subject only in appearance. In reality it has left untouched the real difficulties of the case. It may be that hereafter it will be found to be little more than a verbal distinction, which nothing but ignorance of the subject could have introduced.

Before entering on the subject of the contagion of cholera, it may be as well to devote a few pages to the general consideration of contagious and epidemic agents.

The use we are about to make of that chapter in Dr. Holland's philosophical work which is entitled the 'Method of Inquiry as to Contagion,' has induced us to place the name of his book at the head of our article. We have no intention, however, to enter into anything like a review of this able work; this would be foreign to our present purpose, even if we had not previously noticed it. Suffice it to say that by its philosophical tone, and by its acute and refined criticism, it is a work destined evidently to exert a durable influence on English medical literature.

A remark made by Dr. Holland seems to us the solution of the uncertainty and variety of opinion which exists on the subject of contagion.

"In all common reasoning on the subject, and even in what has been written upon it, infection is too much regarded as a simple and uniform act; and the virus transmitted as the same in quantity and intensity. Such views, however, carry error into every part of the discussion. (Holland, p. 280.)

The full meaning of this passage can only be elucidated by a short analysis of the difficulties and obscurities surrounding the inquiry into the laws of contagion.

The most common notion of diseases contagious, and diseases epidemic but not contagious, we take to be the following. In both cases a *virus* is assumed, a *materies morbi* of undetermined nature which propagates itself when it meets with its conditions of development. In the simply contagious disease, this virus or morbid agent does not lose its power of propagation by transmission through the animal economy; it continues even to propagate itself still more, and to increase, if not in intensity, certainly in amount. In the other case, the agent exhausts itself on the individual; the changes it leads to, annihilate itself; another stock of virus from the original source is requisite for the attack of another person. In both cases, at certain times, each agent acquires an extraordinary power of development. The conditions producing this activity are but partly known, and have generally been referred to unusual deviations in the composition or ordinary states of the atmosphere.

The first question which occurs respecting this opinion is the assumption of a materies morbi, or poison. In the case of contagions communicable by inoculation we admit at once the existence of a virus; in the case of contagions which clearly infect the majority of persons, in the vicinity of diseased people, we also admit the virus, although it is incommunicable by inoculation; in the case of a disease, endemic and local, such as ague, we also analogically admit the virus, and consider it in this case existent in the atmosphere. But in other cases of diseases undoubtedly epidemic, the presence of the materies morbi is more doubtful. For example, at certain times and places, dysentery becomes so general as to be entitled to the appellation of epidemic. It affects people whose habits and modes of living have not in any way conduced to the disease, and it is referred, therefore, to some peculiarity in the atmospheric conditions; to great humidity; to sudden and unequal vicissitudes of temperature; or to the prevalence of certain winds, and other changes of a similar kind. This explanation is probably correct, and if so, it affords us an example of a disease spreading over large districts of country, on account of changes produced in the animal system, by causes acting, so far as we can understand, upon the secretions and processes of assimilation and nutrition. Here the atmosphere has produced an epidemic, and it is not possible to draw a distinction between this and another arising from less obvious causes and spreading over a wider extent of country, because this would be to assume that we are acquainted with all the alterations in the atmosphere, and can accurately measure its variations. The question stands thus: we see that certain atmospheric changes, for the most part obvious enough, are sufficient without reference to a supposed virus to induce in

a great number of people a peculiar disease; how is it to be proved, that analogous changes which may be to us more obscure and latent, do not produce other affections, by means of similar obscure influences on the processes of nutrition?

In this position now remains the subject of cholera. Some would refer it entirely to atmospheric changes; others to a material poison requiring for its propagation certain conditions of the atmosphere, but not reproduced by the human body; and a third class of observers would make it a poison reproducing itself by transmission like smallpox, and merely influenced as this disease is, by atmospheric states favorable to its generation.

At the outset of the inquiry respecting diseases decidedly contagious, difficulties arise which indicate at once how scanty is our real knowledge, and how inefficient are our means of extending it. The question we have proposed is the determination of the presence or absence of certain specific agents; difficult as this is, it becomes much more complicated, if we allow that the two classes of agents are convertible into one another. Thus, a certain class of reasoners, endeavouring to account for the various phenomena manifested in cholera, in continued fever, and other diseases, have imagined that a disease, at first non-contagious and of atmospheric origin, may gradually acquire the property of being transmitted from body to body, or may give rise to an agent capable of doing it; that at different periods of the disease, this primary or secondary agent may manifest different and apparently irreconcilable properties, being at one time expended in the system, and at another, deriving from this system increase of power; at one time annihilated, at another generated, and under both conditions producing an identical pathological state.

It is obvious, that if possible, this question should first be settled; otherwise endless fallacies may be introduced into the inquiry. But it is questionable whether it is at present susceptible of determination: arguments and suppositions on either side balance each other, and the opinions of those well informed on the subject, are in all probability equally divided.

The instance of continued fever completely illustrates the case of cholera considered in this light. Everybody admits a continued fever, arising from local circumstances, from vegetable effluvia, from malaria,—from, in fact, a modification of the causes of remittent fever; occasionally, also, a continued fever seems to arise simply from atmospheric vicissitudes. Neither of these can be considered as at first contagious; yet it is the general opinion, and one founded on long experience, that a fever exists in this country, which is undistinguishable, as far as symptoms and morbid appearances are concerned, from the two former affections, and which yet is contagious. Is this fever distinct from the former, or is it a product of them? We think that no one can doubt, after an examination of the evidence on this point, that a contagious property may be attached at a certain stage to a fever, which at first was not merely evidently caused by malaria, but which presented even the characters of a true remittent. There is equal evidence to prove that a fever apparently similar may be contagious from the commencement. Here we see two causes, seemingly

different, producing apparently an identical disease: although all reasoning on the subject would hold this to be almost impossible.

Some would here make a distinction between contagions, and would assume that the agents of smallpox, measles, or scarlatina are to be distinguished from the more undefined contagion of typhus, as being strictly generated from similar agents which pre-exist, and have never been proved to arise *de novo*. And they remark that these contagions possess also the remarkable power of destroying in the system the property it at first possessed to be affected by, and to reproduce, them.

Great as this difference is, it may be questioned whether much influence ought to be attached to it. Among the diseases themselves which are thus separated, we find differences as striking as those supposed to distinguish them from the comparatively irregular and recurrent typhus. The same virus produces apparently the mere local affection of the fauces and the true or regular scarlatina; yet one disease is recurrent and the other not; smallpox transmitted through the system of a brute undergoes the remarkable change of being afterwards able to be propagated only by actual contact. Between the different poisons of smallpox, scarlatina, and measles are found also great differences in point of diffusion, adherence to clothes, &c. It may be questioned also whether this non-recurrence of a disease is ever quite complete; we know it not to be the case in cowpox, which gives immunity only in certain cases, or, perhaps, for a certain time; perhaps smallpox would, after a longer interval than human life allows it, also return. It is not proved, on the other hand, that typhus does not give a shorter period of exemption; some writers hold this opinion, which if correct, would at once remove this distinction, as far as recurrence is concerned, between itself and the contagious exanthemata. Sir W. Pym and others are strong advocates for this opinion in the case of the "contagious" yellow fever.

The points we have alluded to give us some insight into the difficulty of the subject;—we see great changes occurring in the poison of smallpox by the action of the living system of the cow. If our experience were greater, other diseases of brutes might become known to us, as derived from diseases in man, analogous but not identical. Rabies in the dog again produces in the human body a disease distinctly modified by the transmission. If the chemistry of vitality possesses this remarkable power, why should we doubt that out of a non-contagious miasma it may generate another principle possessing the power of transmission?

At any rate, considering our ignorance, it would not be right to deny the possibility of this occurrence; and yet, admitting it, it seems hopeless to attempt to comprehend principles which, for anything we know, may be continually varying in Nature, although their appreciable effects remain the same.

Leaving this question of "convertibility," we find another difficulty almost as great, which it is necessary fully to investigate before much progress can be made in the inquiry. We have hitherto alluded only to the virus itself, but great modifications are produced in each contagious agent by the surrounding media. It is generally admitted that the effluvia from persons affected with smallpox, measles, &c., being diffused through

the atmosphere become innocuous when diluted to a certain extent. This extent is not accurately known, but appears to differ considerably in the different well-known instances of contagion. We have a right to assume that this extent will not merely depend upon the virulence, intensity, or abundance of the emanating virus, but that it will also depend upon the properties at the time of the medium with which it is commixed. That is to say, we see during certain years that, apparently from unexplained and occult conditions of the atmosphere, the virus of smallpox acquires an unusual power of development; that which happens thus to the whole body of the poison, may continually occur in those detached portions of it, which emanate from an infected person. We can conceive, then, either a rapid or a slow diffusion of a contagion, and it is not too much to assume that, in the case of smallpox, if by any chance those conditions of the atmosphere which favour its diffusion were to be absent, it might, for a time, become a disease propagated only by contact and inoculation. On the other hand, it does not require much stretch of imagination to conceive that the presence of some of its conditions of existence in unusual abundance, might cause a virus to be so rapidly diffused and dissipated as to be incapable of inoculation, and to become only infectious.

It has been observed that under certain circumstances smallpox cannot be inoculated. Dr. Copland noticed this on the west coast of Africa. The same circumstance is recorded by Norris, in the 'Philosophical Transactions for 1781,' (p. 53.) He states that, during the prevalence of the Harmattan on the African coast, smallpox is not contagious.* It has also been observed that the virus of cowpox loses its activity in particular climates in India. And this is not owing to modifications produced by heat in the dried virus, because it is propagated with difficulty even when one case has accidentally succeeded; and, in addition, virus similar to that which proves unsuccessful during one season will succeed in another. Dr. Copland states, on the other hand, that in the hot, moist, and sultry weather, following the rainy season, on the African coast, the variolous poison acquires an extraordinary power of development, and is extensively diffused and fatal.

Once admit that atmospheric conditions will modify a virus to this extent, and the whole question of contagion becomes expanded and altered. Pushed to its legitimate conclusion, this view would consider contagion as simply an accidental property impressed upon a poison by conditions which are necessarily variable and uncertain.

Without going to this length we may feel justified in doubting how far

* Dr. Dobson, in whose paper Mr. Norris's observations are recorded, remarks, "that in the year 1770 there were on board the Unity, at Uhydah, more than 300 slaves; the smallpox broke out among them, and it was determined to inoculate. Those who were inoculated before the Harmattan come on got through very well. About 70 were inoculated a day or two after the Harmattan set in, but not one had either sickness or eruption. It was imagined that the infection was effectually dispersed and the ship clear of the disorder, but in a very few weeks it began to appear among these 70. About 50 were inoculated the second time; the others had the disease in the natural way." (Philosophical Transactions for 1781, p. 54.) The Harmattan has also, according to Mr. Norris, a beneficial effect on all disorders. It is an extremely dry, but not a hot wind. The thermometer during its prevalence seldom averages more than 80 degrees at midday. On account of its dryness vegetables are much injured by it.

the division into diseases communicable only by contact, and diseases transmissible without direct contact and inoculation, may not be hereafter proved to be only an expression of greater or less volatility and diffusion.

If the phrase "conditions of existence," which we have more than once employed, be a correct one, it implies several important laws of contagion. It implies that a virus from want of such conditions may be destroyed or rendered inert, as has been said to be the case with typhus and contagious yellow fever, which are destroyed by very hot or by cold weather respectively. It implies also, not mere existence, but development and increase of a virus, and this consideration leads to the most important, because the most practical, part of the inquiry into contagion; it leads to the question whether a contagious virus is capable of indefinite multiplication, apart from well-known multiplying sources like the animal system. If this is admitted, it becomes almost impossible to make use of the usual evidence which has generally been considered sufficient to determine the question of the contagion of a particular disease.

"It is a frequent mistake," writes Dr. Holland, "in reasoning upon contagion to consider that the infectious nature of a disease may be disproved by showing that it has been spread without any obvious communication through man or human means. The two conditions brought into the question are, in fact, perfectly compatible with each other. If a virus can be transmitted from the body through a few feet of air, we are not entitled, from the partial experiments hitherto made, to set any limit to the extent to which, under favorable circumstances, it may be conveyed through the same or other medium. Common reason here concurs with our actual experience of the transmission of the virus of certain diseases in various ways and to remote distances." (Holland, p. 281.)

If this view be correct, and we do not feel disposed to contest it, much greater scope must be given to the idea of contagion. The fact of a person falling sick who could not possibly have been exposed to the exciting cause of a disease, has hitherto been considered strong negative evidence that the disease in question was not propagated by the human system, but was of atmospheric or other non-contagious origin. But such evidence under this view is worth nothing, and, in fact, it becomes difficult to disprove the presumed contagious nature of any disease spreading epidemically.

If it be allowed that a germ of contagious matter thrown off from the body of a diseased person may be carried to an indefinite distance, and may still possess (or *acquire?*) sufficient intensity to affect individuals who have not been within the usually recognized sphere of infection, then it must be admitted that quarantines, cordons, and similar attempts to localize the contagious virus, are founded on a misconception of the laws of infection; and that the only true way of preventing the spread of a disease is by recognizing, and as a consequence, removing its conditions of development.

We have entered thus far into this very obscure and extensive subject, with a view of indicating some of the difficulties which appear to us to stand in the way of its full investigation. But we are conscious that many eminent and philosophical inquirers will consider that we have imputed to the subject a vaguer and more indefinite character than really belongs to it. To take as an example two writers on cholera, whose names

appear at the head of this article, Drs. Copland and Kennedy: both these gentlemen are firm believers in the contagious nature of cholera, and in the most limited sense of the word. Contagion, according to them, implies either contact or immediate proximity, and the virus requires to act on living systems, to insure its increase and activity. Those states of the atmosphere produced by changes in itself, or by emanations into it from the soil, or from animal or vegetable decomposition, and which we have termed conditions of existence, are considered to have only a limited effect, and as not essential to the development and action of the materies morbi. When the disease spreads they trace intercourse in every case, and Dr. Copland explicitly denies that cholera has ever entered a town which had not held intercourse with an infected place.

"Those who argue against its transmissible nature cannot show a single instance of its appearance in any place without the previous communication with an infected place or persons of a nature to propagate the malady." (Copland, op. cit. par. 93.)

This view of contagion has, at any rate, the merit of being more defined and precise than the one we have already discussed. Attempts have been made to simplify it still more. Many writers are inclined to deny the power of conversion which we have attributed to the living system. They argue that all contagions are fixed, definite, and inconvertible, and once arising *de novo*, probably from animal sources, ever after remain gradually increasing or decreasing according to circumstances.

If this could be proved, the question would at once assume a more decided form and a narrower basis. It appears to us that Dr. Copland has adopted this view, and has laboured to support it with great consistency in all his writings on contagious diseases. In the same category with smallpox, measles, &c., he would include yellow fever, cholera, plague, and typhus. Thus in the case of yellow fever, although he is obliged to admit that a malignant remittent fever will arise from malaria, he yet endeavours to distinguish this from true yellow fever, which he supposes to spring from a contagion of its own, to be a disease *sui generis*, and not capable of recurrence. He distinguishes the "typhoid fever" from true typhus, the one arising from malaria, the other from a specific contagion, and does not allow that one is convertible into the other. So also with cholera; he endeavours to prove its true contagious nature, and is evidently strongly inclined to believe that it only occurs once during life.

Although we cannot regard the definition of contagion as at all settled, and believe that great differences of opinion exist in the minds of many observers as to the full meaning of the word, it is obviously necessary that in entering into the evidence of contagion, as far as cholera is concerned, we should have some decided understanding on the point in question. We will therefore adopt, for the time, the limited and restricted definition of contagion, as implying either contact or proximity. This is the sense in which it is received by the two authors who have most recently written on the subject. It is a view of the question which they profess to prove by evidence, and presents therefore a tangible and defined point for discussion.

In his present article, Dr. Copland reproduces the evidence which he brought forward in 1832, to prove the contagion of cholera. He is also entirely consistent with the view he has taken of contagion generally; he

allows no compromise, and makes no reservation: cholera spreads by human intercourse alone, and derives its existence or action from the living body only.

Dr. Kennedy's work is a reprint of one published at Calcutta in 1826. His arguments for contagion are entirely drawn from the Bombay Medical Report, published in 1819. Although an ardent contagionist, he does not seem to have been able to collect any personal evidence of the fact; and the experience of twenty years, since the first edition of his work, has apparently suggested to him no new argument in its favour.

It is admitted by all parties that cholera is not communicable by inoculation. The observations on this point are perfectly conclusive, and indeed undisputed. It is necessary to say a few words on the precautions necessary in examining into the evidence of a disease which is contagious, but not communicable by inoculation.

We have alluded to the difficulty incident to the subject from our ignorance of the real nature of these presumed morbid poisons, of their alterations, modes of development, and conditions of existence. But in tracing their effects on the system, we find no less obscurity and difficulty produced by our ignorance of the state of the system necessary for their manifestation. If we were doubtful about the agent, we are still more doubtful about the recipient. Of one thing we seem to be certain; that there must be a peculiar condition of the body, a predisposition before the agent can act; and this predisposition, in the case of the better known contagions, is all but universal; every person has it, and yet immediately after it has developed the disease, it is lost, obliterated, and future exposure to the virus produces no return of it. What this predisposition is, in the case of smallpox, measles, &c., we are totally ignorant; the recondite processes of sanguifaction and nutrition concerned in its production and destruction are unrecognized by our present science.

Assuming cholera to be contagious, a predisposition of body must be also assumed. It is also fair to believe that this predisposition is often absent. We know that in the case of smallpox it is sometimes absent. It is impossible to define or to limit the frequency of its absence in the case of cholera.

Admitting this, we must also admit that in examining the evidence of contagion we must anticipate a great deal of negative evidence; we must expect many instances of immunity in persons exposed to the virus, but who wanted the predisposition.

We proceed, then, briefly to consider the evidence of the contagion of cholera, using the term conventionally in its restricted sense, as implying either contact or proximity.

The principal evidence on which a disease incommunicable by inoculation, is yet supposed to be contagious, is chiefly derived from the following sources:

1. It is found that those most exposed to the risk of infection, viz. medical men, friends of the sick, and hospital attendants, show a greater percentage of attacks than persons not so exposed. It does not follow that this increased percentage should be considerable, but it must be uniform and constant.

2. Individuals or bodies of men infected with the disease are capable of carrying it from place to place, and of communicating it to the fixed inhabitants. Fixed inhabitants never receive this disease unless it has been imported by the arrival of infected individuals. It is admitted, however, that it is always a difficult matter to trace the spread of the disease from such infected persons.

3. Individuals or bodies of men preserving a strict quarantine, must necessarily show a diminished percentage of attacks. Allowing that incidental and untraceable introductions of the disease may occasionally occur, still this cannot be supposed completely to equalize the proportion.

We shall shortly consider the evidence under these three heads:

I. *Do medical and other attendants on the sick show an increased percentage of attack?*

Dr. Copland answers in the affirmative. As far as India is concerned, he adduces 3 instances of positive evidence, viz. the case of the 65th Regiment alluded to by Drs. Burrel and Craw, and the statements of Drs. Stokes and Daun. There are a few other statements of a similar kind in the Indian Reports which he has not noticed. The negative evidence from India, opposed to this view, is very definite and conclusive.

In the 'Bombay Report,' published in 1819, we find 39 letters, of which 37 are from different medical officers. Of these, 23 do not allude in any way to the question of contagion, and mention nothing about their hospital attendants; 6 believe in contagion, but of these, 3 mention that the assistants and hospital attendants were not attacked, viz. Dr. Taylor (p. 195), Dr. Jukes (p. 172), and Captain Sykes (p. 118). Of the other 3 who profess their belief in contagion, 2, viz. Drs. Burrel and Craw, cite the same instance of hospital attendants being attacked, and the remaining gentleman, Mr. Coats, does not refer at all to hospital attendants. (Bombay Report, p. 145.)

In the 'Madras Report,' published in 1824, we find altogether 8 instances where the reporters state that those nearest the sick were attacked, but several of these are very doubtful cases, being imperfectly and loosely reported. The greater number of reporters do not make any statement on one side or the other. The following gentlemen expressly state that none of their attendants were ever attacked, viz., Messrs. Gibson (p. 1), Haines (p. 32), M'Andrew (p. 33), Smith (p. 38), Sutton (p. 41-5), Scott (p. 57), White (p. 65), Campbell (p. 77), Goldie (p. 137), Mitchell (p. 141), Chalmers (p. 142), Fasken (p. 201), and Barton (p. 208). In addition, Mr. Scott, the editor of the Report, brings forward many other cases "of the most striking instances of immunity from the disease under the most intimate personal intercourse." (Madras Report.)

In the 'Bengal Report,' published in 1820, we find the editor, Mr. Jameson, stating, "that the medical observer, in his attendance upon patients, did not find himself or his assistants more liable to be attacked by it than such persons as had no communication with the infected" (Bengal Report, p. 123); and he also gives a list of between 250 and 300 medical men in the Company's service in Bengal, most of whom saw the disease largely, and of these only 3 persons were attacked, and only 1 attack was fatal.

The chief Indian writers on cholera since this date, who allude to this point, are Messrs. Orton, Annesley, Twining, and Corbyn; and there are two or three allusions to it in various numbers of the 'Calcutta Transactions,' and of the 'Madras Medical Journal.'

Of these gentlemen, Mr. Orton was the only one who believed in contagion, and yet, after examining into the evidence on the point in question, he says:

"We are *therefore forced* to the conclusion, however at variance with the common laws of contagion, that in this disease, at least, in India, the *most intimate intercourse* with the sick is not in general productive of more infection than the average quantity throughout the community." (Orton on Cholera, 2d Edition, p. 326.)

The other writers are all disbelievers in contagion, and make strong statements respecting the exemption of hospital attendants. For these, see Annesley (Sketches, &c., p. 244), Twining (On Diseases of Bengal, p. 536, 1st Edition, 1832), Corbyn (On Cholera, p. 96), and the 'Calcutta Transactions' (vol. v, pp. 318-19, and vol. vii, p. 330).

The negative evidence then, as far as India is concerned, is exceedingly strong, and did our limits permit us to extract the different statements on either side, it would be seen how much more definite and satisfactory are the arguments of those who argue for the immunity of the hospital attendants, than of those who hold the opposite opinion. In Egypt, Clot Bey states that no hospital attendants were attacked. (Annales de Médecine Physiologique for 1833.)

The European evidence on this point is more contradictory, but still the negative side has a great preponderance of opinion and statement. At Orenburg the attendants were entirely exempt. (Lichtenstadt.)

In St. Petersburg it appears that there was from some cause an unusual mortality among the physicians, but it is not known whether all those who died of cholera had been in personal contact with the disease. Drs. Russell and Barry think that many hospital attendants were attacked in the ill-ventilated hospitals. In some hospitals very few were attacked. Dr. Hamett, on the authority of Drs. Barchervitz and Daun, denies that any of the nurses or sick attendants were attacked in the Cholera Hospitals of Dantzic, Moscow, St. Petersburg, and West Prussia. (Hamett's Report on Cholera at Dantzic, p. 157.)

Sir G. Lefevre "never knew any instance of sick persons communicating the disease to their attendants." (On Cholera in St. Petersburg, p. 33.)

Dr. Becker states that hospital attendants were attacked in Berlin, but his evidence is not satisfactory. He quotes a number of instances in the Cholera Hospital, No. 1, of Berlin, as having been affected by cholera, but on examining these it is extremely doubtful whether more than 4 out of 39 were cases of true cholera.

In Warsaw the medical men and attendants enjoyed a remarkable exemption from attack. (Relation Historique, &c., par A. Brierre de Boismont, p. 130.)

In Paris the physicians and surgeons of the Hôtel Dieu, which received a great number of cholera cases, signed a declaration that they had not observed any circumstances authorizing them to suspect contagion. This

of course implies the immunity of the attendants. To this declaration are appended the names of Dupuytren, Gendrin, Breschet, Majendie, Petit, Recamier, and others of equal celebrity.

Without wearying our readers by undue prolixity, we may state that the negative evidence afforded us by the spread of the cholera in Europe, preponderates greatly over the positive evidence, though not perhaps in so great a degree as the Indian evidence on the same side.

We do not see how we can avoid the conclusion, that there is not sufficient evidence to prove that those nearest the sick are more liable to attack than others not so exposed.

II. *Do individuals or bodies of men, in carrying the disease from place to place, communicate it to the fixed inhabitants?*

Unless a disease be virulently contagious, it is exceedingly difficult to decide a question of this kind. In addition to all the sources of fallacies attributable to variations in the morbid agent, or to the systems on which it acts, we have to take into account the partial and biassed evidence always given on either side of a disputed question, the possibility of coincidences, the doubts which must always exist as to the correctness of diagnosis, and other difficulties of a similar kind.

The carriers of the contagious disease may either be single individuals, or bodies of men, either military or civil. The movements of troops are those best fitted for our examination, as the men move more compactly, and there is no doubt of the time when they enter or leave a place. Bodies of men, not military, are scattered over the country, enter towns and villages at indefinite times, and it is very difficult to prove that the disease was not antecedent to their entrance.

We shall therefore confine ourselves chiefly to military movements, and shall select for criticism two instances of the supposed conveyance of cholera by detachments of troops. The full discussion of all reputed cases would exceed our limits.

1. The first instance is the presumed transmission from Nagpore to Jaulnah in 1818. This is a strong contagionist case, and we have selected it because it is quoted not only by both Drs. Copland and Kennedy, but also by the majority of writers on cholera, and among them the very able author of the article "Cholera," in the 'Cyclopædia of Medicine.'

We possess three Reports from Jaulnah at this time, viz. from Mr. Peyton, the staff-surgeon; from Mr. Campbell, the assistant-surgeon of the Horse Artillery; and from Mr. Kellie, the assistant-surgeon of that native corps.

This last gentleman is a contagionist, and it is from his report that this instance of conveyed cholera has been derived. Dr. Copland, in common with many advocates of contagion, overlooks altogether the reports of Messrs. Peyton and Campbell, who both decide against contagion, and who, with opportunities of observation equal to those of Mr. Kellie, are at least as worthy of being consulted.

In the report from Nagpore it is merely said "to have originated among the citizens in the middle of May, but no case occurred among the troops till the 26th or 27th, although our Sepoys were in habits of daily and intimate intercourse with the inhabitants." (Madras Report, p. 65.)

Jaulnah is but a short distance from Nagpore, and of course, in the

gradual progress of cholera, it might be expected to arrive at the former place. The south-west wind however prevailing, its progress was slow, and it took six weeks to traverse these few miles. During this time there must necessarily have been almost daily arrivals of natives from Nagpore, as no quarantine was ever dreamt of. If there were such arrivals, and it is the duty of the contagionist to prove there were not, they failed to propagate the disease. But let us hear Mr. Kellie :

"Several heavy showers fell about the middle of June. The disease prevailed in Nagpore during the month of May, and upon hearing of the march of Captain Doveton with a detachment, some of whom were affected with the cholera morbus, it was generally apprehended that the disease might be brought hither with it. The detachment arrived here towards the end of June. The cholera appeared on the 3d of July." (Madras Report, pp. 70 et seq.)

To make his case good and prove something beyond coincidence, Mr. Kellie should have shown, if possible, the first steps and commencing spread of the contagion. No care, however, seems to have been taken in inquiring into particulars, and consequently we must receive Mr. Kellie's statement with some degree of doubt.

This doubt is, however, still more increased as we proceed; we find Mr. Peyton the staff-surgeon, denying the contagion, and attributing the disease to the peculiarity of the weather, the features of which he details in his Report. Mr. Campbell makes similar observations. So that here is conflicting evidence: are we to believe Mr. Kellie or Messrs. Peyton and Campbell? The only way, supposing the opinions of all to be of equal weight, is to fall back on general experience, and inquire whether similar instances afford support to one or the other party.

We are not certain, however, that Mr. Kellie is entitled to much credit. He had evidently prejudged the case, as appears by his anticipating that the disease would be brought by Captain Doveton's detachment; and he makes one statement which is certainly erroneous. He mentions that during the epidemic, the "Royals suffered much," and the "Horse Artillery little," and explains this by saying, that the men of the first corps had intercourse with infected people, and those of the second not.

This is, however, contradicted both by Messrs. Peyton and Campbell.

The sufferings of the "Royals" are attributed by Mr. Peyton to their being in "old uncomfortable barracks," and he says when they were moved into tents, the number of attacks was diminished one third in a single day. He expressly mentions that no check was put on communication.

Mr. Campbell's evidence is still more important, and his position as medical officer of the troop of Horse Artillery referred to, enabled him to speak with great decision on this point.

"That this disease is not infectious, but is caused by the direct application to the surface of a peculiar miasma, existing in particular strata of the atmosphere, has been fully proved by the history of its progress, as well as by the phenomena attending it. Had it been infectious at this station, the Horse Artillery could not have escaped with so few cases, situated as *they were in constant communication with the Royals, and the bazaars of the force.* Neither, had it been infectious in its nature, could the medical officers, and the numerous attendants on the sick, who must have been peculiarly exposed to infection, have escaped its ravages." (Madras Report, p. 77.)

Here is direct evidence that Mr. Kellie made an unfounded statement, when he attributed the exemption of the Horse Artillery to want of intercourse. The contrary is positively asserted, by the medical officer of that corps.

In fact, this celebrated instance and reputed proof of contagion is clearly untenable; yet this is the strongest example to be found in the whole of the Madras Reports.

We have carefully reviewed the whole history of cholera breaking out in the marches of troops in India, as far as such history is known in the various reports published in the Presidencies, and in the different writings of authors and periodicals, and we can find no unequivocal instance of transmission. Mr. Jameson satisfactorily accounts, in our opinion, for the attack of the disease in the two doubtful cases detailed in the 'Bengal Report.' Mr. Scott gives 50 marches made by European and Native troops in 1819, 1820, and 1821, and yet we can find only 4 instances in which such marches were supposed to have carried the disease. Dr. Lorimer in his excellent Report, in which 144 marches with cholera are recorded, is able to contribute little evidence on this point; he says merely "in 6 instances it is incidentally recorded to have broken out among the villages." (Lorimer, p. 8.)

In any future observations which may be made on marching in India, we would recommend attention being particularly directed to this circumstance; and the commanding officers, and surgeons of regiments, should be called upon to state their reasons for believing that the outbreak of cholera in a village was really posterior in time to the passage of troops, and not contemporaneous or even slightly anterior.

As an instance of apparent coincidence in the attack on troops and towns, through which they march, we would instance the march of H. M. 69th Regiment in 1818, to Cannanore. The regiment was attacked with cholera on the banks of the Madoor River, 7 miles from Seringapatam; the disease prevailed severely as they marched through the Wynaad Pass, and remained till they arrived at the hill-pass of Pereapatam. The villages and towns on the route were affected either at the time, or subsequently to the passage of the regiment; consequently Mr. Gibson, the assistant-surgeon, unhesitatingly refers it to contagion. But a stricter survey of the mode in which cholera spreads, renders it doubtful whether this may not have been a mere coincidence; the disease may have been creeping along in its usual singular manner, and making, as it were, equal marches with the troops. And this really appears to have been the case. After descending the Ghauts, the regiment seems to have outmarched the disease, and in November, 1818, entered Cannanore healthy, after having been put in quarantine. The cholera, however, followed them on the same route, and attacked Cannanore, after the 69th had been there a length of time sufficient to remove from them all suspicion of contagion. The account of this march also gives us an illustration of the way in which evidence may be vitiated by time. Mr. Gibson, the assistant-surgeon of the regiment, detailing this march in the 'Edinburgh Medical and Surgical Journal,' (vol. xxxviii, p. 289,) states that cholera did not reach Cannanore for a year after the entrance of the 69th Regiment, and then came down coastways from Bombay. This is an error of memory, as Surgeon Wyse

reports it both at Tellicherry and Cannanore, in December 1818, and January, 1819 (Madras Report, p. 147); and Mr. Scott states that it commenced at Cannanore, on the 5th of December, 1818. (p. 11.)

2. The second instance we shall adduce of troops apparently conveying cholera, is an European case; viz. the presumed introduction of cholera into Poland, by the Russians. We have taken this case because it is cited not only by Drs. Copland, Becker, and others, but has been supposed generally to be a decided instance of transmission.

"The cholera morbus," writes M. Boisseau, "declared itself in Poland the 10th of April, 1831, at Iganie, after a battle with the Russians. This circumstance has been considered manifestly to demonstrate the contagion and the importation of this disease." (Traité du Choléra Morbus, &c., par F. G. Boisseau, Paris, 1831, p. 188.)

But afterwards M. Boisseau says, "already, in 1830, M. Sauvé had observed it with the same symptoms." (Ibid. p. 188.)

M. Brierre de Boismont was present at the combat of Iganie, on the 10th of April, and informs us that the first deaths took place among the Poles on the 13th. He discusses the question of contagion very fully. He says that the troops encamped constantly on a marshy ground, and drank muddy and unhealthy water. The divisions of the army which drank this water were those chiefly attacked. (Relation Historique, p. 137.) He describes vividly the miserable condition of the Polish army, and points out how predisposed they must have been for the attack of any disease, contagious or otherwise.

"Conceive, " he says, " these men, wan, ghastly, yellow, and emaciated, whose features express their sufferings, enfeebled by long marches, by privations of every kind, bivouacked for five months, in bitter frost, in woods or on marshy soils, and one can with difficulty form an exact idea of the state of these unhappy victims of war." (Ibid. p. 5.)

" After the battle" he adds, "the troops returned to their former bivouac, and on arriving, they drank eagerly of the muddy water of the marsh." (Ibid. p. 138.)

He seems inclined to think these circumstances would explain the attack of cholera, but does not hazard a decision, and concludes his examination by saying, that immense difficulties encompass the subject which cannot yet be solved. (Ibid. p. 143.)

The question then seemed very doubtful to an eyewitness, who would have been able to come to some conclusion, if this had been attainable.

The French Commissioners sent to Poland by the Minister of War, especially to examine this occurrence, also leave it undecided, but express themselves more opposed to contagion than in favour of it.

" The contagion of cholera morbus is not yet proved in Poland. (Chamberet.) It is better to remain doubtful on this question." (Trachez.) (Du Choléra Morbus de Pologne. Renseignemens sur cette Maladie &c. &c., par la Comm. envoyée à Varsovie par M. le Maréchal Duc de Dalmatie, Paris, 1832.)

The Commission also records a remarkable fact.

" Nevertheless, two months before this time (the battle of Iganie), and before there was any question of the appearance of the cholera in Poland, Dr. Brandt, the President of the Committee of Health, had observed all the symptoms of this formidable malady in three individuals." (Ibid. p. 14-15.)

The Commission afterwards remarks :

"We have shown that the cholera, which we have observed in this country, does not communicate itself from one person to another either by contact or infection." (Soit par le contact médiat, soit le contact immédiat.) (Ibid. p. 57.)

With evidence so conflicting and various, it is impossible to feel that this supposed introduction into Poland is a case on which we can safely rely.

These two examples must be taken as samples of the uncertainty, which surrounds every similar case.

The instances in which individuals have been supposed to introduce cholera into towns or districts are very numerous. Yet in no one case is the evidence perfectly unequivocal. This is so much the case in India, that Mr. Orton, although a believer in contagion, is compelled to write—

"Another hiatus in the evidence of contagion is, that though the importation of the disease has so often been found immediately to precede its appearance in the inhabitants of a place, and even the first cases to arise in the neighbourhood of the imported virus, it has scarcely ever been possible to trace these cases in anything like a regular series." (Orton, p. 326.)

Mr. Scott's case is the strongest in India. Strong doubts can be thrown on that quoted by Dr. Taylor, in the 'Bombay Medical Report.'

Let us take a few individual cases out of India. Its introduction into Orenburg, in 1829, is ascribed either to the caravans, or to the Kirghis Kaisacks. But the Report of Dr. Lichtenstadt seems to us conclusive against either supposition. (Edinburgh Medical and Surgical Journal, for 1831. No. 108, p. 121.)

In the case of Astrachan a most important item of evidence is omitted; as every one will see on reading over the evidence. The ship supposed to have imported the disease was still in the lazaretto, when 17 days after her arrival the disease broke out in the town. No attempt even is made to prove intercourse between the sailors and the inhabitants.

So also at St. Petersburg, Drs. Russell and Barry state that "no direct intercourse has yet been traced between any of the first 5 or 6 cases." (Report, p. 25.) We cannot indeed conceive how introduction from vessels arriving from infected places on the Volga, can be maintained, when we find the first case in St. Petersburg occurring in a journeyman painter, according to the Russian medical men (Protocol sur le premier malade, p. 30), who had not been out of the city, nor had intercourse with any diseased person. Drs. Russell and Barry indeed state that a merchant, who was in his ship in the river, was first seized, but they admit this was a slight case, and out of danger in two hours ; and independent of the doubt which must be felt as to whether so mild a form could transmit the disease, no communication was traced between the merchant and the painter.

Jannichen states that at Moscow the disease could not be traced to importation. Mr. Fergus makes a similar avowal as to Vienna.

Another case which has been much quoted is that of the Topaze frigate, which arrived at the Mauritius from Ceylon on the 29th of October, 1818, having lost 3 men from cholera; 20 days after her arrival cholera appeared

among the natives in the bazaar. Without going into this disputed question, it appears to us that the long period of incubation we should have to assume, if we allow the importation by the Topaze, is sufficient to render this case very doubtful. Eight days is the longest incubative period any writer has ever allowed. The Genoese medical commission sent to Hungary make the longest period 5 days. James Kennedy makes it 3 days. (On Contagious Cholera, p. 215.) Dr. Becker, of Berlin, makes the extreme period 6 days. (On Cholera in Prussia, p. 33.) Russell and Barry make the extreme 5 days. (Report, p. 93.) The 'Cholera Gazette' gives 7 days as the longest period.

Unless it can be clearly proved that the incubative period is sometimes 20 days, or even in this case longer, as it would have to be calculated from the date of the last death on board the frigate, we cannot admit this case as an undoubted and unequivocal instance. The able Report of Dr. Kinnis supplies us with other strong reasons against the importation, but considering the above argument as conclusive, we shall not occupy space in their detail.

The introduction and the diffusion of cholera in England has been a matter of great dispute. After an attentive investigation of the Sunderland controversy, we cannot consider the introduction into that place as at all proved, or even rendered probable.

In the subsequent spread of the disease there is more proof of transmission. Dr. Simpson, in the 49th vol. of the 'Edinburgh Medical and Surgical Journal,' has collected many very strong instances of apparent contagion; only a minority of these, however, are from his own observation. The death of the two nurses at Barthgate is to us the strongest instance he brings forward.

Mr. Sharkey, in the 16th volume of the 'Dublin Journal,' adduces other similar instances, and Dr. Graves, in his excellent article in the same Number, which contains an able exposition of the chief arguments for contagion, has given evidence of a similar kind.

On the other hand, in certain suspicious cases, a strict investigation has shown the evidence for contagion to be worthless. Dr. Craigie's observations on the cases in Musselburgh show how cautious we ought to be in accepting these instances of contagion, which, till carefully examined, appear so strong and incontrovertible.

In America the disease appeared 8 months after its first outbreak in Sunderland. Dr. Jackson's observations on cholera in Quebec seem to discountenance the idea of importation, and certainly there is no evidence of any emigrant ship, with cholera on board, having arrived at Quebec at the time stated. Dr. Graves, however, is not satisfied with Dr. Jackson's objections.

A much stronger instance than this is to be found in the case related in the 'American Journal of Medical Science' for 1833 by Dr. Dickson. The brig Amelia, with an infected crew, being wrecked on Folly Island, near Charlestown, cholera was communicated to certain persons who came in contact with the diseased crew, but to these persons only, and did not spread in Charlestown. This is, perhaps, the best authenticated instance of importation in the whole history of cholera. Dr. Keckeley, in the 14th

volume of the same Journal, attempts to explain it away, but it must be confessed he does not at all succeed in the attempt.

We could ourselves state two or three instances (out of hundreds of an opposite kind) which presented all the ordinary evidences of propagation by contagion.

It may be concluded, then, that the evidence for the contagion of cholera derived from this source is of a much stronger character than that derived from any other quarter.

III. *Do methods of isolation and quarantine ensure exemption in individuals practising them?*

The evidence on this point is completely on the negative side.

Quarantines failed in the only instance in India in which they were tried, viz. at Travancore; they failed at Alexandria and Constantinople. (Clot Bey, and Russell and Barry, p. 67.) They failed in Hungary, Austria, Prussia, and Russia so completely, that Dr, Becker, a strong contagionist, is compelled to state that the inefficacy of the preventive measures in arresting the ultimate progress of the disease is a fact which he willingly admits. (Becker, p. 29.)

Astrachan was in strict quarantine when the disease broke out. A rigid cordon was formed to prevent communication with Russia and Poland, and yet the disease appeared immediately at Dantzic. (Hamett, p. 14; Becker, p. 43.) Konigsberg was attacked in the same way, and in both these cases, as in that of Hungary and Dublin, cholera broke out without any previous communication with an infected place. Dr. Copland states that no instance of such an occurrence is known, but this appears to us erroneous.

The course of the cholera through Egypt affords us striking instances of the inefficacy of quarantines. The harems in Mahomedan countries may at at all times be said to be in a state of quarantine; the seclusion is so rigid and so guarded, that even in the prevalence of the plague, little more precaution can be adopted. In Broussais's 'Annales de la Médecine Physiologique' for September, 1833, is an account from Clot Bey, who states that the harems watched with the most strict quarantine were not exempt. This observer also states that the Viceroy embarked on board a vessel at the first outbreak, and preserved a very strict quarantine; in spite of this, several of his retinue died, and he changed his vessel several times. Clot Bey also mentions that 500 Bedouins encamped in the Desert, and preserving a strict quarantine, did not escape.

In Egypt, as in India, the remarkable fact was noticed that some villages remained altogether free, although there was a constant communication between them and infected places.

On the other hand, we have some instances of cordons and quarantines arresting the disease. The case of the Isle of Bourbon is the most celebrated of these. Cholera was prevailing in the Mauritius in 1818, and Bourbon was accordingly put in strict quarantine. The disease, however, appeared at St. Denis, and was said to have been introduced by the crew of a smuggler. St. Denis being put in quarantine, the disease did not spread over the island. Madagascar, however, being attacked and no quarantine being observed, the mortality was very great.

This is certainly a strong case, but there are some objections to its reception as good evidence. It is derived entirely from the reports of the French physicians, who were bigoted contagionists; the principal writer who insists upon it is M. Moreau de Jonnès, whose elaborate account of cholera is, in many respects, very inaccurate; in fact, so inaccurate, that we cannot be too cautious in our investigation of his statements. He seems to think that nothing is necessary to prove importation of cholera beyond the mere fact of communication existing between two places, one of which is infected. The minutiæ of evidence are altogether disregarded by him; and occasionally, to prove his point, he makes unfounded assertions. Thus he states that Bankok received cholera by trading-vessels from India. Dr. Copland repeats this after him. The fact is, on the contrary, that cholera passed overland, both into Siam and China, first traversing Tartary and Burmah. (Calcutta Transactions, vol. i, p. 205; Bengal Report, &c.) Nevertheless we should have been willing to receive the statements of the French physicians respecting Bourbon, if all subsequent experience had not so completely proved the inutility of preventive measures. If quarantines answered so well at Bourbon, how was it they failed in Europe? The Hungarian cordon was most strictly kept; human intercourse was impossible, and yet cholera appeared. So also in other instances. We agree with Mr. Orton on this point, who says, "we know how warmly the French physicians espoused the cause of contagion, and, therefore, their general statements must be received *cum grano salis*." (Orton, p. 334.)

The other reputed instances of exemption by aid of quarantine, are unworthy of notice.

The instances of villages escaping is no argument that this was owing to seclusion, as we know that cholera, in its strange and eccentric career, constantly passes along particular tracts of country, omitting, in a manner apparently capricious, some portions of it, and attacking others. An instance of this is recorded in the 'Bombay Report,' and is quoted by Dr. Kennedy. Some villages on the island of Salsette during the first visitation of cholera remained free; this was attributed to isolation: but a few weeks afterwards the disease suddenly retraced its steps, and now attacked the localities it had formerly spared. (Preface and Appendix to the Bombay Report.)

Another remarkable instance is recorded in the 'Bengal Report.' Sundeep, an island in the Sunderbunds, remained, in 1818 and 1819, perfectly free from cholera, although the islands all round it, similar in position, cultivation, and geological features, suffered frightfully, and for long periods, from the disease. This singular and unexplained exemption would doubtless have been attributed to quarantine, if this had been practised, but free and uninterrupted intercourse was carried on the whole time between Sundeep and the adjoining islands.

Another instance is the singular exemption of the island Kristofsky, which communicates with the island of St. Petersburg by two magnificent bridges, and with the town by a thousand barges continually plying; it is a frequent resort of the citizens, particularly on Sunday; yet during the prevalence of cholera in St. Petersburg not a single case occurred here. (Observations sur le Choléra Morbus, par l'Ambassade de France en Russie. Paris, October, 1831.)

Before concluding these brief remarks on the evidence of the contagion of cholera, we must allude to another opinion expressed by Dr. Copland, and which we think he is perfectly consistent in holding. He asserts that "the introduction of cholera into this country was certainly owing to the clothes and bedding of sailors who had died at Riga. Of this fact several proofs of a most incontrovertible nature were furnished me by two masters of vessels." (Copland, op. cit.)

In the absence of knowledge of these "proofs," with which Dr. Copland has not favoured us, we have been able to find no evidence of this fact, and we observe that it is doubted by many staunch contagionists.

Thus Dr. Becker, of Berlin, writes:

"There is in the whole history of cholera no authenticated case in which the contagion has been transmitted from one place to another by letters, new clothes, or merchandise of any description." (Becker, p. 35.)

Doepp, the physician to the Foundling Hospital, St. Petersburg, and a strong contagionist, writes:

"I have given myself great trouble to ascertain if the clothes and linen covered with the perspiration of the sick were capable of transmitting the contagion, but I could not meet with any instance of it." (Doepp, quoted by Russell and Barry, p. 146.)

The committee of St. Petersburg having first decided that clothes could carry the disease, reversed this opinion on further inquiry, and, by an imperial decree of August 1831, the precautions of fumigating goods and other safeguards previously adopted were abolished.

In England, the same opinion was held by the best observers. Dr. Brown states that medical attendants never conveyed the disease in their clothes to their families or patients. (Cyclopædia of Practical Medicine.)

The preceding brief sketch is a faithful sample of a much more extensive investigation which we have made, but which our limits will not permit us to detail at length. We have endeavoured to judge each portion of evidence with the utmost impartiality, and to attach to each observation that degree of credence which its greater or less preciseness and exactitude seemed to demand.

Having arrived at the end of this part of the inquiry, we will state the following as the conclusions, which appears to us warranted:—

1. The evidence for the contagion, in the restricted meaning of the term, of cholera is, for the most part, imperfect. This remark particularly applies to the observations collected under the first and third heads:—exposure to the disease is not proved to be followed by increased liability; and isolation is not proved to bestow increased security.

2. At the same time, the instances where cholera has appeared in a place after the arrival of infected persons, are very numerous, and seem to indicate something more than a mere coincidence. There are also some authenticated instances, in which people who had been in contact with these newly arrived persons were the first, or the only sufferers.

3. Admitting that there is evidence of contagion in a limited degree, and in certain cases, it is evidently impossible to consider this a sufficient explanation of the way in which cholera sometimes suddenly attacked large bodies of men, or has travelled over the world, and still continues to affect immense tracts of country in Asia.

In fact, we do not think any one can study the peculiar mode in which cholera attacks a country, either as detailed in the voluminous writings on the subject, commencing with Mr. Jameson's unequalled Report, or by personal witness of its past invasion of Europe, or its present ravages in India, without feeling that contagion *per se* is utterly inadequate to account for its attendant phenomena.

The question then is, whether these two conditions are compatible: if we can conceive a disease to be ordinarily propagated by other agency than that of reproduction by the human system, and yet at times to be so reproduced, and to acquire, in ordinary language, a contagious property?

This is a compromise between two extreme opinions, and is not, perhaps, likely to find favour with either party. Its very simplicity is against it; it seems like cutting the Gordian knot; it may, nevertheless, be the truth.

The observations made at the commencement of this article, on the remarkable changes which the morbid poisons in various ways undergo, at any rate show the possibility of this occurrence. In the present state of science, it would be impossible to prove its absolute certainty.

After all, the question is most important in a scientific view,—practically, as far as legislation and prevention are concerned, it may be considered settled. There can exist no doubt, that even if cholera be contagious, it cannot be localized and restrained by quarantines; theory and experience both demonstrate the inutility of preventive measures of this kind.

The plague, the yellow fever, and the cholera stand nearly in the same position; the discussion of their contagion is in no case yet ended. And we doubt whether a decision in which all parties can agree will be arrived at in these three instances, without the adoption of methods of research more accurate, and of modes of inquiry more comprehensive. The final determination of this question may perchance be the triumph of an advanced organic chemistry, which may hereafter render determinate and visible the agents which are to us so recondite and obscure.

Leaving then, in this indefinite state, the question of the contagious or non-contagious nature of cholera in all instances, let us turn for a few brief moments to the hypotheses which have been formed as to the nature of the agent causing it.

1. The disease has been considered by some to be entirely atmospheric; that is, produced by changes in the ponderable or imponderable elements of the air without the addition of some new ingredient. This hypothesis is contradicted, however, by all the phenomena, which seem decidedly to point out the presence of a virus derived from sources foreign to the atmosphere, and merely existing in it.

2. The materies morbi has been supposed to be a modification of vegetable miasma, produced by peculiar causes of heat, moisture, &c., acting on the productions of the soil.

3. It is supposed to be an agent evolved from the crust of the earth, and produced by volcanic and other changes. This has been a favorite hypothesis in all epidemics, and many curious coincidences between earthquakes, volcanic eruptions, &c., and epidemic diseases have been collected in the extraordinary work of Noah Webster, and in Hecker's 'Epidemics of the Middle Ages.'

4. Another party, leaving in obscurity its origin, regard it when wit-

nessed by us as always allied to the human system. This is the contagious doctrine as developed by Copland, Becker, and others.

5. It has been surmised to be caused by animal life, existent in the atmosphere under certain circumstances.

Our space will only allow us to say a few words on this last very interesting and ingenious conjecture. The arguments on which it is founded have been well stated by Dr. Holland in a chapter which we shall not attempt to imitate, and could not without injury condense. We believe he has stated the principal circumstances which give plausibility to this view. It is certainly an argument in favour of this hypothesis, that it is listened to with greater favour at the present day than it was in 1755, when Linnæus published the essays of Nyander. The progress of science since that time has shown the universal and, apparently, the necessary prevalence of life in all departments of nature. Accustomed as we are to consider everything around us as endowed with life; to recognize in rocks and soils the débris of vitality; in the most subtile fluids the dwelling-places of animalculæ; even in the atmosphere itself the matrix of microscopic germs—there is nothing incredible in the hypothesis of the existence of animals as yet unrecognized by our sight or instruments. Every one even is willing to admit that such races of animals must necessarily exist, and will some day become known to us. But, admitting this, and admitting that the hypothesis of insect propagation being a cause of disease would apparently accord with many of the phenomena familiarly witnessed during epidemics, we cannot allow that any more than an ingenious speculation is made out. The assumptions which we must make before entering on the proper evidence of such a view are enough to stagger our belief.

1. We have to assume the existence of these unrecognized animalculæ.

2. We have to assume that these animalculæ really possess the singular property of producing a formal and peculiar disease in the animal system.

3. We have to assume that for different diseases there is a distinct race of animalculæ. This assumption is quite necessary: it is impossible to restrict the influence of this cause to cholera alone. It must be extended to all diseases, or, if not to all, at any rate to all contagious and epidemic diseases. This, in fact, was done by Nyander, and he has been followed by many writers, particularly by Adam Neale, in his work on 'Animate Contagions.'

Moreover, if this theory be accordant with many of the phenomena of cholera, so also are other suppositions which attribute more to chemical agency than to vitality in the production of the disease. The peculiar powers which modern chemistry is bringing to light are, to say the least, as probable agents as vital influences can be proved to be. It is now well understood that the quantity of a chemical agent is in no degree commensurate with its peculiar effects. A single globule of yeast excites fermentation in a liquid of a thousand times its bulk. A single grain of chloride of palladium injected into the veins produces, according to the interesting experiments of Dr. Blake, an extraordinary effect on the whole mass of blood. It is, perhaps, more in accordance with our present mode of thinking, to attribute epidemics to some chemical virus, which is in too small a quantity to be detected by our present means of research.

But we must leave this ingenious speculation of "insect propagation," and can only wish that we could justly give it some name, more honorable than that of "speculative." It is one of those points which lie on the outskirts of science, which is to be discussed and to be remembered, but not at present to be adopted. The science of medicine may be compared to a vast continent, on which have been founded numerous little colonies, complete in themselves, but surrounded by unknown and unproductive regions. Far off in the interior of the wilderness lie these obscure, though vast speculations; dimly we see them, but cannot discern whether they are real, or but visionary structures, similar to those which the bewildered traveller may sometimes witness, when before his eyes rise the lofty fanes and gorgeous temples of the desert mirage.

We shall conclude this article with a brief summary of the principal phenomena manifested by the cholera or its virus apart from the question of contagion. An immense deal of information has been accumulated on this head, of which we shall only extract the points most certain and most important.

We may review this subject under three heads—1, the virus itself; 2, the state of the media favoring its transmission; 3, the state of the system favoring its reception. We still follow Dr. Holland, in his 'Notes and Reflections' (p. 277), in believing that the whole subject may be comprised under these three heads.

I. *The virus itself.* The first and most striking fact respecting the choleraic virus lies immediately upon the surface. This is, its gradual extension over the face of almost the whole globe. From the moist and level surface of Bengal it passed towards the high table land of Central Asia, the steppes of Tartary, the luxuriant and antique forests of Burmah, and the hot and burning countries of the peninsula of Hindostan. Along the defiles of the Hindoo khosh, and the lower plateau of the Himalaya, across the plains of Persia, and the sandy and untracked deserts of Arabia, it pursued its singular and erratic progress. Penetrating into Russia, it passed gradually over the face of Christendom, unchecked by the cordons and barricades of European civilization. The extent of its ravages is marked by the fact that in almost every language it is named, and, except in Australia and the remote islands of the South Seas, it is known and dreaded by every branch of the great human family.

The consideration of this diffusion is a strong argument in favour of its propagation by human intercourse: but it is only an argument, not a proof. Observers who saw it in India in its outset, such as Mr. Orton and Dr. Russell, and found no evidence of contagion, reversed this opinion after the epidemic of 1830-2. Many indeed, consider this argument incontrovertible. They ask what property it was that could enable cholera thus to traverse nations so diverse and countries so various? What condition in common had the Hindoo and the Englishman, the Chinaman and the American, the Mongolian and the Caucasian? What condition in common had the climates of tropical Asia and of arctic Siberia, the burning regions of the dark Hindoo and the cold and northern latitudes of the Saxon and the Celt? The explanation which most easily and naturally occurs is, that the disease is independent of all climates and of all vicis-

situdes; that it is not bound to the soil, nor circumscribed by the limits of the winds, but that, from the living frames which testify its presence, it derives at once its origin and its progress.

Next to the universality of its prevalence, the most striking fact about the agent is its peculiar and irregular mode of spreading. It differs in this from every other known disease. Influenza approaches nearest to it, but there are important differences in their mode of diffusion. Both alike seem to be uninfluenced by temperature, and to prevail equally in all climates. But influenza travels much more rapidly, is more like an atmospheric disease, is not so bound to the soil, and in the intervals of its occurrence seems completely to disappear and not to manifest itself in certain places, as cholera has continued to do since 1817; or, if influenza does not altogether disappear, its form becomes modified. Cholera, on the contrary, creeps along, selects a certain tract of country, and shows an indisposition to traverse seas, and even rivers, although it selects the banks of these, while influenza has even been known suddenly to attack a ship's crew in the middle of the Atlantic.

Dr. Kennedy calls attention to the similarity of many of the phenomena of yellow fever to cholera. He says: "Its characteristics (yellow fever) as an epidemic seem in some respects so parallel with cholera, that almost the very same terms may describe both, excepting, indeed, in the grand distinction of contagiousness." (Hartley Kennedy, p. 79.) The alliance of cholera with remittent fever, in point even of symptoms, has been noticed by many writers, and these analogies and resemblances, vague as they are, ought certainly not to be disregarded, as they may point out hereafter the mode of investigation necessary to be pursued.

A great peculiarity about the choleraic virus is, its gradual development and exhaustion. Thus it enters a place and, something in the manner of plague, attacks a few of the most predisposed individuals; then increasing, it rapidly reaches its acme, and then declines. Mr. James Kennedy attributed to it a kind of monthly course; and Mr. Orton and others have fancied the variations were dependent on lunar changes. Mr. Scott's examination, however, does not countenance this.

Dr. Lorimer adds another example to this curious peculiarity:

"The average duration of a severe epidemic outbreak of cholera is under four weeks, and the period of its greatest virulence is limited to about one third part of the whole time of its continuance, or about nine days, during which time nearly one half of the attacks are fatal, while, during the whole period of the epidemic, the deaths amount to one third of the seizures........The virulence of the epidemic is confined between the fourth or fifth and fifteenth day of the epidemic." (Lorimer, p. 10.)

It must be remembered, however, that Dr. Lorimer is speaking only of marches. In cantonments the attacks are generally shorter than this.

It cannot be said, however, that this course is uniform. Sometimes it attacks quite suddenly, and in a way almost inexplicable. We have known, in a cantonment perfectly free from the disease, ten men of the same regiment to be attacked in a single night, and every one of these cases to be fatal, while neither in this or the other regiments at the same station did any subsequent case occur at this time. In the Marquis of Hastings' camp on the banks of the Sinde, in 1817, it reached its acme on the second day.

In this development and decline, cholera seems to have some singular analogies both to plague and epidemic smallpox.

It is this fact also which has given some countenance to the opinion before referred to, that the development of animal life comes nearest to the spread of the choleraic agent.

The cessation of cholera has been attributed by some to the exhaustion of those predisposed to the attack. One fact seems to countenance this, viz. that troops have entered a camp which cholera has just left, and have then begun to suffer severely from it, while those who were previously in camp have experienced no return. From this absence of predisposition is also perhaps to be explained the exemption one corps will exhibit while marching with another corps which is infected.

That the choleraic agent is highly diffusible and susceptible of alteration is proved by a multitude of observations. Sudden atmospheric changes often destroy it: as often announce its appearance. The Indian records abound in instances of this kind, and to these and to Mr. Orton's work we refer for examples.

II. *Condition of the media favoring the development of the agent.* Although our knowledge of the choleraic virus is so limited, it is not so with the conditions which favour its diffusion. These things are cognizable by our senses, and, accordingly, an immense mass of facts has been collected, which gives us information of the highest importance.

These are contained chiefly in the excellent and truly scientific Report from Bengal, edited by Mr. Jameson, in the 'Madras Report,' which presents us with a great accumulation of facts, and in the work of Mr. Orton. Dr. Lorimer's Report also adds to our knowledge on this subject.

We may consider this subject with reference to—1, the soil; and 2, the atmosphere.

1. It is an universal remark that low, confined, and particularly marshy places suffer more frequently and more severely. This has been the case both in India and in Europe. High and airy situations are visited more seldom and with less severity.

The account which Mr. Jameson gives of the encamping ground of the Bengal army in 1817, affords us, in a few words, the characteristics of the soils which have been found most favorable for the propagation of the virus.

"In the ground of encampment in which the disease prevailed most, the soil was low and moist; the water was foul, stagnant, and of brackish quality, and everywhere not more than two or three feet from the surface of the earth, and the vicinity abounded in animal and vegetable putrified matter; whereas, at Erich, where the army regained its health, the situation was high and salubrious, and the water clear and pure from a running stream." (Bengal Report, p. 106.)

The banks of rivers seem also efficient in promoting the spread of the disease. Its peculiar attachment to rivers, and the way in which it spreads along them, independent, it may be, of human intercourse (Orton and others), is indeed a very peculiar feature, and one almost distinctive of cholera. It is not the mere moisture which aids the development, as the poison often remains close to the bank, and does not attack the crews of ships lying at some distance. Or it passes along one bank without touching

the other, although free intercourse goes on, and then, suddenly crossing the river, traverses in an opposite direction the bank hitherto untouched.

In entering a town cholera almost always attacks the lowest and dampest quarter. This was universally the case in India. It was the case also in Persia (M'Cormack, in Medico-Chirurgical Transactions, vol. xii); in Russia, Hungary, Prussia, &c. (Lichstenstadt, Becker, De Boismont, Boisseau, Hamett, Russell and Barry, &c.); in England (Craigie, Brown, Hazlewood and Mordey, and numerous others); and, in fact, this observation is universal.

It does not appear, however, that jungly or marshy country by itself is always the most favorable for development. Dr. Lorimer makes the following remarks on this subject:

"With regard to the nature of the country passed over; this, of course, has been very various, hilly, jungly, and open; but the encamping ground, with few exceptions, has been described as good and open. In connexion with this point the following returns, taken from the tabular statement on the accompanying map, relative to the nature of the country and soil at the place of attack, are given. They show that the greatest number of attacks have occurred on cotton soil, and what is described as open country. Further, it was thought proper to direct attention to the distance of the nearest river from the spot of outbreak, and the results are here briefly but clearly exhibited; 49, or nearly one third of the whole number, have occurred on the banks of rivers, and 82, or fully one half, within five miles." (Report, p. 6.)

Of 121 epidemic attacks, 60 occurred on the "black, or cotton soil," 46 on the "red soil," and 15 on "various soils." (Ibid. p. 6.) In the same 121 cases, 99 occurred in country described as "open," 15 in "jungly country," and 7 in "hilly districts." (Ibid. p. 7.)

It does not follow, however, that cholera will not ascend hills, although these decidedly enjoy a much greater immunity than the plains. For the first three years in India it was repelled by the Himalayas. It afterwards attacked the towns on the lower plateau, and it was a singular circumstance, which after observation has confirmed, that the per centage of attacks to the number of inhabitants was much less than in the lower regions, while the fatality was exceedingly great. (Calcutta Transactions.) It seemed, indeed, as if only those particularly predisposed were attacked, and the majority of these died. We are not aware that cholera has been observed at an altitude of more than 6000 feet above the level of the sea.

Dry sandy soils are unfavorable to its development. This was witnessed both in India and in Arabia. They are not, however, exempt.

That an effect is produced on the virus by emanations from the soil, independent in whole or in part of atmospheric states, is evidenced by the sudden change which sometimes occurs in the choleraic virus by changing the ground of an infected army or battalion.

The army of the Marquis of Hastings had not retreated fifty miles from the banks of the Sinde river before the disease left them. A portion of the army in Burmah being attacked with cholera in 1825, moved to higher ground, only a mile and a half distant, after which not another case occurred. (Orton.) Here they could not have changed the atmospheric condition. Either ascending or descending hills has often checked cho-

lera in regiments. Many examples of this are given both by Jameson and Scott.

Dr. Lorimer remarks:

"In 63 instances, or fully one half of the epidemic outbreaks, it is recorded that the disease continued to the end of the march, ceasing gradually: in 22 instances it is stated to have disappeared on change of weather, or after heavy rain; in 11, after crossing a river; while in 9 it is distinctly recorded that the disease not only continued, but became aggravated, both in frequency and severity, after crossing a river; in 5 instances the disease disappeared on the regiment being encamped on high ground. In the remaining 10 the required information on this point is not recorded." (Report, p. 11.)*

It must be confessed, however, that our knowledge of the effect of soil is limited and of the most general kind. It remains to be seen whether geological features have any influence upon it. That these affect remittent fevers is now known from the observations of Mr. Heyne on the hill forts of India that are subject to or exempt from fevers. It remains to be determined whether cholera is at all influenced by the same circumstances.

2. The conditions of the atmosphere in cholera attacks have been the subject of much discussion, and some have even supposed that the disease is altogether irrespective of them. This is, however, decidedly erroneous, and contradicted by all observations on the subject.

To enter into this extensive subject would exceed our limits; we shall merely indicate the most prominent facts. We refer our readers to the 'Madras Reports' and to Mr. Orton's excellent work, and more particularly to the 'Bengal Report,' pages 149 to 165, and pages 106 to 122, where is recorded an immense mass of evidence on the effects of low, damp situations, filth, drains, stagnant water, rivers, and changes of temperature, moisture, barometrical changes, on the diffusion of the disease.

It may be true that the choleraic virus will prevail independent of these, but that it is influenced by them it is impossible to doubt.

Temperature by itself seems to have little effect on the virus: neither the heat of Indian or the cold of the Russian winter retard its propagation. It also prevails in India in the cold and the hot months. Sudden changes of temperature often attend its outbreak, but then other causes are generally concerned in the production of these, which may have an effect on the virus.

The humidity of the atmosphere has attracted a great deal of attention. The outbreak of the disease in the provinces of Chittagong, Behar, Dhacca, and Sylhet was preceded and attended by unusual floods in Bengal (Jameson's Report, Introduction, pp. 68 et seq.; Tytler, on the Morbus Oryzeus.) The seasons in 1816 and 1817 were indeed so remarkable, as to have attracted the notice of every one, and to "have formed the topic of common conversation." (Jameson.) During the progress through

* The virus seems, indeed, almost to adhere to the soil. It seems to prefer travelling overland. Thus, in 1818 it passed down to Palamcottah, at the very end of the peninsula, before it reached Ceylon, although between Calcutta, Madras, &c., and Trincomalee there was constant communication. So also it took five months (three months—Orton) to pass from Masulipatam to Madras, and crept during this time very slowly along the land, while there is a constant passage of native boats between the two places, and the passage only occupies ten days.

India the outbreak of the disease in almost every place was attended by phenomena similar to those which attended its primary appearance. (Orton.) Orton traces this progress with great care. (pp. 168 et seq., 2d edition.) In Europe the same thing was noticed by several observers. Jannichen of Dresden decided that the choleraic virus had a peculiar affinity for moisture. Baumgartner, on the other hand, could trace no hygrometrical alterations during its prevalence. It broke out in Russia in one or two places when the thermometer was—20° Fah., and when the air must have been nearly dry.

In India it sometimes appears at the commencement of the monsoon; occasionally, on the other hand, a very heavy fall of rain checks it. Messrs. Jameson and Scott remark that troops marching in dry and cool months enjoy a considerable immunity from the disease.

Dr. Lorimer remarks, that "in the cool and dry, and hot and dry weather the ratio of attacks on the march is nearly the same, and in the cold and rainy, and hot and rainy weather the ratio also corresponds to an unit, though it will be seen, on reference to the tabular statement, that in rainy (whether cold or hot) weather the ratio of attacks is nearly doubled." (Report, p. 5.)

From the table (p. 6) it appears the per centage of attacks to marches per cent. is, in cool and dry weather, 17·54; in hot and dry weather, 19·11; in cool and rainy weather, 35·95; in hot and rainy weather, 35·84; and in variable weather (that is, "cold, hot, and rainy"), no less than 68·42.

In the 'Madras Reports' (published in 1824) the kind of weather is often described as cloudy, close, sultry, with drops of rain, or occasional showers. Mr. Orton says, there is no instance known to him of it breaking out epidemically during the early years of the epidemic "in the serene or settled weather." (Orton, p. 184.) It would appear, however, that it has often been present in weather which is described as "exceedingly pleasant," although this might be from occasional showers lowering the temperature.

That moisture *per se* is not powerful in spreading the disease may be presumed from the retardation of the virus by seas and broad rivers, but it does not follow from this that moisture may not be one of the conditions which is necessary to constitute the peculiar disposition of the air necessary for the rapid development. It is certainly in this direction that we look for some probable elucidation of the unknown laws of the choleraic virus.

The winds retard and aid the passage of the agent. It is a common, though an erroneous notion, that cholera generally passed against the wind. This is true only to a certain extent: a strong adverse wind always delayed its progress, although it did not prevent it.

This may be instanced by the case we have already cited of its first passage from Masulipatam and Madras. From Masulipatam it passed very slowly in the teeth of the wind; but from Madras, the north-east monsoon setting in, it acquired a great velocity, and rapidly reached the towns and districts to the south, although, on account of the heavy rain, intercourse was interrupted and at times quite arrested.

The idea that it travelled only westward is now known to be a mistake. At the time it was spreading from Bengal towards Bombay, it was also passing easterly through Tartary and Burmah. Of late years it has constantly passed from Hindostan, through and across Burmah, to Siam and China.

Mr. Jameson, remarking that the disease at its first origin had a singular tendency to travel westward, says: "Upon reference to the various reports of the rise of the disorder in different parts of the country, it was discovered that, in a vast majority of instances, the wind was blowing from the east and south-east at the time of its breaking out." (Bengal Report, p. 98.)

Under this head, viz. alterations in the media favoring the transmission of the agent, we are disposed to put the well-known observations of the predilection of cholera for all places where human beings are thickly crowded together. This was particularly insisted upon by Jameson, in a passage extracted by Dr. Copland. The emanations and effluvia from a crowded camp or city seem to give the virus its conditions of existence in a high degree. It is generally supposed that this is confined to large assemblages of men. Dr. Lorimer, however, is inclined to think that our view of this point should be enlarged. During marches, even when the force does not exceed a company or two, he finds "the ratio of attacks decidedly greater as the strength of the regiment increases." (p. 4.) It seems to increase, even with these small numbers, in a very regular manner. Thus it appears that the ratio of attacks to strength per thousand are as follows: for 100 to 300 men = 61·616; 300 to 500 men = 68·085; 500 to 700 men = 58·117; 700 to 900 men = 89·009; 900 to 1100 men = 86·212; 1100 to 1534 men = 132·151. (p. 38.) Treasure parties, which are small, are seldom attacked: out of 352 marches cholera appeared 8 times. (p. 4.)

This is a most important observation, inasmuch as, in ordinary marches in quiet districts, a remedy can easily be found by dividing the troops into small bodies, as is recommended by Dr. Lorimer in his Report.

The electrical conditions of the atmosphere have been supposed to exert an influence over cholera. We might certainly anticipate some influence, from the universal diffusion of electricity and its high importance in the economy of Nature, but at present this is mere conjecture. Cholera is said to prevail both when the atmosphere is negatively and positively electrical, so that we have not even the slightest observation in favour of the presumed influence of this agency.

Dr. Prout noticed an increase in the weight of the atmosphere during the prevalence of cholera. This observation at present stands alone.

III. *Condition of the individual favoring the reception of the agent.* Certain pre-existing diseases seem to give a great predisposition for the reception of the choleraic virus. A peculiar kind of watery diarrhea is sometimes seen to prevail in a place where it is raging, and in many cases on this cholerine, as it has been termed, true cholera supervenes. Many suppose this to be the first stage of the disease. Common tropical dysentery does not appear to predispose to it, in any great degree, as there are often many dysenteric cases in hospital, when a regiment is attacked with cholera, and there is no unusual prevalence among them.

Some diseases of the lungs have been supposed occasionally to have a predisposing influence, particularly chronic bronchitis, and pulmonary emphysema, but the observations on this point are not conclusive.

The action of mercury affords no immunity; many examples are recorded in the Bombay and Madras Reports, and in later publications, of patients freely salivated being attacked.

Debility and impaired health from any cause are predisposing circumstances; but it cannot be denied that many men are attacked in the plenitude of health and strength.

The influence of marching, in India, was much insisted on by Orton, who devotes a chapter to the special consideration of this subject. His conclusions are completely borne out by Dr. Lorimer, whose tables are very conclusive. The ratio of attacks to marches per cent., in all branches of the Madras native service, is as follows. Under 200 miles the ratio of attacks is 8·52 per cent.; under 400 miles it is 33·94 per cent.; under 600 miles it is 33·33 per cent.; under 800 miles it is 46·66 per cent.; under 1252 miles it is 75·00 per cent. Or the marches being computed by days, the ratio of attacks to marches per cent. stands thus: under 20 days = 8·029 per cent.; under 40 days = 17·762; under 60 days = 20·232; under 80 days = 40·540; under 100 days = 44·446; under 120 days = 50·000; over 120 days =50·000.

The extraordinary predisposition which marching induces is marked by the fact, that at the end of the third month the attacks of cholera to all the marches performed by the Madras native infantry were 61 per cent.

It has been generally supposed that cavalry suffer less on the march than infantry, from their less exposure and fatigue. This is probably correct; although Dr. Lorimer's tables make the proportion rather less than is commonly given.

According to this Report, for the whole number of marches, the respective corps of the Madras native service, give the following ratio of attacks, to marches, per cent. Among the infantry, 24·688 per cent.; among the cavalry, 20·270 per cent.; among the artillery, 50·000 per cent.; and among the sappers and miners, only 11·764 per cent. The returns from the artillery and the sappers and miners have only been available for the last few years. Dr. Lorimer, to account for the singular exemption of the sappers, mentions that they are, from the nature of their service, inured to the weather, being frequently under canvass, and also being men of low caste, they are not so restricted in their food.

The analogy between cholera, fever, and bowel complaints has led to the belief that these diseases prevail together, and certainly in many cases remittent fevers and dysentery prevail subsequent to an epidemic attack of cholera. Dr. Lorimer thinks, however, there is no uniform connexion between cholera, fever, and bowel complaints. His tables are certainly very full on this point, and seem satisfactory. It may not be uninteresting to give the whole sickness among the Madras native army since 1821. The aggregate number of men during this period has been 1,655,236; of cholera there have been admitted 22,347, and have died 8836 cases; of fevers there have been admitted 420,201, and have died 5946 cases; of dysentery and diarrhea, there have been admitted 67,564, and have died

4098 cases; of all other diseases there have been admitted 548,796, and have died 11,258 cases.

The ratio of deaths to strength per 1000 per annum, drawn from an average of the same period, are of cholera 5·3382; of fevers 3·5922; of dysentery and diarrhea 2·4751; of all other diseases 6·8014. The average deaths per 1000 per annum are, therefore, 18·2069, which is considerably below the mortality among the European troops, if cholera be included in the calculation.

At page 36 of this Report is a table showing the comparative admissions and deaths from cholera among the Madras native troops on the line of march and in cantonments. The ratio of admissions to strength per 1000 are for the line of march 20·9230, and for cantonments 8·5068; the ratio of deaths to strength per 1000 are for the line of march 8·6363, and for cantonments 3·2700; the ratio of deaths to admissions per 1000 are for the line of march 412·7698, and for cantonments 384·4074.

Dr. Lorimer's return evidently points out the three grand rules which ought to be attended to in all marches in India, viz. to shorten the marches as much as possible; to make them in as small bodies as possible; and, whenever it is practicable, to choose the dry months for the time of moving.

Although neither the conditions of the soil or of the atmosphere, or the state of the individuals who happen to be exposed to it, have anything more to do with cholera than as auxiliaries to its development or its reception, it cannot be doubted that the study of these phenomena, as it is the easiest, so also it is the most important part of the investigation of cholera.

In this direction only can we at present look for any practical results, and we receive Dr. Lorimer's able Report with great satisfaction as the first of a series which will place these matters on a certain basis. We think, however, that future reports should be more minute than this could possibly have been, as the returns which furnish it being retrospective are necessarily incomplete. We think if an extensive series of investigations were set on foot over the whole of India, inquiring minutely into all the attending circumstances of each attack of cholera, the results would in very few years be most important. The conditions of existence of the virus, and the predisposing circumstances being known, the prophylactic measures might be able to arrest its progress, and to limit its ravages. To accomplish this, would be a triumph worthy of that great Company, whose subjects and whose servants are alike the victims of this fatal and incurable disease.

The increased prevalence of cholera in India, since 1841, and its recent advance into Persia, has naturally given rise to the opinion that this disease may be expected again to travel in its old footsteps towards the colder countries of the northern and the western world. And there seem good grounds for this opinion. But if such an occurrence should take place, it is possible that at the expense of present suffering, great benefit would result to the whole human race. We would fain hope that if cholera should again traverse Europe, the laws of its diffusion might be more surely indicated, its true nature better illustrated, and a more successful treatment discovered.

Admitting this possible invasion to be probable, it would be the part of the

European governments to endeavour to institute so minute an inquiry into the conditions of the media favoring the spread of the disease, that nothing should be left unnoticed. This can only be done by combined effort working in one direction; it ought not to be left to individual effort, which is partial, limited, and therefore comparatively unproductive. From the moment cholera appeared in Europe, government should combine all classes of observers, and call in the aid of every science. The chemists and the meteorologists, the geologists and the physicians, should all be called upon to supply their observations, independent indeed, but all essential. Facts of all kinds should be noted without regard to opinion, and till the close of the epidemic no premature generalization should be attempted.

Some rules, simple indeed, yet not unimportant, may be deduced from our present knowledge, as applicable in the event of another invasion of the disease.

1. As there is no valid evidence of their use, quarantines, cordons, and other general measures of seclusion may be properly abandoned.

2. At the same time, all unnecessary exposure of patients, their excretions, clothing, &c., in situations frequented by other persons, and all direct intercourse between the sick and the well, not necessary for the comfort, nursing, and proper medical treatment of the patients, should be avoided as a measure of wise precaution, and on the ground of possible evil. But no valid evidence exists for justifying the neglect of the sick, in any the least degree, by their relatives or attendants, on the ground of personal danger: we do not contemplate the possibility of neglect by the medical profession, under any considerations whatsoever, and therefore do not name this.

3. As dampness and vegetable and animal effluvia seem powerful agents in spreading the virus, the dwellings of the poor should not only be made as dry and airy as possible by draining, ventilation, and whitewashing, &c., but government ought to take into consideration the site of dwelling-houses, and should compel the builder not only to follow certain rules in the construction of his tenements, as is done now by the Building Act, but should also compel him to choose a proper and dry locality, or to take means to render it so.

4. The cholera hospitals should be removed from the low and damp parts of the town where the disease chiefly prevails, and should be on dry soil, as high as possible, and built with even a freer circulation of air than other hospitals.

5. In some places, considering the peculiar way cholera is limited by a street or road, apparently from some attractive force exerted towards it by certain soils or other local influence, it might be advisable to effect, if such a thing were possible, the removal of the inhabitants from a part of the town severely affected with the complaint.

6. The profession should endeavour to discover some certain method of diagnosis, by which the true epidemic might be distinguished from those pseudo-choleraic cases which accompany it, and which lead to wrong notions of the nature of the disease, and erroneous methods of treatment.

7. All classes should guard against the predisposition which seems essential to the action of the agent. This is increased by disorders of the

digestive organs, particularly by watery diarrhea; and by all means physical or mental which lower the strength and diminish the tone of the system. For instance, the apprehension of the disease is known to predispose to it in a very singular way. Dr. Lorimer mentions a native regiment which has been fortunate enough to have escaped cholera up to the present date. The commanding officer attributes this to the custom prevalent in the corps of offering up sacrifices previous to a march; the confidence the men have that cholera will afterwards not attack them seems certainly to have been a protective influence. The increased number of attacks of cholera which take place at night seem to show that at this time the system is peculiarly liable to the action of the agent.

Our space admonishes us that we must take leave of our subject and the authors from whose pages we have drawn much and valuable instruction. Although we differ considerably from Dr. Kennedy, we have derived great pleasure from a perusal of his work, which is evidently the composition of an able physician and a clever man. Of Dr. Lorimer's excellent Report we have already spoken; and Dr. Copland's great and deserved reputation is sufficient to warrant that his article on Cholera, is written with his usual tact and ability, although we have not hesitated to dissent from the confident opinion he has formed of the contagion of the disease. We think that, on this point, he is not merely wrong, but has allowed his prepossessions to blind him to obvious truths, and even to disregard palpable facts.

Art. II.

Traité des Syphilides, ou Maladies Vénériennes de la Peau, précédé de Considérations sur les Syphilis, son Origine, sa Nature, &c. Accompagné d'un Atlas in-folio, contenant douze Planches dessinées d'après Nature, gravées et coloriées. Par Alph. Cazenave, Médecin de l'Hôpital Saint Louis, &c. &c.—*Paris,* 1844.

A Treatise on the Syphilitic Eruptions of the Skin, preceded by general Remarks on the Nature and Origin of Syphilis; accompanied by an Atlas of twelve Plates, coloured and engraved, and drawn from Nature. By Alph. Cazenave, Physician to the Hospital of Saint Louis, &c. &c. —*Paris,* 1844. 8vo, pp. 630.

It has been aptly observed by a French writer, that we might frequently economise time and trouble, by satisfying ourselves on two points, before undertaking the perusal of a new medical work. The facts to be ascertained are these:—Has the author had sufficient opportunity for studying the subject which he has undertaken to describe, and if so, was he mentally qualified for the task? If we test by this ordeal the claims on our attention of the author, whose work we are about to review, we shall find that they are worthy of our best consideration.

Biett was the first to describe separately the syphilitic eruptions, and to draw the attention of medical men to that interesting class of diseases. In fact, he made their study peculiarly his own, and, although he never published a distinct work on the subject, he promulgated his views through

the medium of lectures, which were afterwards published by his pupil, M. Cazenave, in his excellent 'Manual of Diseases of the Skin.' M. Cazenave enjoyed the friendship of Biett for nearly twenty years, and has been attached to the Hospital of St. Louis for the same period, in different capacities; hence we may view the 'Traité des Syphilides' in a great measure as the expression of Biett's opinions on the venereal eruptions. Indeed, M. Cazenave states it was originally intended that this work should be a joint production, and laments that a long illness, and subsequently death, deprived him of the valuable co-operation of so sound and conscientious an observer as his late friend and master, Biett. But, if we have occasion to regret that the eminent dermatologist who had the care of the Hospital of St. Louis for a long series of years has not himself written on a subject which he understood so well, we have the satisfaction to know that the work of his pupil and successor, M. Cazenave, contains a faithful exposition of his views and extensive experience in this branch of dermal pathology.

M. Cazenave's work is divided into two parts. The first is occupied with the history and general pathology of syphilis; and the second contains an elaborate account, illustrated with cases and coloured plates, of the various syphilitic eruptions. It is with the latter division that we shall be chiefly occupied in the present article, but as some of the author's views on the primary disease are peculiar, it is proper that the reader should be made acquainted with them in the first instance.

I. PRIMARY SYPHILIS.

The author sets out with the following propositions, which he considers are supported by the past history of syphilis, and by the observations of himself and others: 1. There is a syphilitic disease. 2. It has existed from the remotest antiquity. 3. It is the result of a special poison which infects the system; there is but one virus. 4. Syphilis is contagious. 5. It is hereditary. 6. It is announced by various primary symptoms; it occasions secondary symptoms. 7. Mercury is still the best remedy that can be employed in the treatment of that disease. It would be foreign to our present purpose to follow M. Cazenave through all the *questiones vexatio* arising out of the foregoing propositions; but it is due to the author to state a few of his arguments in favour, of the antiquity of the disease, of one virus, and of the mercurial treatment.

I. The author considers that syphilis is not a disease of modern origin, and essays to prove that proposition from the works of the earliest writers —from the Leviticus of Moses down to the writers at the close of the fifteenth century, the period which has generally got the credit of being distinguished by the birth of the *morbus gallicus*.

Under the generic name of gonorrhœa, says M. Cazenave, the ancients have confounded all manner of venereal infection, and hence has arisen the idea that the syphilitic disease, according to the general acceptation of that term, was altogether unknown to them. Hippocrates, in describing the *morbus femineus* of the Scythians, mentions the occurrence of ulcers of the pudenda. Celsus recommends (De Medicina, l. vi, cap. 18) the operation of phymosis in a case of a rebellious chancre on the corona. Galen describes buboes and ulcerations resulting from disease of the genital organs.

Oribasius, Ætius, Paulus Ægineta, and several other writers of the same period, describe the disease more distinctly; but the question of antiquity is not solely dependent on medical authority for support. It is further corroborated by historical writers, as for example by Pliny and Josephus. The former relates the case of a woman who was drowned in the lake of Como, because her husband was attacked in his private parts with an incurable disease (*maritus ex diutino morbo circà velanda corporis ulceribus putrescebat*), and Josephus, in describing the death of Herod, states that the genitals were in a state of putrefaction, (*ipsa quoque verenda putrefacta scatebant.*) With regard to the secondary symptoms, or syphilitic eruptions, M. Cazenave considers that they were confounded with lepra, elephantiasis, mentagra, lichen, &c., a mistake which, by the by, is by no means uncommon even at the present day. The author also quotes several of the Arabian writers in support of his views, and then proceeds to discuss the famous epidemic of the fifteenth century, and the supposed American origin of the disease.

The author alleges that the epidemic of 1494 was not syphilis, nor did syphilis, or that epidemic, originate in America. The disease which ravaged Europe towards the close of the fifteenth century, was one of those plagues which history tells us has visited the world from time to time in all ages. It was of a typhoid and not syphilitic character; but the frightful immorality of the time, and its results, and a remarkable similarity between certain symptoms of the epidemic and of the venereal disease, will, according to M. Cazenave, account for the erroneous views regarding their identity promulgated by the writers of that period.* At the time alluded to, both diseases evidently coexisted; and the symptoms of the syphilitic malady acquired a violent intensity under the influence of the epidemic; but if the former were merely the result, or continuation of the latter, as alleged, "how is it," asks the author, "that on the cessation of the epidemic, the venereal disease continued to exist independent of the former, of which it was, in point of fact, a terrible auxiliary, but not a consequence?" M. Cazenave regards the notion of the American origin of syphilis as a fable invented by a Spanish writer, Oviedo, in 1518, nearly twenty years after the commencement of the epidemic, in order to justify or palliate the frightful crimes committed by the Spaniards in America, and which excited the indignation of Europe. In support of this view he cites a host of authors, and some interesting cases; but we must refer the reader to the original work for the details.

II. M. Cazenave is decidedly opposed to the doctrine of a double virus as regards the venereal diseases, or, in other words, he holds the opinion that chancre and gonorrhœa are results of the same specific poison.

"Being placed under favorable circumstances," says the author, "for observing the development of the secondary symptoms of syphilis, I am in a position to state that in a great number of cases, the general infection of the system has originated in a gonorrhœa (blennorrhœa); and if my opinion, with the proofs which I shall adduce by and by, shall be deemed insufficient for establishing this fact, I may

* M. Heusinger states, in his remarkable work, 'Recherches de Pathologie Comparée,' "that most of our domestic animals are subject to contagious discharges and ulceration of the generative organs; they are transmissible by coitus, and exhibit a resemblance to syphilitic diseases. In the opinion of M. Heusinger, they are nearly in the same condition as the syphilitic affections of man were before the close of the fifteenth century."—Rev.

invoke the testimony of M. Baumes, who asserts that syphilitic diseases of the skin are very frequently the result of a blennorrhagic discharge, and altogether independent of syphilis. I hope to be able to show that blennorrhagia, like chancre, may produce that morbid condition called constitutional syphilis."

M. Cazenave endeavours, with much earnestness of purpose, to establish this very unorthodox view of the theory of syphilis. He denies that gonorrhœa is the result of simple inflammation of the mucous membrane, and is at issue with the doctrine of Balfour, Duncan, and Benjamin Bell, who were the first supporters of the non-syphilitic character of the urethral discharge. He reprobates the unscientific and illogical argument so frequently put forth by the partisans of this doctrine, namely, that if a patient should contract syphilis from a female labouring under gonorrhœa, and apparently no other disease, still there must have been syphilitic virus present, *however it might have been concealed,* or else the patient could not have contracted that disease. This, the author alleges, is an easy way of answering opponents, but will not bear the test of scrutiny.

The "*chancre larvé*" of M. Ricord comes under this category, and is pronounced by M. Cazenave to be a sophistical figure of speech, a loophole for retreat when obliged to explain how it sometimes happens that inoculation with the muco-purulent matter of blennorrhagia produces chancre.

The author ridicules M. Ricord's idea of a "visionary" chancre, by which he can account for those cases in which syphilitic sores are produced by contact with gonorrhœal matter, and also his *post hoc propter hoc* conclusion that if inoculation with matter supposed to be syphilitic does not produce a chancre, the virus was not venereal; but if it did succeed, it must have been genuine syphilitic pus.

We freely admit that the doctrines of M. Ricord are open to objection on many points, that his inferences are not always correct, nor his analogies complete; but however plausible the counter-statement put forth by M. Cazenave may be, it does not appear to us to be conclusive of the validity of his own theory of a single virus. But it is only fair to let our author explain in his own terms the unity of the syphilitic poison. Nothing is more common, says the author, than to meet with cases where a man has taken syphilis from a female, who upon examination proved to have no chancre, but merely a muco-purulent discharge,—at least M. Cazenave meets with such cases constantly. M. Ricord objects to this, that there was a chancre, but it was overlooked; a statement which is easier made than proved; and the author prefers to view this fact as proving that the muco-purulent matter of blennorrhagia may produce a chancre, rather than to adopt the sophism of his antagonist. In support of this view, he cites the authority of Hunter, the results of experiments conducted by M. Castelneau, and some even conducted by M. Ricord himself. M. Castelneau succeeded in producing the true syphilitic pustule from the matter of blennorrhagia, and M. Ricord succeeded in six cases at least in obtaining the same result. It is a fallacy to say that blennorrhagia is not syphilitic, because inoculation with its matter will not always produce like results, for there are other symptoms, the "pustule plate," for example, which cannot be inoculated, and it is not only a syphilitic symptom, but frequently a primary symptom. It is by no means uncommon to see two individuals infected from the same source, and yet one will have a gonorrhœa and the other a chancre.

But the advocates of a double virus, says the author, will occasionally have

need of a triple poison to explain some of their own views: for example: Vigarous relates a case in which six young men, intimately united by friendship, had connexion one after the other with the same woman, who infected all with the venereal disease. The first and fourth (according to the order in which they presented themselves for treatment) had chancres and buboes; the second and third gonorrhœa; of the last two, one had a chancre, and the other a single bubo. This girl must have had a triple virus, chancre, bubo, and gonorrhœa. But how shall we explain the singular fact that all these individuals, being placed in the same position, under circumstances alike favorable to all, were infected by different diseases? Why have they not all had chancres, buboes, and gonorrhœa? The partisans of the double virus will perhaps say that individual susceptibility and different poisonous matter may, in some sort, explain the problem.

M. Cazenave follows up his critical exposition of what he considers fallacious in M. Ricord's doctrine, with great energy, but at the same time without evincing any of that asperity or partisanship which too frequently disfigures and destroys the value of scientific discussion. Although he denies the truth of M. Ricord's axiom that chancre only can produce chancre, and that chancre is the only characteristic symptom of genuine syphilis, he does so by opposing facts against experiments, which are not infallible, and merely considers M. Ricord the victim of a theory which stakes the whole doctrine of syphilis on the point of the lancet.

"I am aware," says Cazenave, "that M. Ricord has made numerous experiments, that he has been able to try them at will, and that on the great arena where he has taken so prominent a position by his zeal and industry, and the *éclat* of his researches, he has been able to collect all the elements necessary for rational conviction. Nevertheless, I am compelled to say that he is deceived; he has only regarded syphilis in one of its phases, and, carried away by his love for experiments, he has forgotten that the theory of venereal infection can never be reduced within the limits of a mere experiment."

III. With regard to the general treatment of syphilis, our author is again at issue with M. Ricord. This practitioner, it is well known, considers syphilis to be a local disease at the commencement, and hence concludes, that if you can arrest it in this stage by cauterization or other means, you preserve the constitution from syphilitic taint. But M. Cazenave considers the postulate and its corollary to be equally erroneous, and the treatment by cautery irrational, if not injurious. The author does not found his objection on the common occurrence of bubo, swelled testicle, chordee, &c., after the sudden arrest of blennorrhagia, nor of a simple syphilitic sore being converted into a phagedenic or gangrenous ulcer by the imprudent use of the cautery. He opposes it on higher grounds: he views chancre and blennorrhagia as merely the symptoms, the expression of a general infection, and concludes that their disappearance is no proof of the eradication of the virus they represent. We might as well attempt to cure rabies by cauterizing the bitten part after the manifestation of the symptoms of hydrophobia. But the most injurious result likely to occur from M. Ricord's doctrine is that it may prevent the adoption of a rational and general treatment, by means of which the constitution may be relieved from syphilitic taint, and, consequently, all the secondary symptoms prevented.

M. Cazenave's experience goes to prove that the cauterizing treatment is decidedly favorable to the development of the sequelæ of syphilis, and especially of the venereal eruptions. The author does not advocate the use of mercury as a *specific* for syphilis; far from it; but he regards it as the best remedy we possess for enabling the constitution to rid itself of the general taint, and as a prophylactic against secondary symptoms. With regard to the accidents alleged to result from the administration of mercury, it is enough to say that these accidents occur to patients labouring under the disease who never took mercury, as proved by Cullerier, Biett, and Ricord; and, on the other hand, they are scarcely ever met with in individuals who have been under the influence of that remedy, but who have not had syphilis;—a fact established by the joint researches of Biett and the author. M. Cazenave selected 153 syphilitic patients, whom he observed with great care, and took every precaution to avoid deceit. These observations have led him to the following conclusions:

"Mercury is not a specific for syphilis; and however carefully it may be administered, it will not always prevent a relapse; the primary symptoms of the disease may disappear under the influence of an antiphlogistic treatment, but the mercurial treatment is the most powerful means we possess for aiding a constitutional reaction against the syphilitic virus."

II.—SYPHILITIC ERUPTIONS.

Although the venereal disease appears to have first shown itself in Europe under the form of cutaneous eruptions, and the earliest writers on syphilis confined their descriptions of that complaint to a pustular affection of the skin; still, until the early part of the nineteenth century there was no attempt made to arrange and classify the syphilitic cutaneous diseases under a distinct head. However, about that period these eruptions were grouped together for the first time, and described under the name of *syphilides;* but as this classification was formed without any reference to the elementary characters of the diseases, distinct varieties were confounded together, and species established on characters that were altogether secondary and insignificant. Such was the history of the syphilides when Biett first turned his attention to the subject, and his admirable and unrivalled essay, published in M. Cazenave's Manual, bears testimony to the value and importance of his researches into the history of that interesting class of diseases.

From the close of the fifteenth to the beginning of the nineteenth century the syphilitic eruptions were described under the vague general term of *pustules*, and even the writings of the learned Astruc, John Hunter, and B. Bell are characterized by the same vagueness of description as those of the authors who described the venereal eruptions of 1493. Alibert was the first to substitute the more correct and significant term "*syphilide*" for that of pustule, the propriety of which Biett at once recognized, and subsequently adopted it in his lectures on the syphilitic eruptions. Moreover, this sound practitioner saw the necessity of studying this group of diseases apart, according to their different forms, their relation with the primary symptoms, and the actual state of the patient at the time; and following out this view, he classified the syphilides according to their elementary lesions, and established with a clearness and

precision hitherto unknown, the striking and well-defined characters of the syphilitic exanthemata, the syphilitic vesiculæ, the syphilitic pustulæ, the syphilitic tuberculæ, papulæ, and squamæ. Under these six groups every variety is included, and each variety has its own distinct description.

These are the views promulgated by M. Cazenave in the work now under consideration.

By the term *syphilide* the author means every cutaneous eruption developed under the influence of the venereal virus, and characterized by the elementary lesions of the simple eruptions, but possessing a certain specific physiognomy, which is altogether unique. The syphilides are primary or consecutive. In the first instance they accompany chancre or blennorrhagia, or, as more frequently happens, immediately succeed the sudden removal of these lesions by cauterization or other revulsives. But the *consecutive* syphilides are by far the most frequent. These manifest themselves after the total disappearance of the primary symptoms, and long after the patient fancies himself free from all syphilitic taint. They invariably assume a chronic character, and present three different orders of symptoms, which may be described as—1, symptoms which are *common* to all varieties; 2, those which are *special;* and, 3, *concurrent* symptoms.

Common symptoms. Amongst the various symptoms which—if we may use the term—individualize the syphilides, the *colour* of the eruption, its form, the tint of the adjoining skin, and the nature of the squamæ, scabs, and cicatrices are the most remarkable. All the syphilitic eruptions are stamped by a peculiar tint or colour, unique of its kind, more or less distinct according to circumstances, but the constant and invariable attendant of every variety, whatever may be its form. It is extremely difficult to give a precise definition of this colour, and even Fallopius, who points out the importance of this symptom, in a diagnostic point of view, admits the difficulty, for he says, "*Color non potest explicari, non enim est ruber, non albus, non pallidus.*" This colour, then, which it is impossible to define by a single term, varies from a *coppery red* to a brownish gray, and it is the dull, grayish, obscure, intermediate shades which constitute what is called the "syphilitic tint." This copper colour is the most constant and remarkable symptom of the syphilides. It is of itself sufficient to identify the disease, and is invariably present in every variety and form of these eruptions without exception. The change of colour is produced by a vitiated secretion of the chromatogenous apparatus, and not by an altered condition of the capillary rete, as alleged by some writers. It is analogous to the abnormal colorations of the ephilides and the pigmentary nævi; it does not disappear under the pressure of the finger, and the depth of the colour depends in great measure upon the degree of injection of the cutis; for example, when there is much sanguineous congestion present the syphilitic tint is masked altogether, and according as this condition subsides it becomes gradually more pronounced.

The shade of colour is also regulated by the nature of the eruption and the temperament of the patient; thus, in a young sanguineous subject, with a fine, white skin, attacked with roseola, or certain forms of lichen syphilitica, the "tint" partakes somewhat of the colour of the adjoining

surface, by reason of the capillary injection, but it is always duller than that of the surrounding skin, and as the general redness disappears, it assumes its natural coppery violet colour. In persons in the decline of life, with a dry, shrivelled skin, the tint is still duller, and of a violet red shade. In persons of a bilious habit, whose skin is naturally brown, the syphilitic tint presents remarkable shades; it is, in general, scarcely at all red, but rather of a dark gray colour, approaching a brown, which is gradually blended with that of the surrounding tissue; and, finally, in cachectic individuals, the colour of the eruption is livid, and even old cicatrices in these cases present a peculiar blueish appearance.

The syphilides have a remarkable tendency to assume a *circular form*. This disposition is not confined to isolated patches of limited extent, but may be observed in cases where the eruption is considerably diffused. In the latter instance, although the circle may not be complete, we may always find detached patches, showing a distinct tendency to form a ring. This circular form, however, is not invariably pathognomonic of the disease, for there are some varieties in which it does not appear, and, on the other hand, it occurs in certain cutaneous diseases, which are not special; as, for example, herpes, lepra, lichen, &c.; but again, it should always be remembered that certain eruptions which do not, in their simple form, show this peculiarity, invariably do so when they become syphilitic. For example, the tubercular and pustular syphilides show it in a remarkable degree, and even the horny syphilide, which we sometimes see in the palms of the hands, also evinces this peculiarity. The face, above all other regions of the skin, seems to be particularly predisposed to this form of the eruption, and hence the origin of the term *corona veneris*.

The syphilitic eruptions are further characterized by their tedious chronic progress through their various stages. They are slow in being developed, the process of suppuration is difficult, and cicatrization is long in being accomplished; in short, each phase is longer of being completed than another, and this very chronicity forms an important feature of the disease. The anatomical characters are also *sui generis*. In the squamous eruptions the scales are thinner and drier than those of the simple forms of these diseases. They are likewise much smaller, and never cover the patch completely, but form a whitish fringe or list (*lisière*) around it, to which Biett attached much importance as a diagnostic. The scabs are formed slowly, and the papular elevations of psoriasis and lepra syphilitica are frequently observed without any. The syphilitic incrustations are thick, greenish, sometimes black, hard, furrowed, and very adherent. They always indicate a certain amount of destruction of the tissues beneath. These scabs sometimes cover ulcerated surfaces, and then they are softer, broader at their base, surrounded by a flabby copper-coloured rim, or they may repose on a cicatrized base, in which case they are dry, shrivelled, horny, and uneven at their base, and by gradual destruction disclose a cicatrix beneath, into which they seem to penetrate by a kind of mammillated projection.

These considerations lead us to speak of a very important character common to all the syphilides, with the exception of the exanthemata and squamæ. We allude to the remarkable tendency which these diseases evince to destroy the tissues in which they are produced, as shown by the

indelible cicatrices which they leave behind. The syphilitic ulcerations belong exclusively to the vesicular, pustular, and tubercular species. They are round, more or less deep, grayish or very red at the base, with swollen and sharp cut edges. The round form of the ulcerations is always present; it is especially marked in those which occur in ecthyma. It is not so clearly defined in the tubercular syphilides; in the serpiginous ulcer, for example, the borders are certainly circular, but the ring is less regular in form, and, moreover, it is not the result of a single tubercle, but of several, and of repeated destruction of the tissues. It is a remarkable fact, that this destructive process is frequently accomplished without ulceration or a breach of surface, and we may observe the curious phenomenon of the formation of an indelible cicatrix, without the slightest wound or breach in the continuity of the skin. This singular process of destruction is particularly observable in the papular and tubercular syphilides, and seems to be the result of degeneration of the tissue in the first instance, and of absorption of it afterwards.

The syphilitic cicatrices present several characters which belong also to the lesions they have succeeded. Thus they are generally round, and more or less depressed; when recent they are of a bronze colour, and are sometimes slightly prominent, and the superficial vessels may be seen ramifying beneath the epidermis. At a later period they shrink, the process of absorption apparently going on internally; they lose their violet colour, and become white and still more depressed, their surface, which is now of a dead white hue, is tense or puckered, and sometimes intersected with hard prominent bands. In some cases they are of a blueish white tint from the beginning, and are surrounded by a copper-coloured areola, which gradually subsides, until it becomes lost in the tint of the surrounding skin.

Special Symptoms. Whatever may be the characters by which the syphilides are known, whatever may be their aspect, they invariably commence in the form of spots or blotches (*taches*), vesicles, blebs, pustules, papulæ, squamæ, or tubercles; hence M. Cazenave proposes to classify and describe them in the following order:

Syphilides {
- Exanthemata
- Vesiculæ
- Bullæ
- Pustulæ
- Papulæ
- Squamæ
- Tuberculæ.

Syphilitic exanthemata. These eruptions are characterized by irregularly-shaped blotches, sometimes scarcely raised above the level of the skin, of a coppery red colour at first, which, by and by, passes into a grayish tint, and disappears slowly but never completely under pressure of the finger. They are rarely accompanied by febrile symptoms, are never followed by ulceration or cicatrix, and the only trace that remains after resolution, is a slight stain, which sometimes continues for several months. There are two varieties in this class pretty distinct from each other. In one (roseola) the spots are broad and irregular, and scarcely, if

at all, raised above the surface. In the other (erythema papulatum) the patches are more circumscribed, distinctly rounded, and slightly prominent.

Roseola is one of the least common of the syphilitic eruptions. It consists in slight patches or *efflorescences*, more or less diffused, and usually seated on the chest, the neck, the face, and upper extremities; of a violet or copper colour at first, which finally changes to a gray tint. It is rarely accompanied by febrile disturbance, but it is not unfrequently preceded by dull pains in the limbs, headache, and angina, characterized by a violet-red colour of the mucous membrane of the mouth, velum palati, and pharynx, accompanied by a sensation of heat and difficulty of swallowing. M. Cazenave has even observed in some cases, a peculiar ulceration of the amygdalæ with sharp cut edges and gray base, but never deep or extensive.

Erythema papulatum shows itself in the form of patches of very limited extent, not larger than the circumference of a shilling, and frequently less, slightly prominent, round, of a grayish brown tint at first, and subsequently a pale red, which does not disappear under pressure, and terminates by resolution. This eruption appears without premonitory symptoms, and most commonly on the extremities, especially the arms. It is frequently ephemeral; it appears and disappears continually, and is suddenly transposed from one place to another. Its duration is much shorter than that of roseola, and it is never followed by desquamation. M. Cazenave is of opinion that this eruption is frequently the result of the sudden suppression of blennorrhagia by copaiba, &c., but altogether independent of that medicine, for he has met with the disease in many cases where it was not administered. The author has satisfied himself that both roseola and erythema syphilitica are generally produced by the sudden removal of chancre or blennorrhagia, more especially the latter, by means of revulsives. M. Cazenave here relates several cases illustrative of the primary and consecutive forms of the syphilitic exanthemata. In the latter, the eruptions appeared in intervals of from two months to ten years after the primary disease, and evinced the same characters in each instance.

Syphilitic vesiculæ. This species was supposed, until very recently, to be the rarest of all the venereal eruptions. Biett met with only a few cases. But M. Baumes and M. Cazenave agree in stating that syphilitic vesicular eruptions are by no means so rare as supposed, and require merely careful observation to be detected. M. Cazenave distinctly states that they may appear in any of the forms of the simple vesicular disease. Sometimes they manifest themselves in the character of round, globose, detached vesicles, as in varicella; sometimes in the form of small circular discs, as in herpes; occasionally in an eruption of numerous vesicles thrown into groups, and disseminated, as in eczema. The vesicles are distinguished from those of the simple forms, by a copper-coloured areola round their base. Contrary to the habit of the syphilides in general, the vesiculæ seldom appear on the face. They are most commonly observed on the neck, chest, and upper extremities. Their duration is also shorter than that of the other species, and varies from two or three weeks to as many months. The author gives several cases illustrative of the different varieties of these eruptions, and amongst others an interesting case of

syphilitic varicella, of which there is a well-executed coloured plate in the accompanying atlas. As it will answer the purpose of a detailed description, we shall give this observation in full.

"A young girl, 16 years of age, of healthy constitution, had complained for a few days of heat in the throat, with difficulty of swallowing, anorexia, and irregular fever; a number of small eminences now appeared on different parts of the body, and she entered the Hospital of St. Louis. The eruption was at once seen to be vesicular, and pronounced chicken-pock. It was the sixth day of the eruption; it covered nearly the whole body, and the vesicles were in different stages; some being nascent, others dried up. Biett having examined the patient, discovered a strong resemblance between this eruption and two other cases of syphilitic vesicular eruption which had come under his observation. This diagnosis was soon confirmed by the progress of the disease. The vesicles were small, resting on a broad base, and surrounded by an areola of a vivid copper colour; their progress was slow, and they were unattended by any local symptoms. They gradually faded away, and the fluid was absorbed; but, in some, the contents of the vesicle hardened into a thin scab, which adhered for some time. Every one of them, however, left behind a coppery injection of the skin, which presented all the characters of a syphilitic blotch. In addition to these circumstances, a careful examination of the throat disclosed a round grayish ulcer, with sharp cut edges, &c. The treatment employed was insignificant, as Biett wished to see if any more decisive symptoms would manifest themselves; but the patient left the hospital in a fortnight. After the lapse of a month she was visited at home, when her body was found covered with true syphilitic pustules."

Syphilitic bullæ. The bullæ have been observed in the syphilitic form in the only two varieties known of that class, pemphigus and rupia. Syphilitic pemphigus is almost invariably a disease of new-born infants, derived from their parents, and consists in an eruption of one or more bullæ, of irregular size and form, existing at the period of birth, and usually situated on the palms of the hands and soles of the feet. The blebs are soft, contain a sero-purulent fluid, and are surrounded by a violet-coloured areola. Rupia syphilitica is more rarely met with than the preceding variety. In this instance the bullæ are larger, nearly circular, contain a blackish fluid, which dries into a scab of the same colour, of a conical form. They are surrounded by a copper-coloured areola, and terminate in indelible cicatrices of a circular form.

Syphilitic pustulæ. The history of the pustular syphilides is, perhaps, more interesting than that of any other class of venereal eruptions; because, under this title, all the syphilitic eruptions were described by former writers, and also on account of the great frequency of some of its varieties. Although every form of the simple pustular diseases may appear on the skin under a syphilitic character, M. Cazenave confines his descriptions to the following varieties as being most commonly met with in practice: 1, a syphilitic pustulo-lenticular eruption; 2, impetigo, in two distinct forms—impetigo non confluens, and a syphilitic pustulo-crustaceous variety; 3, ecthyma. The lenticular form is the most common of the syphilitic pustulæ. It is characterized by an eruption of isolated pimples, about the size of a small lentil, irregularly diffused, slightly prominent, of a peculiar colour, suppurating partially, and terminating in a small cicatrix, of less diameter than the original lesion. It attacks every region of the skin; the face, the back, the chest, the limbs, &c., and varies a little in form according to the region in which it is developed.

On the face, chest and back it corresponds to acne; the pustules are larger, more prominent, and round; they suppurate only partially, and are crowned by an incrustation of some thickness, terminating in a cicatrix with a depressed centre. On the limbs they are flattened especially round the base, which is broader, and less rounded than that of the preceding. They are of a copper colour, and the size of a small lentil. The centre becomes slightly prominent, and we may presently remark a minute collection of pus at the apex, which disappears in the course of a day or two, either by absorption or by the rupture of the pustule. The elementary lesion now assumes a new phase. It is converted into a small copper-coloured papular-like elevation, with a depressed cicatrix at its apex, which is sometimes perforated down to the base of the pimple. According as the disease advances, it loses its pustular appearance, and might be mistaken by a careless observer for a papular eruption; but new pustules are constantly appearing in the vicinity of the old, which will serve to clear up the diagnosis. It is, however, owing to the frequency of errors of this kind, that some writers have supposed that the papular eruption was one of the most frequent of the syphilides, and M. Cazenave has had cases under his care where the disease, strange to say, was mistaken for common itch. The progress of the lenticular eruption is always slow, and it terminates in a minute central cicatrix superimposed on a hardened base.

The simple form of syphilitic impetigo closely resembles syphilitic varicella. The pustules, which are of moderate size, are detached from each other, are preceded by a red copper colour, have not a hardened base, and are formed by an elevation of the epidermis, which covers completely the blotch. This variety has the appearance of a number of small, soft, slightly resistent tumours, pretty close to each other without uniting, and encircled by a reddish areola. It is usually preceded by febrile symptoms. It may appear at once over an extensive surface of the body, the abdomen, buttocks, internal aspect of the thighs, and sometimes on the upper extremities, but seldom on the face. If the pustules are not torn, they may continue without change for several days; the contained fluid gradually coagulates into a brownish scab, larger than the pustule, which, by and by, becomes so thin that you can see a cicatrix beneath. In some rare cases the pustules increase in size, and run into each other, and form a single incrustation.

The pustulo-crustaceous impetigo is a much severer form of the disease than the preceding, and is particularly obnoxious from occurring so frequently on the face. It commences by a redness and tumefaction in the parts about to be attacked, which are soon followed by small collections of matter. These speedily unite, and then the disease consists in one or more large blotches, surrounded by a copper-coloured areola, and covered by slightly raised scabs, irregular in form, greenish, soft to the touch, incurvated at the centre, and flattened at the circumference. These scabs conceal grayish ulcerations, with slightly elevated edges, which secrete a sero-purulent matter. By means of this matter, the incrustations are incessantly renewed until the disease is at length modified, then they dry, shrivel, and fall off, exposing to view an unseemly scar, whose deformity is in proportion to the number of times the scabs have been reproduced. This eruption may appear on several parts at the same time,

but it has no tendency to spread to the adjoining parts, and generally acquires at once the size and form it retains throughout its different stages. It attacks the chest, the neck, but more especially the face and forehead, and, unlike the preceding variety, is seldom seen on the lower extremities.

Ecthyma syphilitica is characterized by pustules of a much larger size than the preceding varieties, forming small isolated tumours, with indurated bases, containing purulent matter, and terminating speedily in incrustations, which in their turn are succeeded by cicatrices. In some cases the pustules are larger than those of impetigo, but do not exceed the circumference of a sixpence. They are perfectly round, slightly conical, distended by a thick yellowish fluid, are surrounded by a copper-coloured areola, but are not accompanied by induration at the base. These burst in a short time, and are then covered with a brown scab of the same size and form as themselves, slightly adherent, with raised edges, and concealing a superficial ulcer. This variety is generally diffused over a considerable surface of the skin, and occurs frequently on the scalp. Although the pustules are usually scattered, we sometimes, but rarely, may observe them thrown into groups, in which event they terminate in a broad thick scab, not unlike that of the pustulo-crustaceous variety already described. There is another form of syphilitic ecthyma and severer than the foregoing, in which the pustules are considerably larger, and of an oval form. They commence by a violet-coloured blotch, which rises at the centre, and is quickly distended by a thick fluid resembling a mixture of pus and blood. The pustule is encircled by a livid areola, which again is embraced by one of the characteristic copper tint. At the point where the distension of the epidermis ceases, there is a slight puffiness, which gives the pustule a flattened appearance. The coats of the pustule soon give way, and the contained fluid is partly discharged; this coagulates and forms a blackish incrustation, thick at first, but gradually dries into an eschar. This scab, which is exactly the form of the pustule it has succeeded, is more prominent at the centre than at the circumference. If the incrustation be removed at an early period, we shall see beneath a deep ulcer with a grayish base, studded with small red granulations, with sharp cut edges, and close to the latter a whitish line, which is composed of portions of epidermis, separating the scab from the ulcer. If the scab is not interfered with, but allowed to proceed in its course, we may observe it gradually desiccate, and fold upon itself, as it were, by the desiccation of the central portion. The white border mentioned fades and desquamates, and we may now observe the circumference of the incrustation penetrating, so to speak, into the substance of the skin; the scab itself gradually gives way and cracks, until it finally disappears, and is replaced by a round depressed cicatrix of a peculiar violet tint. This eruption occurs most frequently on the extremities, especially the lower, and is sometimes accompanied by a papular disease. The pustular syphilides are most commonly consecutive, but they may be primary symptoms, and syphilis sometimes manifests itself by them alone.

Syphilitic tuberculæ. The tuberculæ, which have been invariably confounded with the pustular diseases by former writers, are by far the most frequent of all the venereal eruptions. They appear in the form of small, solid, resistent tumours, containing neither pus nor serum, and

more or less prominent. They are sometimes disseminated over a large surface, sometimes, on the contrary, disposed in groups, few in number, and confined within narrow limits. Their size and form is also variable. In some cases they are small, about the size of a pea, round, shining, and of a coppery colour; in others, broad, flattish, spherical, or ovoid, and seem imbedded in the substance of the skin. Sometimes they remain smooth, and shining throughout, sometimes covered with thin scabs: when ulceration takes place, it is succeeded by thick scabs; they occasionally terminate without leaving any other trace than a grayish stain, which disappears in the course of time, but more frequently in an indelible cicatrix, whose size and form depend upon whether it was preceded by ulceration or not. Finally, whatever may be the severity of the disease, it sometimes pursues its course, and corrodes the tissues from without inwards, without extending beyond the parts on which it first appeared, whilst, on the contrary, we not unfrequently meet with cases in which it extends its ravages over a considerable surface, destroying the skin to a greater or less extent in its progress. This frightful disease may appear on any part of the body; but unfortunately, it most frequently occurs on the face, the nose, the ears, the eyebrows, and the scalp. It sometimes is developed slowly and gradually, whilst in other instances it appears suddenly, is accompanied by fever, inflammation of the mucous membrane of the pharynx, velum palati, and tonsils, and pursues its course with great rapidity.

M. Cazenave describes five well-marked varieties of the tubercular syphilitic eruption: 1, cases in which the tubercles are disposed in groups; 2, those in which they are disseminated; 3, a corroding form of the disease; 4, a *serpiginous* variety; 5, one characterized by flattened tubercles. The characters of the first two varieties may be found in the preceding remarks. They are of a milder nature than those which succeed, and the tubercles in some cases are very minute, and resemble papulæ, and rarely suppurate. They are always consecutive, never appearing with the primary disease.

The third variety is very different from the two preceding, and always tends to destructive ulceration. In cases of this kind the tubercles are large, few in number, penetrate deeply into the cutaneous tissue, of a semispherical form, and pointed at the apex. They occur almost invariably on the face, especially the nose and lips, and are sometimes accompanied by considerable tumefaction, in the midst of which they are buried, and can with difficulty be detected. This tumefaction gives way under appropriate treatment in some few instances, and resolution follows, but more commonly this condition appears and disappears until at length the parts are struck with inflammation of an unhealthy character, and ulceration ensues. The tubercles, after remaining stationary and indolent for a considerable time, are suddenly reanimated. The areola, which had almost faded, assumes a more intense hue than ever. The tumours now begin to ulcerate at the apex, and generally follow one of two courses: they may remain soft, scarcely painful, and but slightly ulcerated; or become tense, exceedingly painful, surrounded by an erythematous blush, and the ulceration at the apex extends deeply into the substance of the tubercle. The ulceration is succeeded by a blackish, dry, thick incrustation, which

soon falls off, and discloses a deep ulcer with cleanly cut edges, a new scab is formed, falls, and exposes to view a more extensive destruction of the parts, and finally a deep eschar, of a violet colour, of an almost circular form, and as regularly shaped as if cut out by a chisel, terminates the progress of the disease. Thus, in an exceedingly short space of time, a frightful mutilation of the face may ensue, and the alæ of the nose, the lips, the ear, and the eyelid are more or less involved in the destructive ulceration going on around. M. Cazenave gives several interesting cases illustrative of this variety, which we regret want of space will not allow of being related here.

Serpiginous tubercular eruption. This variety has a still stronger tendency to destroy the tissues in which it is developed than the foregoing, but it has this peculiarity—that the ulceration is much more superficial, but covers a greater extent of surface than the preceding form, so that, what it loses in depth it gains in length. Indeed, we may sometimes see the whole body covered with indelible and irregularly-shaped cicatrices. The serpiginous eruption manifests itself in the form of large, red, hard, and circular tumours, scattered here and there, few in number at first, and varying from the size of a pea to that of a filbert. They are most commonly observed on the face and trunk, but the author has seen many cases in which they attacked the whole of the cutaneous envelope, with the exception of the palms of the hands and soles of the feet. They are seen not unfrequently on the back of the neck, scalp, and shoulders, and seem to have a predilection for parts covered by hair, as, for example, the temples, eyebrows, anus, genitals.

These tubercles are smooth and shining, of a coppery tint, are never covered by squamæ, and remain indolent and stationary for a long time. Then they become suddenly inflamed from some accidental cause, ulcerate at their apices, the tubercle is completely destroyed, and is succeeded by a thick, hard, conical incrustation, of a black or grayish colour, and very adherent. If this scab is detached before the formation of a cicatrix, it exposes to view a gray superficial ulcer, which is soon covered by another incrustation, not so black or compact as that just described, but always thicker at the centre than at the circumference. When once the process of ulceration is established, the disease advances rapidly, by the development of new tubercles close to the old, or at the edge of the cicatrices. The inflammatory process is renewed here, and this state of things goes on until extensive ulcerations are formed, and destruction of the surrounding tissues, to a greater or less extent, ensues.

While the ulceration is healing at one of its extremities, new tubercles are being incessantly developed at the other, so that we may frequently see in the same patient and at the same time all the characters of the eruption: thus, we may find a crop of salient copper-coloured tubercles scattered round the diseased parts; scabs of various degrees of density, size, and hardness, dry and prominent; grayish ulcerations, with sharp cut edges, some of which are round. others without any regular form, and more twisted in spirals, all of which are intermingled with indelible cicatrices. These, again, are either old, whitish, fibrous-looking eschars, or more recent and copper-coloured, and intersected by superficial vessels. They unite pretty uniformly, but are here and there interrupted by fur-

rows or indentations, which give them a peculiar figured appearance. They are crossed by bands of considerable thickness, which frequently arrest muscular motion, and are studded with small tubercular eminences, which in their turn ulcerate, and produce scars and furrows of the most unseemly kind. The cicatrices have often the appearance of an extensive burn. The progress of this eruption is generally slow and its duration protracted. It is always consecutive.

The fifth and last variety of the tubercular syphilides is not the least interesting of this species. It is often a primary symptom, and is distinguished from the preceding varieties by the flattened appearance of the tubercles, which are commonly, but erroneously, called "*pustules plates.*" This eruption is characterized by round, thick, and flattened tubercles, varying in size from that of a lentil to that of a shilling at their base, and of a vivid copper colour, and are perforated on the top by small linear ulcers. The smaller variety is found chiefly on the sides of the nose and lips, the larger on the scrotum, penis, pubis, thighs, and anus. The summit of the tubercle soon ulcerates, and presents the appearance of a narrow slit, from which a sanious fetid matter is discharged. The whole scrotum is sometimes covered by these tubercles; they are isolated, perfectly round, and very prominent. Round the margin of the anus they may coalesce and form large surfaces, but the ulceration is always superficial. The tubercular syphilitic eruptions are represented by several excellent and correctly coloured plates in M. Cazenave's atlas.

Syphilitic papulæ. These eruptions are characterized by small, solid, resistent elevations, of a well-marked copper colour, containing neither pus nor serum, and diffused over a considerable extent of surface. The papular syphilides manifest themselves in two distinct forms, one of which assumes an acute, and the other a chronic character. *Lichen syphilitica* is the acute variety, and is generally ushered in by slight febrile symptoms, as the exanthemata. It appears in the form of innumerable small papulæ, sometimes confluent, and possessing a shining appearance, which, joined to the copper tint, gives them a most remarkable aspect. This is the eruption which Carmichael says so frequently accompanies blennorrhagia. It occurs on most parts of the body, but especially on the neck and face, and, from the similarity of several of its symptoms with those of the febrile eruptions, it is not unfrequently mistaken for one of the latter.

The duration of this eruption is generally short, and it terminates by resolution in the course of a fortnight or so. The papulæ generally terminate in a kind of insensible exfoliation, leaving no other trace of their existence than slight stains, which soon disappear.

In the other variety the papulæ are much larger than those of the preceding, and commence in the form of small yellow blotches. They are isolated and scattered over a large extent of surface, and the eruption may be seen in different stages in the same individual at the same time. In one place we may find hard, prominent, copper-coloured papulæ; in another small cuticular elevations, more prominent, softer, and of a paler red. Here we observe yellow stains, approaching to a rosy tint, which are about to become papulæ; and elsewhere we see small gray spots, more depressed than the preceding, which are merely the traces of former

papular elevations. All these are separated from each other by sound portions of skin, the colour of which, however, is of a peculiar *earthy* appearance, perfectly unique. This variety occurs most commonly on the extremities, shoulders, neck, forehead, and scalp. It is not accompanied by pruritus, is always consecutive, and frequently complicated with other venereal eruptions. It assumes almost invariably a chronic form, and disappears, sometimes after the lapse of several months, by resolution. The author relates several cases illustrative of this and the preceding variety.

Syphilitic squamæ. The scaly syphilitic eruptions are extremely interesting. The anatomical characters are essentially different from the preceding species; for, instead of the usual products of inflammation which characterize the diseases already described, we have here a double lesion of the chromatogenous and blennogenous apparatuses—those which secrete the colouring matter of the skin and the epidermic matter. These eruptions are characterized by dry, grayish, slightly adherent, epidermic lamellæ, covering irregularly-shaped patches of skin, slightly prominent and of a copper colour, and sometimes even of a blackish tint. The lamellæ are merely the results of a thickening of the epidermic matter, which dries, cracks, and is detached in scales. M. Cazenave describes three varieties of this species: syphilitic lepra, syphilitic psoriasis, and a *horny* variety which Biett mentions.

Syphilitic lepra must not be confounded with the *lepra venerea* of authors, which is a pustulo-crustaceous disease (*impetigo syphilitica confluens*), and another example of the mischief produced by vague terms. Syphilitic lepra appears in the form of circular discs, with depressed centre and raised edges. The patches, unlike those of lepra vulgaris, scarcely ever exceed the circumference of a shilling. They commence in the form of a small elevated patch of epidermis, not unlike a broad papula. They are first of a copper colour, and subsequently of a violet hue, and are covered with dry, hard, grayish scales, which fall, and are renewed incessantly until the disease is at length suppressed. It terminates by resolution, and leaves behind a dark circular stain, which corresponds exactly to the round disc which it has replaced. This eruption, which is one of the rarest of the syphilides, occurs generally on the limbs, and is preceded by some constitutional disturbance.

Syphilitic psoriasis is a much more common variety than the preceding. It appears in the form of P. diffusa, but more frequently in that of P. guttata. In the first the patches are broad, irregular in form, and of a deep coppery-red colour, and are covered by hard, brittle scales, of a dull white tint. This variety occurs frequently on the face. It sometimes coexists with the second form of the eruption, and then it is seen on the palmar aspect of the hands, and about the malleoli. Psoriasis guttata is much more commonly met with than the preceding form. It is characterized by prominent patches, without a central depression, of a round or oval shape, and rarely exceeding the size of a farthing in circumference. These patches are isolated, scattered in considerable numbers, of a vivid copper colour at first, which finally passes into a grayish or leaden hue, and are crowned with gray and very adherent squamæ. The scales, however, are very flimsy, are reproduced with difficulty, and the red elevation which they leave behind is quite pathognomonic of the disease. Its base

is encircled at the point where it rises above the level of the skin by a small white rim, with a fringe or border (*lisière*), at first sight like that of a faded vesicle. After continuing for a certain time, the patches begin to disappear, and their colour at the same time becomes fainter, but the latter is of longer duration than the former. It changes successively from a copper tint to a brown, then to a gray, and finally vanishes altogether, leaving no trace of its existence behind. This eruption is generally diffused over the whole body.

The *horny* variety of the syphilitic squamæ is generally developed in the palms of the hands and soles of the feet. It appears in the form of slightly elevated spots of a copper colour, circular form, and covered by hard grayish scales. Sometimes these scales are multiplied in great numbers, and coalesce, so as to form one large thick patch, which increases by successive layers, cracks, and forms painful fissures. The squamous patch is invariably surrounded by a pretty broad copper-coloured areola. In another set of cases the patches do not exceed in circumference that of a farthing, are rounded, scarcely prominent, and have a hard, white, horny point in their centre, which penetrates like a corn into the substance of the skin. Around this horny matter we may constantly observe a circle about two lines in breadth, like a zone, and of a very peculiar colour. This variety may exist for months, and even for years, without occasioning much inconvenience.

The syphilitic squamæ are most commonly consecutive, and hence are generally met with in adults; but M. Cazenave has often seen these eruptions in young children, and even in new-born infants. In the latter, he has met with a case of this disease developed in the palms of the hands and soles of the feet.

M. Cazenave next proceeds to describe the *concomitant* symptoms of the syphilides, which may be developed in the skin itself, the cellular tissue, the appendages, the mucous and serous membranes, the osseous and fibrous tissues, and consist in blotches, tumours, ulcerations, caries, necrosis, &c., but want of space prevents our entering into the details of this interesting chapter. We must therefore be content with recommending it to our readers as well worthy perusal, and pass on at once to the causes and treatment of the syphilitic eruptions.

Causes. M. Cazenave denies that mercury is a common cause of syphilitic eruptions. He has administered that remedy in a host of cases at the Hospital of Saint Louis, where no symptoms of the kind were produced, and he has, on the other hand, repeatedly met with them in cases where mercury had never been administered. The result of his extensive experience in the treatment of these diseases goes to prove, that if the mercurial treatment will not invariably prevent the appearance of secondary symptoms, it certainly is not the cause of the syphilitic eruptions, whose real origin is a specific virus, involving the constitution generally. The syphilides may be primary, secondary, or hereditary. In the first instance, they are the immediate results of blennorrhagia or chancre, or they may appear consentaneously with either of these lesions. In the second, they are manifested long after the original disease has disappeared, in a system poisoned by the syphilitic virus, and only waiting for some accidental cause for their development. In the third, they are

transmitted by the venereal act to the embryo, but the manner in which this is accomplished, and the laws which influence its transmission in one case more than another, are mysteries which baffle all inquiry.

Those of the syphilides which most frequently appear as primary, i. e. accompanying the earliest symptom of syphilis, are erythema and roseola syphilitica, syphilitic papulæ and vesiculæ, the lenticular variety of the syphilitic pustule, and superficial ecthyma.

The secondary, and most common form of the syphilides is always developed by some accidental cause, and frequently occurs after the lapse of an almost incredible period of time from the date of the primary and original disease. The author relates several cases, showing how the venereal eruptions were not developed for thirty and thirty-five years after the system had been primarily infected, and in a number of cases the primitive affection was blennorrhagia.

In 172 cases treated by the author, the average time that elapsed between the primary disease and the syphilides was ascertained to be five years and ten months, when blennorrhagia was the elementary form of the venereal complaint; four years and three months when they were the sequelæ of chancre; three years and one month when preceded by chancre and bubo; after repeated primary infections eight years and three months; after chancre, complicated with blennorrhagia or bubo, three years and eight months; and, as a general mean, five years and two months may be considered to be the period which elapses between the termination of syphilis and the invasion of the venereal eruptions. Chancre, complicated with bubo, seems to produce the syphilides more rapidly than any other of the primary symptoms, complicated or not, and the widest range is between chancre and blennorrhagia; five years is the shortest period for the development of the secondary eruptions by the former, and thirty-five years the longest by the latter. The tubercular syphilides are the longest of appearing after the primary disease, and the pustular syphilides the most rapid.

M. Cazenave does not agree with Mr. Carmichael that there is a certain degree of relationship between the nature of the primary symptom and the particular form of eruption which may succeed it; or, in other words, that certain primary symptoms will produce certain secondary lesions. The author maintains that any form of the syphilides may succeed indifferently blennorrhagia or chancre, in a severe or mild form. M. Cazenave also positively states, as the result of long and careful observation, that the venereal eruptions may and do appear as sequences of a simple blennorrhagic discharge, and in this opinion he is supported by MM. Martius, Legendre, and Baumes. Out of 157 cases which he selected and examined with the most rigid scrutiny, and in whom one or other of the syphilides existed, 42 had for their origin chancres, simple, or complicated with bubo; 60 had originated in blennorrhagia, with or without buboes; 48 had chancres, blennorrhagia, and buboes, together or at different periods; 5 had the bubo *d'emblée;* and 2 originated in the primary form of eruption; hence he concludes, that if blennorrhagia is not the most frequent cause of the syphilides, it certainly is as commonly so as chancre.

The eruptions which M. Cazenave has observed as the sequences of blennorrhagia were in the following proportions in 58 cases: Syphilitic

tubercles in 23; S. pustules in 16; S. papulæ in 6; S. squamæ in 7; S. exanthemata in 2; S. vesiculæ in 4. The author's researches lead him to the conclusion that the primary disease occurs most generally between the ages of eighteen and thirty, and the syphilides between those of twenty and forty. He divides the syphilitic life (*la vie syphilitique*) into seven periods, and obtained the following results in 158 cases: Up to ten years only one case (hereditary); from 10 to 20, seven (2 hereditary); from 20 to 30, sixty-seven; from 30 to 40, forty-three; from 40 to 50, twenty-seven; from 50 to 60, eleven; above 60, two. With regard to the influence of the seasons on the development of the syphilides, the author considers that they occur more frequently in cold than warm weather. Out of 112 cases carefully examined by the author, 14 occurred in January, 12 in December, 11 in March, 9 in February, 8 in November, 7 in October, and the highest number in the warm months was 13, which occurred in June. These researches lead to a conclusion the reverse of that generally entertained upon the point, which the author himself admits. In alluding to the singular tendency which the syphilides have to appear upon the head, and especially the face, M. Cazenave states that in 172 cases, they occurred 66 times on the head exclusively; in 31 cases they appeared first on the head, and thence spread to other parts; and in 75 they attacked other parts of the body. Trade or profession has little or no influence upon the development of the syphilides. The preceding statements are supported, according to the author's general plan, by numerous cases, several of which are extremely interesting, and may be read with profit.

Diagnosis. The special character of the syphilitic eruptions, their contagious nature in some cases, and their destructive tendency, invest the diagnosis of these affections with a peculiar degree of importance. It would be equally unfortunate to mistake simple cutaneous diseases for these, as not to be able to recognize them when they really exist, and it is unnecessary to dwell on the frightful results sure to attend errors of this kind, examples of which we may see every day in the streets of this metropolis. The diagnosis of the syphilides is based upon a knowledge of their general characters, and on a certain *ensemble* which it is difficult to describe, depending on the peculiar colour and arrangement of the eruption and general state of the patient. M. Cazenave enters minutely into the diagnostic characters of each particular eruption; but as most of them may be found under the head of "Symptoms," which we have already pretty fully described, we must, for the present, be content with recommending this important chapter to the attention of our readers, and proceed to discuss the different therapeutic measures adopted by the author for the cure of the syphilitic eruptions.

TREATMENT.

It were useless to enumerate the long list of remedies which have been employed in the treatment of constitutional syphilitic affections, we shall therefore confine ourselves to a consideration of those the utility of which has been demonstrated by experience. M. Cazenave gives a preference to mercury above all other remedies, in the treatment of the venereal eruptions.

He has found it, after long experience, to act with greater certainty and promptitude than any other medicinal agent in this class of disease, and although it may not be able to eradicate altogether the syphilitic constitutional taint, it can so modify it as to prevent a speedy relapse. Our own experience with regard to the treatment of the syphilides fully bears out M. Cazenave's views upon this point. We have witnessed in the practice of Biett, and more recently in that of M. Cazenave himself, the happiest results follow the judicious administration of mercury, and this after a variety of other remedies had failed either to remove or modify the eruption or the constitutional taint.

M. Cazenave strongly condemns, and justly so, the production of salivation, as recommended by some writers on this subject. He has found mercurial inunction most servicable in the secondary form of the syphilitic affections, in which the bones are involved, but still more so in the primary syphilides. A scruple at first, and subsequently two scruples of mercurial ointment may be rubbed in, and a warm bath every third day will cleanse the skin and facilitate the absorption of the mercury. It has been recommended to apply the ointment to the prepuce, but this practice is objectionable, in consequence of the irritation and tumefaction of the glans it produces. The author prefers applying the mercurial ointment internally in the form of pills, after the manner of Biett, and according to the following formula: Mercurial ointment and Sarsaparilla powder, of each three scruples; mix, and divide into forty pills. One to be taken morning and evening to begin with, and afterwards the dose may be increased to four pills during the day, but not to exceed this. This method, however, is slow, and unfortunately has a great tendency to produce ptyalism, which should always be prevented if possible.

Protochloride of mercury. Calomel is, perhaps, the least active of all the mercurial preparations in the treatment of the venereal affections of the skin; nevertheless, it has been recommended by Clare, Cullerier, and Brachet, of Lyons, and Biett has found it in some rare cases attended with good effect, when snuffed into the nostrils. It is now seldom employed.

Bichloride of mercury. This remedy ranks amongst the most useful preparations of mercury for the treatment of syphilitic skin complaints, but it is so difficult to be able to push it far enough to effect the object in view, and other preparations answering the purpose as well without this drawback, that practitioners are not fond of employing it frequently. Biett used to administer it in the form of pills, thus: Alcoholic extract of aconite, 6 grains; Bichloride of mercury, 2 grains; marshmallows powder, 18 grains; make eight pills. Begin with one pill *per diem*, and increase the dose gradually to four each day. For our own parts, we prefer using all remedies in skin complaints in a fluid form. They are more diffusible through the system in this than in the solid form, and we have invariably found their action more certain, and the curative effects more rapid when administered in the manner indicated. For this reason we prefer, with M. Cazenave, employing Van Swieten's liquor or Larrey's syrup to Biett's pills; besides, they are much easier borne by the patient. But M. Cazenave, like his preceptor, is generally fond of pills.

Ammoniacal protonitrate of mercury. This remedy, which is the soluble mercury of Hahnemann, is very useful. It is more easily managed and

easier borne by the patient than the preceding. It operates with promptness, and may be used beneficially either in a mild or severe form of the eruption. The author prescribes it in the form of pills: Soluble mercury of Hahnemann, two scruples; Liquorice powder, two scruples; make forty pills. One pill night and morning, to be increased afterwards to four pills *per diem.* This remedy is borne well by feeble and delicate patients.

Iodides of mercury. Of all the mercurial preparations, and of all the remedies of whatsoever kind, which have been recommended in the treatment of syphilides, none can approach, in therapeutic value, the iodides of mercury. We are indebted to Biett for the introduction of these valuable remedies in the treatment of the venereal eruptions. This practitioner at first preferred the biniodide, and administered it in pills in the following form: Biniodide of mercury, ten grains; Liquorice powder, one drachm; make sixty pills. Dose, from two to three *per diem.* But he soon relinquished this preparation for the more manageable, and more efficient protoiodide of mercury. This is undoubtedly one of the most valuable remedies we possess, and it is certainly that under the influence of which we can almost invariably modify, if we cannot cure, the syphilitic eruptions. This agent seems to acquire a double value from the combination of iodine with mercury. In the great majority of cases it is borne easily by the patients, and may be continued for a considerable period without causing any inconvenience. It seldom occasions salivation. Like all the mercurial preparations it may derange the digestive organs, and occasion diarrhea; but these accidents occur but seldom, are slight in their nature, and speedily disappear on the temporary suspension of the medicine. The skin is specifically influenced by the protoiodide of mercury. The patches of disease assume a more lively and healthy aspect, and evince a tendency to resolution. But the beneficial influence of the remedy is not confined to the skin, for the general condition and aspect of the patient undergoes a remarkable alteration. The countenance becomes more animated, and the eruption advances towards resolution with a rapidity which, in some instances, is really surprising. It is worthy of note, that when the administration of the protoiodide is likely to be followed by beneficial results, these latter will begin to appear in the course of a very few days from the commencement of the treatment. M. Cazenave relates a number of cases in support of the remedial efficacy of the protoiodide of mercury in the syphilitic eruptions. He has not in the least exaggerated the merits of this excellent remedy, for we ourselves have seen all that he has said in favour of it fully borne out in practice, in his wards at the Hospital of Saint Louis, and in our own practice in this country. We cannot avoid giving in full the following remarkable case related by the author, in favour of this method of treatment:

"M——, aged 30, holding a situation in a public office, was admitted into the Hospital of Saint Louis on the 15th of June, 1834, to be treated for an aggravated form of syphilis, which had occasioned a general cachectic state of body. This patient enjoyed good health up to the age of 18, when he contracted a blennorrhagia, accompanied by chordee, which was cured by antiphlogistic measures. In the space of eighteen months after this he contracted chancres twice. In one case he was cured by the internal administration of calomel, and in the other by simple emollients. Since that period he has been attacked six or seven times by

the same complaint, always at the base of the glans, and without concurrent ulceration of the mouth or throat. Six or seven years before his admission to the hospital he suffered from what he called an obstruction of the liver, accompanied by jaundice. Three years from his admission, and without any intermediate symptom, M—— was attacked by swelling and superficial caries of the frontal bone. These symptoms were treated by mercurial inunction, which occasioned violent gastric derangement. New tumours appeared upon different parts of the head, and disappeared without any treatment, and without occasioning injury of the bones. Soon after this the patient was obliged to perform a long and fatiguing journey from Strasbourg to Bayonne. Immediately after his arrival at the latter place he was attacked by severe pain in the head, congestion, and dyspnœa. The patient was bled freely, the original pains were removed, and a violent coryza ensued, unattended by inflammation of the nose. Emollient fumigations were applied to no purpose. The pain was renewed in an aggravated form, and thick scabs, accompanied by fragments of bone, were discharged through the nostrils, and at once declared the real nature of the disease. Three months after the appearance of the coryza a small pimple showed itself on the palate, broke, was converted into an ulcer, and spread rapidly. In two months the roof of the palate was perforated, and fragments of bone were discharged by the mouth and plates of the nasal bones through the nares. These symptoms disappeared under treatment by mercurial friction and Van Swieten's liquor, and the hole in the palate was partially filled up. A month afterwards the dyspnœa, congestion, and nasal hemorrhage were renewed, and small ulcerations appeared on the right ala of the nose and on other parts of that organ. The lesions of the palate and nasal fossæ reappeared and the right cheek was covered with tubercles, which were presently converted into irregularly-shaped ulcers, with sharp cut edges and grayish base. He now came to Paris, and was treated with Van Swieten's liquor, which only produced an exasperation of all the symptoms. Although his diet was confined to milk the constitutional disturbance continued, the disease proceeded with unabated violence, and the destruction of the palate was advancing rapidly. Iodine was administered for three months without the least benefit, but rather the reverse. The patient now entered the Hospital of Saint Louis in despair, without the slightest hope of cure, and only for the purpose of terminating, to use his own expression, a miserable existence, which he even repeatedly attempted to end by violent means.

"On his admission to the hospital he was emaciated, pale, his face hideous to look at, and where the skin remained intact it presented a yellowish colour. The whole of the nose, as far as the upper lip, was covered with thick yellowish-green scabs. These incrustations extended to the cheeks on either side, especially the right, and reposed on a well-marked copper-coloured base. A fetid sanious matter was discharged in abundance from fissures with which the scabs were intersected, and, on examination by the mouth, a horribly offensive smell issued from it, and the frightful ravages of the disease were brought into view. Almost the whole of the palate was gone, and the destruction appeared more extensive from the indented and furrowed edges it occasioned, and the gray malignant aspect of their borders. The left anterior portion of the superior alveolar case, deprived of teeth, was extensively destroyed, and presented the same kind of ulcerated surface as the exfoliated palate. The patient was moreover wasting from the effects of colliquative diarrhea, extremely irritable, prescribed his own treatment, and declared that he would not take mercury in any shape, to which he attributed his present unhappy condition. Nevertheless, Biett resolved to try the protoiodide of mercury, and, judging from former experience, with hopes of success.

"Accordingly, he deceived the patient by administering in the first instance small doses of opium, which were moreover of use in preparing the patient for the mercurial remedy. In a day or two the opium was omitted and the protoiodide

administered for fifteen days. The patient himself was astonished at the marvellous effects of these pills, under the influence of which suppuration ceased, the scabs fell off, leaving a depressed surface of a coppery-red colour, slightly squamous, and possessing the characters of a solid firm cicatrix. The destructive process going on at the palate and maxilla was arrested, the aspect of these parts was entirely changed, and even presented some points of cicatrization. Such was the state of things when a modified form of cholera broke out in Paris, spread through the hospitals, and, amongst others, M—— was attacked, but not severely. The protoiodide, which he had taken for fifteen days only, was now suspended for eighteen days. Nevertheless, he continued to improve, although more slowly than when taking the pills. When he was scarcely convalescent from this complaint the patient was attacked by swelling of the face from cold, which terminated in erysipelas. As soon as this affection disappeared almost the whole of the scabs on the face fell off, the parts beneath cicatrized completely, as also the ulcerations in the mouth. During the month of August gastric symptoms supervened, and the medicine was discontinued until the end of that month. However, the patient took thirty of the pills without our knowledge, which he purchased from one of the patients in the ward.

"An attack of varicella, which lasted for six days, again interrupted the treatment, but when this eruption subsided the protoiodide was again administered and continued up till the middle of October, at which period M—— was completely cured. The nose was not destroyed, but it was covered with cicatrices of a round, depressed, whitish appearance, as were also the other parts of the face attacked. In the roof of the palate there was an irregularly-rounded cavity, about the size of a half-crown piece, with cicatrized edges, and forming a free communication between the mouth and nares. The left side of the upper maxillary bone was destroyed as far as the two last molars, which were the only teeth remaining on that side. The general health of the patient was good: he was getting fat, the colour returned to his cheeks, and the skin lost the sickly tint it presented before. A plate of silver and platina was fixed where the palate was perforated, and the patient was discharged perfectly cured."

The author relates a number of cases of a similar nature, treated with the same remedy, and with like success. He usually administers the protoiodide internally in doses of one, two, three, to four grains *per diem*. In the mild, simple forms, not of long standing, M. Cazenave uses the following formula:

Protoiodide of mercury, ten grains,
Liquorice powder, thirty grains.

Make twenty pills. Dose: to begin with one, to be increased to two, and afterwards to four pills in the twenty-four hours.

In the severe and inveterate forms of the disease, as, for example, the tubercular varieties, where a more active and energetic method of treatment is indispensable, the author prefers the following:

Protoiodide of mercury, two scruples,
Liquorice powder, four scruples.

Make forty pills. To be administered in the same manner as the preceding.

It is sometimes necessary to commence with two pills, and to increase the dose rapidly. The mercurial preparation should not be prescribed in too small doses. The author has repeatedly observed this remedy unattended by any beneficial results when so administered; but, as soon as the dose was increased and given freely, it had the desired effect. Biett ascertained that when opium is given in combination with the protoiodide

of mercury, the therapeutic qualities of the latter are completely neutralized: hence we should always prescribe it in an uncombined form.

Notwithstanding the great and important advantages to be derived from the mercurial preparations in the treatment of the syphilides, it is but fair to add that in some cases they do not succeed. Whether from idiosyncrasy, the patient will not bear the medicine long enough, or from a repugnance to it, he will not allow it to be prescribed, or from morbid irritability of the digestive organs, the remedy cannot be administered at all; from whichever of these causes it occurs, there is no doubt but we are sometimes baffled in our attempts to relieve the patient. Other remedies have been recommended when the foregoing fails, but our faith in their efficacy is not great.

Acids. In some of the milder syphilitic affections, as S. roseola, S. papula, the exhibition of the acids is not unfrequently followed with benefit. The author orders nitric acid (dilute) in doses of four minims, three times a day, in barley-water or orgeat. In the severer forms it may be tried when the preceding medicine fails.

Gold. The preparations of gold are not of much service in the secondary forms of syphilis, and the author has very little faith in their remedial powers in the treatment of the syphilides. The preparations of silver, so strongly recommended by M. Serres, of Montpellier, have been frequently tested by Biett and M. Cazenave in these complaints, but always without success. The author recommends sudorifics as useful auxiliaries to the mercurial preparations, and has even found them occasionally beneficial when unaided by other medicine. Iodine in its simple form, although extolled by many writers for its efficacy in syphilitic secondary diseases, has invariably failed in the hands of M. Cazenave. But there are combinations of iodine, other than those already described, which are extremely useful in many cases: these are the iodide of iron and iodide of potassium.

Iodide of iron has been recommended by M. Ricord in secondary syphilis, and the author's experience leads him to believe that it may be sometimes useful, but not so frequently as M. Ricord imagines. This preparation appears to us to be objectionable from the chemical changes which it is liable to, for it is almost impossible to keep it from undergoing a certain degree of decomposition in the solid form, and even the solution, with a coil of iron wire to preserve it, cannot be kept long undecomposed. In our own practice we use a *syrup* of the iodide of iron, which continues for several months without undergoing any change. This preparation is made by several London chemists.

Iodide of potassium. M. Cazenave has found the iodide of potassium to be only second to the iodide of mercury in its valuable therapeutic effects in the treatment of the syphilitic eruptions. Indeed, he seems to think that in some instances it is fully as efficacious as the mercurial preparation. Although he has occasionally observed it to cause considerable pain at the epigastrium and posterior fauces, it can generally be continued six or seven weeks, or longer, with impunity. The author uses two formulæ, a stronger and a weaker, which are prescribed according to the condition of the patient, the irritability of the constitution, and the duration and severity of the particular eruption present.

Iodide of potassium, ʒij;
Distilled water, ℥xvj;
Syrup ℥ij.
Mix. Dose: two or three spoonfuls per diem.

Or the following:

Iodide of potassium, ʒij;
Syrup ℥vj.

Dose: to begin with one spoonful, then two, and subsequently three in the twenty-four hours.

Such are the various modes of treatment which Biett and Cazenave have found to be attended with most benefit at the Hospital of Saint Louis, in the venereal diseases of the skin. As a general rule the author preferred the mercurial preparations to all other remedies, and only had recourse to the different remedial agents noted above, when he could no longer administer the protoiodide or in cases when it was inadmissible from the first. The powerful effects of this remedy are more strikingly displayed in the tubercular forms, and those complicated with tumefaction of the soft parts and periostosis, than in any of the syphilides. Its efficacy in removing those unsightly protuberances of both skin and bone which characterize the tubercular syphilides, is oftentimes truly wonderful. We have repeatedly seen them vanish, as if by magic, and were as much astonished at the rapidity as the completeness of the cure. While we have such an agent at our command, we do not despair of alleviating, if not curing, the most inveterate form of this hideous disease, provided the patient will bear the medicine; and our past experience justifies us in all that we have said in favour of this heroic remedy.

The practitioner must always be guided in the selection of the treatment by the nature and form of the eruption, its duration, the particular constitution of the patient, and the anterior treatment. The acids are indicated in the semiacute varieties, especially in S. exanthemata, S. vesiculæ, and one or two forms of syphilitic lichen. Sudorifics are mostly of use in the pustular, but particularly the squamous eruptions, and, as we have before stated, the mercurial preparations are specially applicable in the most severe forms of the syphilitic eruptions. Whatever may be the treatment adopted, it should always be preceded for a certain time by what we may call preparatory measures, both hygienic and medicinal. This observation applies in particular to those cases where the mercurial preparations are to be administered. Biett always gave opium in small doses for a week or two before he prescribed the protoiodide of mercury, and generally with good effect, and during the administration of the latter, he recommended the occasional use of the vapour-bath, and sudorifics.

M. Cazenave has not much faith in topical remedies in the treatment of the syphilides. He seldom uses ointments, unless to dress an ulcerated surface, and then employs this formula:

Protoiodide of mercury, ℈j;
Prepared lard, ℥j. Mix.

The author has sometimes employed with benefit the following ointment in cases of syphilitic lupus, with the view of modifying the parts of the skin:

Biniodide of mercury, gr. xij;
Prepared lard, ℥j. Mix.

The author has never found any benefit attend the use of escharotics, except in one form of syphilitic tubercle (*tubercule plat*) which he has occasionally modified by the application of nitrate of silver. In these cases aromatic vinegar has also been applied with advantage; but as a general rule escharotics are useless and frequently dangerous in the treatment of the venereal eruptions.

Baths are extremely useful auxiliaries in the treatment of the syphilides. The vapour-bath and douche, are particularly serviceable in the papular, tubercular, and squamous varieties. Tepid baths, rendered emollient by the addition of starch and gelatine, are beneficial in certain forms of the exanthemata, in lichen, and in impetigo syphilitica. Alkaline baths are also useful in these forms, and in certain stages of the pustular syphilides, when the dryness of the scabs seem to indicate that the ulcers are cicatrized. Cinnabar fumigations are often very serviceable in the tubercular eruptions, when administered directly by means of an apparatus to the diseased parts. It is peculiarly applicable in cases of tubercle on the scrotum and about the anus. Dr. Burgess recommends a preparation of iodine and sulphur in the form of vapour in these and similar cases:* ℞. Sulphuris ʒiij; Hyd. sulph. rubri ℈ij; Iodinii gr. x. M. ft. pulv. sex. Dr. Burgess states that he has found this remedy exceedingly beneficial in the squamous and tubercular eruptions. It should be applied in the form of vapour, by means of an apparatus, to the parts affected.

M. Cazenave considers it indispensably necessary that the treatment should be continued for some time after the eruption has disappeared. It is impossible to lay down any precise rules as to the limits of this period, which it is evident must be regulated by the character of the preceding disease and the tact of the physician. If the eruption was mild, of short duration, and yielded easily to the treatment, say in the course of a month or six weeks, it should be continued for a month longer, at the same time, gradually diminishing the dose. If the disease, on the contrary, was of a severe and obstinate character, of considerable duration, and the treatment had occupied a period of several months, the patient should be allowed to repose for about a fortnight after the disappearance of the disease, and then the treatment may be recommenced, and followed in the same manner as before for a given time, then discontinued and begun again as in the first instance. It is sometimes necessary to discontinue the medicine three different times before we have done. It is a singular coincidence, and one worthy of being remembered, that a patient who may have borne the mercurial remedies well for several months without intermission, will suddenly, and after a discontinuance of the treatment for a few days, evince an almost invincible intolerance of the remedy employed. This is a sure indication that the treatment is complete.

We have now concluded our analysis of the 'Traité des Syphilides.' The importance, as well as the novelty of the subject in its present form, has led us to give as copious a review of M. Cazenave's work as our limits would permit. Still we have not been able to do it the full amount of justice which it deserves. M. Cazenave is, beyond all dispute, the first dermatologist of the day. His works on cutaneous pathology are models of their kind, alike remarkable for their clearness of arrangement, lucid

* Translation of Cazenave's Manual of Diseases of the Skin; with Notes and Additions. By T. H. Burgess, M.D.

descriptions and practical character; and amonst these the monograph before us takes a prominent position. The 'Traité des Syphilides' is the first attempt that has been made to collect and publish in a separate and complete form the various syphilitic eruptions. It is, in fact, an elaborate extension of Biett's well-known article on the 'Syphilides,' published in the author's 'Manual of Diseases of the Skin,' enriched by the author's own experience, and copiously illustrated by cases and coloured plates. We could point to more than one English work on 'Diseases of the Skin,' which has received the approbation of a considerable portion of the medical press, for the excellence of the chapter on the Syphilitic Eruptions, which has been borrowed partly or wholly from the article on that subject in M. Cazenave's Manual. The dauntless hardihood and recklessness of consequences displayed by the author of one of these piracies forbids us to shut our eyes on one of the most heroic achievements in plagiarism that has, perhaps, ever occurred in the history of medical literature. The 'Compendium of Diseases of the Skin' by Dr. Jonathan Green, published as an original work, contains an article on the Syphilides which, with the exception of two or three lines at the beginning, is a *mere translation* of M. Cazenave's article. How any man, pretending to a literary reputation, or to a reputation of any kind, could have been guilty of such wholesale appropriation of another's property we cannot pretend to divine. But so it is; and there it will remain, a lasting stigma on the author, and on the press, which not only tolerated but lauded it.

We most cordially recommend the 'Traité des Syphilides' to those of our readers who are familiar with the language in which it is written. They can judge for themselves of the merits of the work from the analysis with which we have furnished them; and we have merely to say, in conclusion, that it has been our text-book in the treatment of the Syphilitic Eruptions for some time past, and we have always found it a safe and faithful guide in practice.

Art. III.

Mémoires et Observations Cliniques de Médecine et de Chirurgie. Par L. Morand, m.d., &c.—*Tours,* 1845.

Clinical Essays and Observations in Medicine and Surgery. By L. Morand, m.d., &c.—*Tours,* 1845. 8vo, pp. 258.

This volume consists of memoirs on scrofulous ophthalmia, on hernia, on diphtheritis, &c., together with a series of reports of the more interesting medical and surgical cases that have occurred in the practice of M. Morand, a surgeon of Tours. To these is added a description of two new instruments invented by the author—an obstetrical forceps and a gum-lancet. Although the majority of the essays contained in this work are neither, in point of novelty or of practical importance, of a higher order than the average run of "Original communications" to journals—in which several of them have already appeared—yet as some of them contain facts that may be interesting to our readers, we will give a succinct analysis of the principal papers, taking them in the order in which they are presented to us by M. Morand.

Scrofulous ophthalmia. The first essay in situation as well as importance is on the coincidence of scrofulous ophthalmia with inflammation of the pituitary membrane, and on the necessity of treating the affection of the nose before undertaking the cure of the disease of the conjunctiva. This memoir, it appears, was laid before the Royal Academy of Medicine of Paris, and was favorably reported upon by Velpeau, who was appointed to examine into its merits. It consists of an account of an epidemic ophthalmia that broke out in 1841, amongst the children inhabiting some model-farms at Mettray. In the course of this epidemic, M. Morand's attention was especially directed to the state of the pituitary membrane, which he uniformly found to be in a congested or even inflammatory condition; and so intimately was this state of the mucous membrane of the nose connected with the conjunctivitis, that he could foretell the probable supervention of the latter disease on finding the nasal cavities reddened or irritated, and the Schneiderian membrane congested or inflamed. Before being affected with ophthalmia, many children likewise suffered from the ordinary symptoms of coryza. The continued observation of these facts led M. Morand to the conclusion that this condition of the nostrils preceded the inflammation of the conjunctiva, of which it was, as it were, the starting-point, and occasioned by its persistence the numerous relapses that his patients suffered from.

"It results, from my observations," says M. Morand, "that in scrofulous ophthalmia the olfactory membrane participates with the conjunctiva in the inflammation that is set up; that it is especially about the turbinate bones, and in the anfractuosities of the nasal fossæ that the inflammatory action resides, and that this shows itself in the form of an œdematous engorgement precisely similar to what is observed in the eyelids. The more I study this disease the more convinced am I that it is so. A little attention suffices to show that the redness and tumefactions of the pituitary membrane almost always precede or accompany that of the conjunctiva. This can be more positively determined by means of the speculum auris. On examining attentively the interior of the nasal fossæ, one cannot fail to observe that the redness and swelling of the nostrils, and even of the upper part of the lip, that are so commonly observed in persons of a scrofulous habit, are merely an evidence of the inflammatory action going on in that membrane. It is by proceeding in this way that we can best appreciate the degree and extent of this inflammatory action, the extension of which to the palpebral and ocular mucous surfaces is often very rapid; sometimes, however, it remains for a long time stationary, without showing any disposition to extend." (p. 3-4.)

From the repeated observation of the coexistence of the inflammations of the Schneiderian membrane and of the conjunctiva, M. Morand was led to adopt a new mode of treatment, which appears to have been pretty successful. Instead of applying his remedies to the eyelids or eyes, he attacked the disease existing in the interior of the nose. Before making the observation that has just been referred to, M. Morand had tried a variety of general and local means; two oculists of eminence had been consulted, but without avail; the ophthalmia resisted most obstinately, and cases continued to occur with a frequency that caused considerable uneasiness to the directors of the farms at Mettray. From the time, however, that he determined to attack the disease in the nostrils, the inflammation of the eyes speedily subsided, and relapses were much less frequent than before. The plan adopted by M. Morand was to cauterize the interior of the nostril of the side corresponding to the affected eye with

nitrate of silver, in the solid form or in solution, or as ointment, every morning and evening for the first week; after this once a day, and when the disease was declining, every second or third day. He enters a caution against cauterizing the aperture of the nose, and states that as soon as the caustic has been applied, the patient should make some deep inspirations through the nostrils, so as to spread the application over as large a surface as possible.

In an appendix to the memoir that we are now considering, M. Morand states that he has latterly had occasion to examine twenty-one patients with scrofulous inflammations of the conjunctiva, and that in nineteen of these cases he found the inflammation of the pituitary membrane to be more or less marked. He likewise observed that whenever there was an increased degree of irritation in the nasal fossæ, there was a marked exacerbation of the conjunctival inflammation, and frequently considerable tumefaction of the upper lip.

In order to determine the existence of the irritation of the pituitary membrane, it is by no means always necessary to look for redness of the nostrils and swelling of the upper lip; for, in the absence of these signs, it may easily be recognized by the constant flow of a more or less viscid mucus from the nostril that corresponds to the inflamed eye. It is also easy to ascertain the existence of this irritation by the introduction of the speculum nasi, or of a probe into the nasal fossæ, by which means any constriction or congestion may be readily detected. M. Morand has likewise found that the swelling of the upper lip, which frequently assumes an eczematous character, will very commonly yield to the application of the nitrate of silver, in one or other of the forms already mentioned, to the interior of the nostrils, when all other means have failed to relieve it.

It has long been known to surgeons that conjunctivitis is by no means an unfrequent complication of eczema of the upper lip and of the scalp, and the facts adduced by M. Morand in proof of the coexistence of inflammation of the ocular and nasal mucous membranes are interesting, as belonging to the same class with these; and although he may perhaps have overstated the frequency of this complication, yet the possibility of its occurrence should be kept in mind, and in all cases in which the ophthalmia is more than usually obstinate, it would be well not to overlook the condition of the nasal fossæ. With regard to the application of the caustic to the interior of the nostrils, instead of to the eye itself, we think, without recommending it, at all events, in every case, that it is a means that might be useful when the disease has evidently been primarily seated in the nose, or when, on account of the photophobia, it is difficult to separate the eyelids sufficiently to make the necessary local applications to the eyes: and there appears no reason to doubt that, if employed in conjunction with proper local and general treatment, it might materially tend to shorten the duration of attacks, and lessen the liability to relapse in cases of scrofulous conjunctivitis.

Cataract. The next subject that engages our author's attention is the question as to the propriety of operating for cataract when only one eye is affected. On this interesting point he adduces no arguments, but contents himself with relating the cases of two old men, on whom he operated by depression on one eye only, with perfect success; he does not mention, however, whether double vision followed the operation or not.

Epistaxis. We next come to what M. Morand is pleased to call a *new* means of arresting epistaxis, but which merely consists in plugging the nostrils with a piece of amadou rolled up in a piece of paper, instead of with lint or prepared sponge, as generally adopted.

Leech-bites. In order to arrest the hemorrhage from leech-bites, he recommends the application of a mixture of six parts of olive oil and two or three of yellow wax; this should be spread on a thin layer over the bleeding orifices, which must previously have been wiped dry.

There is nothing to detain us in the reports of two cases of *pericarditis* treated with blisters, colchicum, and antimonial wine; in two cases of *meningitis,* in which tartar emetic ointment was applied to the scalp with marked benefit; or in an instance of apparently *spontaneous gangrene* of the leg, in which amputation was had recourse to. Cases of this description, although interesting in themselves, are scarcely of sufficient importance to merit special notice here.

Fistula. The next case is intended to illustrate the advantages to be derived from the employment of stimulating injections for the cure of fistulæ, so situated that they cannot be laid open. It is that of a young woman who had suffered for nearly a twelvemonth from a fistulous opening in the right iliac fossa, consequent on an abscess in that region. After all the ordinary means of treatment had failed, it was resolved to try the effect of stimulating injections; for this purpose, a solution of the chloruret of soda was thrown in, with the best effect, the fistula being firmly closed in a fortnight.

Hernia. The next paper that we come to is a long memoir on the propriety of uniting by the first intention the wound that results from the operation for hernia. In this country it would, to say the least, be a work of supererogation for a surgeon to publish a memoir of from thirty to forty pages on the propriety of attempting, in certain cases, to heal the wound after an operation for hernia by the first intention, of bringing the edges together by a few strips of plaster, rather than dressing it from the bottom. In France, however, the case is different; the surgeon "leaves the wound open, and dresses it with a piece of perforated rag, spread with ointment and covered with pledgets of charpie and longitudinal compresses." (p. 79.) And it is amusing to see the hesitation with which M. Morand ventures to dissent from this practice. Although he admits that after union by the first intention, the cicatrix is small and linear, the rapidity of the cure is much increased, and the chances of those accidents that may complicate any wound diminished; whereas, by the after-treatment adopted in France, peritonitis, purulent absorption, and even tetanus are not unfrequent, and the resulting cicatrix is large, weak, and liable to excoriate. Yet he is far too patriotic not to come to the conclusion that "on reflecting on the advantages and disadvantages of these different plans, we cannot hesitate to follow that adopted by the French surgeons of the present day." (p. 79.)

We pass over without extract or comment, as presenting no feature of particular interest, the three reports that follow; one of a case of dothinenteritis, in which laudanum was administered with a view of allaying the delirium; the other a case of hooping-cough, complicated with an intermittent fever; and the third some cases illustrating the advantages of the "onguent Napolitain" in abscesses about the face; and proceed to the

memoir on diphtheritis, the longest, and one of the most important essays in the volume.

Diphtheritis. M. Morand relates eleven cases of diphtheritis, which are interesting on account of their having been treated almost solely by cauterization, with a solution of the nitrate of silver, and apparently with a fair share of success, only two out of the eleven cases having proved fatal. He is a warm advocate of the direct application of the caustic to the seat of disease in these cases, and thinks, with his townsman Bretonneau, that when this means fails there is no resource left but in the performance of tracheotomy. He employs a solution of nitrate of silver, in the proportion of one part of the salt to three of water, and applies this freely three or four times a day. By these means the inflammation is modified, the fibrinous exudation is lessened, and the extension of the disease to the bronchi prevented. Should the disease continue in spite of this, there is nothing left for the surgeon to do but to open the trachea, and the proper moment for doing this is one of the most difficult questions that can present itself in practice. If done too early the patient's danger may be increased by the possibility of the supervention of bronchitis in consequence of the operation; if, on the other hand, it be delayed until the early stages of asphyxia have supervened, the lungs becoming engorged, the bronchi loaded with viscid mucus, hematosis effected with difficulty, and the prostration of the vital powers great, it will be next to impossible to prevent the mischief in the lungs from proceeding to a fatal termination—the patient dying asphyxiated. Under these circumstances an operation would be useless, and we think that we are only expressing the opinions of the best surgeons, when we say that it will seldom hold out a reasonable chance of success if it be performed after the lungs have become at all extensively engorged. In order to maintain the wound in the trachea sufficiently open, M. Morand has invented a dilator, which, although it may attain its object tolerably well, appears to us, from the representation given of it, to be far too cumbrous for ordinary use, and rather to be applicable to the purposes of a speculum auris or nasi, for which it seems likewise to have been intended.

Incontinence of urine. For the cure of incontinence of urine, M. Morand recommends the extract of belladonna administered in the form of pills; each of which contains a fifth of a grain of the extract. For children of from four to six years of age, one of these pills night and morning may suffice; if, however, at the end of a week no effects are produced, he recommends another at noon, and at the end of another week, a fourth at bedtime. Children of from eight to fifteen years of age may begin with three a day, and carry the dose up to six or even eight; and adults may take from twelve to fifteen. This treatment, whatever its general success may be, has the disadvantage of being somewhat tedious, from three to four months being the average time required for a cure to be effected; and even then the disease is liable to relapse, as we learn from M. Morand's own statement.

As the remaining two or three essays contain nothing of general interest, we must conclude our notice of this small work, which, though it possesses fewer claims to originality than its author appears to think, evidently emanates from the pen of an acute and intelligent practitioner.

ART. IV.

Leçons sur les Phénomènes Physique des Corps Vivants. Par C. MATTEUCCI. Edition Française, publiée, avec Additions considérables, sur la Deuxième Edition Italienne.—*Paris,* 1847. Avec 18 figures.

Lectures on the Physical Phenomena of Living Beings. By C. MATTEUCCI. French Edition, translated, with considerable Additions, from the Second Italian Edition.—*Paris,* 1847. 12mo, pp. 406.

IN the year 1844, the Government of Tuscany charged Professor Matteucci with the duty of delivering at the University of Pisa a series of lectures on the Physical Phenomena of Living Beings. In establishing a distinct course upon this subject, the Tuscan Government exhibited a degree of enlightenment, which we should be glad to see reflected by the bodies that have the control of medical education in this country. No less sagacious was it in the choice of a professor; for it would have been difficult if not impossible, to find anywhere a man better fitted for such an office than M. Matteucci, who unites the highest experimental skill with extensive knowledge of this class of subjects, and who combines, in a very rare degree, the acuteness of a practised observer with the sagacity and generalizing powers of a true philosopher. His lectures were immediately published in Italy; and in a second edition, which was speedily called for, very extensive additions were introduced. The French translation has been made from the second edition, under the supervision of the author, who, from his long residence in Paris (where he acquired much of his proficiency in physical science), may be considered almost half a Frenchman; and he has made it the vehicle for the publication of his latest views on many of the subjects to which he has of late more particularly devoted his attention, including those which he brought forward at the late meeting of the British Association at Southampton. Consequently this French edition may be regarded as possessing the authority of an original work, instead of being crowded with the errors which too often disfigure such reproductions, especially among our Gallic neighbours.

We attach peculiar importance to the appearance of this little volume, for reasons which will more fully appear as we proceed. The lectures are in subject and style just what might be advantageously addressed to a class of students; whilst their conciseness will prevent them from taxing his powers of attention to any considerable extent. The only fault that we have to find with them, is that they go into somewhat too much of detail on the author's favorite subject, Electricity; and that other topics are consequently handled with undue brevity. We cannot but hope that they may be given to the British public in some shape or other; and this defect might then be easily rectified.

On the importance of a due acquaintance with the physical phenomena of living beings, as well to the scientific physiologist and pathologist, as to the practitioner who aims at nothing higher than the skilful application of the therapeutic art, a very few prefatory words will suffice. It has been elsewhere pointed out that there are in the living body three classes of phenomena; the first of them obviously *physical,* and not in any way

modified or checked by the vital operations, such as the action of muscles upon bones according to the laws of leverage, or the propulsion of the blood through the large vessels by the force-pump action of the heart equalized by the elasticity of their walls; the second class as obviously *vital*, being altogether different from anything which is seen in physics or chemistry, and referrible to a distinct category, such as the whole process of cell-development and reproduction, which may be considered as the fundamental type of this class of operations; and thirdly, an intermediate group, in which physical laws appear to be operating under peculiar conditions, which scarcely anything except a living organized body can supply, such as the change of venous into arterial blood in the pulmonary capillaries, the production of animal heat, and many chemical transformations. Of this last class of phenomena, a large proportion have been ranked in the second division; and by some physiologists they are still referred to it, the results appearing to them too far removed from those of any ordinary physical or chemical principles to be reasonably attributed to them. But although there are many phenomena, in regard to whose character doubts must long remain,—the peculiarity of the conditions under which they occur, making it difficult to imitate them experimentally,—there can be no question that the present tendency of chemical and physical inquiry is to limit more and more the domain of the peculiarly vital operations, by showing that wherever living beings come into contact (so to speak) with the external world, the laws of action which are dominant in the latter, exert their full influence in the bodies of the former; and that it is only within the *penetralia* of the system that the purely vital laws have free scope. Thus, to commence with the act of digestion, no intelligent physiologist any longer entertains a doubt that it is to be entirely explained on chemical principles; a conclusion which might have been anticipated from the simple anatomical fact, that the food whilst still within the intestinal canal is really on the exterior of the body, and can scarcely be regarded as more under the influence of vital operations than if it were in contact with the skin. Whilst a part of the alimentary materials undergoes simple solution, another portion undergoes very important transformations; and the nature of these is gradually being elucidated by experimental researches, whose whole plan is to imitate as much as possible the conditions under which the phenomena take place in the living body, and whose whole success depends on the degree in which that imitation is effected. Of this we shall presently meet with striking examples, when we come to notice the results of late inquiries into the mode in which fatty matters are reduced to a state fit for absorption, and in which amylaceous substances are transformed into sugar, lactic acid, or even into fat. In regard, again, to the absorption of these products, or their introduction into the living system, everything tends to show that a large part of the process takes place in strict conformity with physical principles; that which appears, from the observations of Mr. Goodsir, to be due to the vital operation of cell-growth, being probably far inferior in amount to that which takes place by simple imbibition, which, as the researches of Professor Matteucci have shown, will legitimately account for many phenomena previously supposed to be beyond the scope of its operations. When we examine these researches, we shall find that their success is due to the

same method of conducting them as that which has been so successful in Organic Chemistry; namely, the imitation, so far as is practicable, of the conditions under which the phenomena occur in the living body.

As soon, however, as the alimentary matter has been introduced into the circulating system it is subjected to a new set of influences. There is no check to the purely chemical transformations which were previously going on; but a new power comes into play, by which, with little or no appreciable change in the chemical constitution of certain of the nutritive materials, they are caused by a new arrangement of their components to present new properties, the crude unorganizable albumen being converted into spontaneously fibrillating plastic fibrine. Now we would not venture to assert dogmatically that this is a change never to be accounted for by any other than vital agency; but such is the view we are at present inclined to take of it,—the change being one which is peculiar to the living body, alike in its character, in its purposes, and in the mode in which it is effected. In its character, since we meet with no substance in the inorganic world, which is at all analogous to fibrine in its properties; in its purposes, since this transformation is evidently but a stage of preparation for those processes of more perfect organization which are altogether peculiar to living bodies; and in the mode in which it is effected, because there appears to be sufficient evidence for referring the transformation to the growth, development, and disintegration of successive generations of cells floating in the circulating fluid, in which, as we shall next attempt to show, the very essence of vital operations consists. The circulating fluid, having undergone its requisite preliminary elaboration, is converted by the process of nutrition into living organized tissues, endowed with various properties; some of which are simply physical (such as elasticity, penetrability by liquids, and the like); whilst others are unlike those which we meet with in any form of inorganic matter, and are, therefore, termed vital, for the sake of distinction (such as contractility, sensibility, &c.) Now, as we have argued on several former occasions, all these properties are alike in this,—that they are entirely dependent upon the peculiar arrangement of the material substances by which they are exhibited, and disappear as soon as that arrangement has ceased to exist; but they also differ in this,—that the physical properties may exist in forms of matter purely inorganic (as elasticity in steel, penetrability to liquids in sandstone), whilst the vital properties can only manifest themselves in structures that have been generated by a living body, and are maintained under conditions which it alone can supply (the contractility of muscle continuing to manifest itself only for a short time after the cessation of the circulation of arterial blood through it, and the sensibility of the nervous tissue being immediately suspended by any check to that function).

As the cell is the simplest type of an organized structure, and as the whole process of nutrition in the most complex organisms appears referrible to the laws of cell-development, we need not go beyond this, in examining how far it is possible to account for that process on purely chemical and physical principles. Although we here differ from Professor Matteucci, who seems to think that our knowledge of these will go far to explain the phenomena of cell-growth, we are bound to express our decided conviction that the most important and characteristic of these phenomena belong to

a category altogether distinct. There is, it is true, a certain analogy between the transforming power that a cell-germ appears to possess over the elements upon which it operates, (especially in the vegetable kingdom), and the *catalytic* action of many substances with which the chemist is familiar; there is a certain conformity between the phenomena of endosmose, and the gradual enlargement of a cell by the penetration of liquid into its interior; and there is a decided resemblance between the apparently homogeneous membrane which forms the cell-wall, and the pellicle which has been shown by Ascherson to be produced whenever oily matter and albumen come into contact. But even if it were granted (which we are not yet ready to do) that the selection of the peculiar contents of the cell and the transformation effected in them is a simple result of the chemical constitution of the germ or nucleus,—that the enlargement of the molecular point, in which no cavity can be detected, to a perfect cell, is due to the penetration of the matters thus attracted, according to the physical laws of imbibition—and that the cell-wall is merely the product of the contact of oil and albumen—we still ask what there is in the inorganic world which bears the remotest resemblance to the phenomenon of *reproduction* even in its simplest form (that in which the offspring constantly resembles its parent), still less in that extraordinary manifestation of it which we see in the course of the embryonic development of the higher animals, where the primitive cell first produces a congeries apparently similar to itself, but where this homogeneous mass gradually developes itself into an assemblage of heterogeneous organs and tissues, differing alike from each other and from their common ancestor in form and composition, but all having the most intimate relations of mutual dependence, and forming one perfect organism of high complexity, resembling that by which the germ was originally produced. All this appears to us so utterly devoid of the remotest analogy in the inorganic world, as to have an unquestionable claim to rank as a distinct order of phenomena; and we cannot stretch our imagination so far as even to glimpse at the possibility of including them legitimately in the same generalization with those of physics and chemistry.

But when we pass from the *constructive* to the *destructive* operations which are continually going on in the living body, we return to the domination of physics and chemistry. As soon as the process of disintegration commences in any tissue, the products of that disintegration seem to be no others than those which would elsewhere take place with the same materials placed under the same circumstances. Thus carbonic acid, which is the one most constantly generated of all these products in the living body, is the one which is most invariably produced by the decomposition of organized matter removed from it; and although the conditions of the production of urea, lithic acid, fæcal matter, and other excrementitial substances, are not yet fully understood, yet the demonstration which has been given of the possibility of generating them artificially, shows that the affinities which cause their elements to combine in the proportions and modes severally peculiar to them can be no other than chemical. Of the act of respiration, by which the carbonic acid of the system is set free, and that measure of oxygen is introduced which is requisite to excite the various operations of the tissues by its chemical action upon them, it is

almost superfluous to remark that it is a process of a purely physical character; its peculiarity chiefly consisting in the state of extremely minute division in which the blood is exposed to the air, and in the peculiar power of absorbing gases which that liquid possesses. Of the process of secretion, which brings us back again from the penetralia of the organism to the external world, we do not yet know enough to separate clearly the phenomena which are due to simple transudation from those which are the result of the vital act of cell-growth; and here there is a wide field open for inquiry which is almost sure to be productive. Many of the fluids poured forth within the living body agree so closely with the serous portion of the blood in their general characters, that it would appear scarcely necessary to attribute their separation to anything else than an act of transudation; whilst for those which are more particularly destined to be cast at once out of the body, and which consist chiefly of substances alien to the normal constitution of the blood, the selecting power of cells appears to be required.

From these preliminary remarks, we trust it will appear that no scientific physiologist can have any excuse for neglecting to make himself acquainted with all that physics can contribute to the elucidation of the phenomena of living beings; and that such knowledge is also of the highest value to the practitioner, whose influence is chiefly, if not solely, exerted over those processes in which physics and chemistry have the greatest share. He cannot himself reach the mysterious *penetralia* of the system; but he can tread its outer courts, and exercise much control over the ingress, and watch the egress of its ministers; and in proportion as he becomes acquainted with the respective offices of these, may he regulate at his will all that takes place within.

We shall now enter upon a somewhat detailed examination of Professor Matteucci's treatise; confining ourselves, however, almost entirely to those topics which are least perfectly treated in physiological works, and especially dwelling on his original observations. After a general introduction, the first subjects discussed by Professor Matteucci are Capillary Attraction, Imbibition, Endosmose, and Absorption; which are all referrible to the same category, and a due acquaintance with the physical phenomena of which is indispensable to the physiologist who would seek to determine the purely vital powers concerned in the introduction of fluids into the living tissues. As these topics are handled with peculiar skill, and many new and important observations are brought forwards, in the treatise before us, we shall dwell upon them somewhat at length.

We should scarcely think it necessary to remind our readers of the ordinary phenomena of Capillary Attraction, to which Professor Matteucci adverts in the first instance, did we not know that many vague and incorrect notions are prevalent respecting it. When a solid body is plunged into a liquid, the latter is *elevated* or *depressed* around the solid, according as the substance of the latter *is*, or *is not*, moistened by the liquid; thus, if a glass rod be dipped into water, the surface of liquid will be elevated around it, whilst, if it be dipped into mercury, the surface will be depressed. In the same manner, if a tube of small diameter and open at both ends be plunged into the liquid, this will be raised or depressed in a degree which increases with the diminution of the diameter of the tube;

thus, in a tube of 1 millimeter in diameter there will be an elevation of water to the height of 30 millimeters, whilst there will be a depression of mercury to the depth of 13 millimeters. That this force should play an important part in the introduction of liquids into living tissues will be easily perceived, when it is borne in mind that the parietes of their vessels are peculiarly susceptible of being moistened by aqueous fluids, and that their calibre is commonly not more than from 1-100th to 1-200th of a millimeter. (A millimeter is about 1-25th of an inch.) These phenomena are entirely independent of the thickness of the walls of the tubes; they are not influenced by the pressure of the surrounding air, but will take place equally in a rarefied or condensed atmosphere, in any other gaseous medium, or in a perfect vacuum. They are influenced, however, by temperature; the elevation or the depression of the column of liquid diminishing with an increase of heat, the tube and liquid remaining the same. The degree of elevation or depression has no relation with the density of the liquid; thus, if we take water as the standard, and represent its elevation in a given tube by 100, that of alcohol will be 40, and of essence of lavender 37, whilst that of a saturated solution of common salt will be 88. It is very important to bear in mind that simple capillary attraction will never cause a liquid to *flow over* from the top of a tube, although its summit be much lower than the elevation which the same liquid would attain in a tube of corresponding diameter; this is easily understood when it is borne in mind that there is nothing to continue attracting the fluid, when once it has been raised to the summit of the tube. The case is very different, however, if the liquid be carried off by any agency existing at the end of the tube; for it will then be drawn up and replaced as fast as it is removed. This is the case in the action of the wick of a lamp or candle, which continues to draw up the oil so long as the liquid is got rid of by combustion from the summit of the wick, but ceases immediately that this process is checked and the capillary tubes remain saturated. So when a bundle of cotton threads is allowed to dip into water, and to hang down over the side of the vessel, a continual dripping takes place at its pendent extremity, simply by the agency of gravity, and the supply is kept up through the agency of capillary attraction; but let the cord be held in a horizontal position, and the overflow ceases.

"To avoid any false application of the phenomena of capillary action to the animal economy, it must be constantly kept in view that a space which is already completely filled with liquid is incapable of exercising any capillary action whatever; that the action of a capillary tube on liquids is due less to the substance of the tube, than to the nature of the liquid with which its internal surface is moistened; and lastly, that it is never by the agency of simple capillarity that liquids are caused to flow forth from the upper end of the tubes into which they are raised." (p. 19.)

Many of the phenomena of capillary action are best studied in that form of it which is commonly termed imbibition, and which consists in the introduction of fluid into porous substances. These may be regarded as channeled out by an immense number of capillary canals, into which the liquid is attracted; and the same law holds good here, as in ordinary capillary action, that the readiness with which the solid substance may be wetted with the liquid, or, in other words, the degree of molecular attraction

subsisting between them, is the chief regulator of the result The following experiments, which were performed by Professor Matteucci in conjunction with Professor Cima, appear to us to deserve peculiar attention. Six tubes, each about 8-10ths of an inch in diameter, were filled with finely-sifted and well-dried sand, and their lower ends, having been tied over with cloth to prevent the escape of the sand, were then immersed to the same depth in different liquids, and allowed to remain there for ten hours, care being taken to keep up the surface of the liquids surrounding the tubes to the same height. At first the rise of all the liquids took place rapidly; but its rate soon diminished, and the retardation continued until the stagnation became completed, the point of saturation having been reached. All the saline solutions employed had the same density. The following are some of the results of these comparative experiments, as expressed by the height to which each liquid rose in the cylinder of sand:

	Millim.
Solution of carbonate of soda	85
sulphate of copper	75
Serum of blood	70
Solution of carbonate of ammonia	62
Distilled water	60
Solution of common salt	58
White of egg, diluted with its volume of water	35
Milk	55

When thick solutions of gum or starch, or fixed oils, were employed, the amount of imbibition was almost nought; and it was but little more when strong saline solutions, or liquids holding finely-divided particles of solid matter in suspension, were employed. This last fact may assist us in explaining why serous infiltrations take place much more readily in the living body, when the density and viscidity of the circulating fluid, and its quantity of solid particles, have been diminished by loss of blood or other causes.

The following experiment demonstrates in a striking manner the degree in which imbibition is affected by the peculiar attractions existing between the solids and liquids employed. Three tubes, respectively filled with sand, pounded glass, and sawdust, were immersed at their lower extremities in water, and three similar tubes in alcohol; the following were the comparative results:

	Tube with sand.	Tube with pounded glass.	Tube with sawdust.
Alcohol	85 millim.	175 millim.	125 millim.
Water	175	182	60

Thus we see that, whilst the imbibition of the two liquids was nearly the same in the pounded glass, more than twice as much water as alcohol was drawn up by the sand, and more than twice as much alcohol as water was drawn up by the sawdust. Another experiment showed that when the substance was closely pressed in the tube, the amount of imbibition was much increased, as we should expect, from the diminution in the size and the increase in number of the capillary channels, under such circumstances. One of the most remarkable results, however, was obtained by varying the temperature. Two tubes, similarly prepared with sand, were placed in water, the one at a temperature of 59°, and the other at 131°. After 70 seconds, the water had risen 10 millimeters in

the former, and only 6 in the latter; but the difference was far more striking after a longer period; for in 11 minutes the water had risen 175 millimeters in the former, and only 12 in the latter. It is impossible not to perceive in this result how much the influence of temperature upon the activity of animal and vegetable life may be explained on simply physical principles. It must be for the mathematician to combine these curious results with those other phenomena of molecular action which have been already alluded to, and which present a series of problems of the highest interest, the complete resolution of which can scarcely yet be anticipated; but the general fact, in its present state—namely, that simple physical imbibition is greatly accelerated by warmth and retarded by cold,—is quite sufficient for the purposes of the physiologist.

The subject must not be dismissed without adverting to the remark made by Professor Matteucci, that the effects of chemical affinity may be developed by the simple play of the capillary forces and of molecular attraction.

"If we reflect that a liquid of any kind constantly elevates itself to the same height in a capillary tube,—that during the imbibition there is always a greater or less production of heat, as the experiments of Pouillet have shown,—that there is, moreover, according to Becquerel, a disengagement of electricity,—and that, lastly, capillary attraction is only exercised at very minute distances, and between the molecules of bodies,—we cannot but admit that this force unites the principal characters of chemical affinity. The observation of Dobereiner's is well known, that if a mixture of water and alcohol be inclosed in a bladder and exposed to the air, the water continually escapes, and the alcohol is retained. In this case, the water is drawn into the membrane more readily than the alcohol, and is dissipated on its exterior by evaporation. Another more conclusive fact is that stated by Berzelius; namely, that salt water, if filtered through a long tube filled with sand, is discharged more or less completely deprived of salt.* I have myself confirmed this experiment, employing for the purpose a tube full of sand about 8 meters (26 feet) in length; and I have found, in fact, that the density of the water issuing from the tube was to that of the liquid introduced by its superior orifice, as ·91 to 1·00. But it must be added, that this difference did not continue in the same degree; at the end of a certain time the saline solution was as dense at its exit from the tube as at its entrance. This proves that the decomposition of the saline solution takes place in the first action of contact between the particles of sand and itself. I have obtained an inverse result, by employing a solution of carbonate of soda, which I caused to traverse a tube full of sand, 10 feet in length. The density of the liquid at its exit from the tube was to that of the liquid as it entered it as 1·005 to 1·000." (p. 29.)

If the solution of saline substances in water is to be regarded as a result of chemical affinity, then unquestionably their separation must be regarded in the same light. But we are not sure that such a mode of viewing the phenomena is correct. The facts themselves, however, are of the highest physiological interest; because they show us how, by a process of simple filtration, an important change may be effected in the proportions of the components of a solution. Thus we see that, in the case mentioned by Berzelius, the saline matter was drawn from the water by the attraction of the particles of sand, until probably every one of them had acquired a thin coating of it and could attract no more; whilst in the second case,

* Is not this the explanation of the frequent occurrence of springs of fresh or brackish water near the sea, in circumstances where collections of fresh water could scarcely be supposed to exist?—REV.

the water appears to have been attracted more powerfully than the carbonate of soda dissolved in it, so that the latter was allowed to pass on, whilst the former was partially retained. We thus see in the ordinary separation of the thin serous fluid, which distends the cavities of areolar tissue, and bedews the surfaces of serous membranes, and which is like a diluted blood-serum, nothing but a process of filtration; which allows the more aqueous part of the liquid to escape from the vessels, whilst the fibrine of the liquor-sanguinis, with a good part of the albumen and saline matter, together with the floating corpuscles, are kept back.

The phenomena of endosmose next claim our attention, constituting, as they do, a group which borders most closely on the domain of purely vital operations. We find Baron Liebig giving utterance to what appears to us a most unjust sneer at physiologists, for their supposed manner of viewing this subject:

> "The commonest phenomena are even yet personified in the minds of many physiologists as peculiar powers, as properties which they are tempted to explain by peculiar causes, differing from other known causes. Thus the restoration of equilibrium between two fluids of different nature, or two solutions of dissimilar substances, separated by an animal membrane, has obtained the names of endosmose and exosmose; and men regard these names as if they were independent things, while the phenomenon is nothing else than a filtration, different from the ordinary one only in so far as that the passage is effected by an attraction (a drawing, an affinity) instead of by pressure." (Animal Chemistry, 3d Ed. p. 165.)

Now we are not aware that the terms endosmose and exosmose have ever been used in any other sense than as the expressions of certain general facts,—namely, as the words import, the existence of an inward and an outward current, under certain conditions. Every one admits that these currents are due to molecular attractions of the same nature with those concerned in the ordinary operations of capillarity and imbibition; but with the alteration in the conditions there is a marked alteration in the results, and physical science has not yet succeeded in fully accounting for the phenomena. These phenomena, however, form a group so distinct, that they may be all conveniently embodied under one general term; and we cannot think of one more appropriate than that which was applied by Dutrochet, who was the first to draw attention to the phenomena. Thus if we say that the form of the blood-corpuscles may be changed by endosmose, we express in a concise way the fact, that if they be placed in pure water or in diluted serum there will be a passage of fluid towards their interior, which will distend and even burst them; whilst if they be placed in a solution of salt or sugar, of greater density than their own contents, the chief current of fluid will take place in the other direction, and the blood-corpuscles will be emptied. Thus the term is used to express the *proximate conditions* of a certain phenomenon, which it brings definitely before the view of the mind. With the *ultimate causes* the physiologist has nothing to do, until physical investigation shall have determined them; which, as we have the authority of Professor Matteucci for asserting, has not yet been effected. For although it might not seem difficult to give a general explanation of the fact, that two liquids of different densities, disposed to a ready admixture with each other (such as syrup or mucilage, and water), should pass towards each other when

separated by a porous membrane, and that the more rapid current should be that of the rarer fluid towards the denser,—there are many variations and exceptional phenomena, for which no such general explanation is adequate to account. For instance, when alcohol and water are employed, the principal current or endosmose is from the water towards the alcohol, although the latter is the less dense of the two. A fact still more difficult of explanation, is the agency of sulphuretted hydrogen in immediately checking the process. As soon as the least putrefaction commences in the membranous septum, the endosmose ceases, and the liquid returns by filtration; and if a fresh membrane be exposed, even for a short time, to sulphuretted hydrogen, no endosmose will take place through it, even between two liquids ordinarily most energetic in their action on one another. In like manner, the introduction of a very small quantity of sulphuretted hydrogen into the liquids employed is sufficient to retard or check the process, even though these liquids, when pure, are powerful supporters of the endosmotic current. And this is probably the reason why the liquids of the large intestine should be an exception to animal fluids in general, in having no endosmotic action with water. One of the most remarkable of all exceptional cases is that of hydrochloric acid. When this liquid has a density of 1·020, the endosmose takes place from the water to the acid; whilst at the density of 1·015, the chief current takes the contrary direction, being directed from the acid towards the water. But if the temperature of the liquid be raised above 68°, the endosmose again takes place in the first direction.

It was attempted by Dutrochet to determine the relative endosmotic powers of different solutions of the same density; that is, the relative energy with which they would cause a current of water to pass towards them through the membranous septum. Thus, he states the powers of solutions of gelatine, gum, sugar, and albumen to be to each other respectively as 3, 5, 11, and 17. But these results are true only for one kind of membrane; for, as the researches of Professor Matteucci have shown, different membranes exercise very different influences over the endosmotic process; nor is it immaterial which side of the membrane is exposed to the water, and which side to the solution. Professor Matteucci's experiments have been made upon three classes of membranes; cutaneous, gastric, and vesical. Of the first class he examined the actions in the skin of the frog, the eel, and the torpedo. It was with the skin of the torpedo that he first discovered the important influence effected by the position of the two sides of the membrane. When the external surface of the membrane was in contact with the solution, the rise of the liquid was rapid; whilst if the internal surface of the membrane were in contact with the solution, the rise of the liquid was very slow in comparison. Thus a solution of gum would rise 30 millimeters, and a solution of sugar even 80 millimeters, in the tube of the first endosmometer; whilst the same solutions would only rise 18 or 20 degrees, or even much less, in the second. In like manner a solution of albumen would be raised 26 millimeters in the first case, whilst it rose only 13 in the second. The same results were obtained with the skin of the frog. When the skin of the eel was employed, it was noticed that the immediate rise was the same, when a saccharine solution was employed, whichever side of the membrane was

turned towards it; but the same inequality soon manifested itself, as in the preceding cases. When a solution of gum or of albumen was employed, the difference was immediately apparent. When the same comparative experiments were tried with alcohol (the passage of water towards which is itself an exceptional phenomenon) still more curious results were obtained. When the skin of the frog was employed, the endosmotic current was found to be most active when the internal surface of the membrane was turned towards the alcohol; but the contrary was the case when the skin of the eel or of the torpedo was used as the septum, the endosmotic current passing most readily from the exterior towards the interior. Some curious results were also obtained on watching the successive changes in the elevation of the column, from hour to hour; from which it appears that the membranes, when no longer fresh, have a different action on the fluids from that which they exert at first. Upon these, however, we cannot dwell; and we shall only stop to notice the following results of a long series of comparative experiments undertaken to determine the relative endosmotic actions of these three membranes upon solutions of sugar, albumen, and gum, and upon alcohol. In each case the *external* surface of the membrane was in contact with the liquid.

	Skin of eel.	Skin of frog.	Skin of torpedo.
Solution of sugar . .	15	25	100
albumen . .	8	15	30
gum . .	6	22	120
Alcohol . . .	55	80	35

"This table proves, 1st, that with the skin of the torpedo the endosmotic current is the strongest, if we employ as the interior liquid a solution of sugar, gum, or albumen; 2d, that with these same liquids, the current is much less powerful in the skin of the eel than in that of the frog; 3d, that when alcohol is employed, there is a powerful endosmotic current from the water to the alcohol, which is much stronger than the similar current in the eel, and stronger in this than in the torpedo; 4th, that this powerful current across the skin of the frog takes place in spite of the position of the skin, which is not the one most favorable to the passage of water towards the alcohol; 5th, that the intensity of the endosmose, in the case of any one skin, varies according to the liquids employed; the order, from the highest to the lowest, being as follows:

"*Skin of torpedo.* Solution of gum, syrup, alcohol, solution of albumen.

"*Skin of frog.* Alcohol, solution of sugar, gum, albumen.

"*Skin of eel.* Alcohol, solution of sugar, albumen, gum.

"These last results prove that the order in which Dutrochet arranged these liquids, according to the intensity of the endosmose which takes place between them and water, cannot be regarded as holding good in all cases; we shall see that it cannot even be regarded as invariable in the single case of the membrane of the urinary bladder, which was the one employed by this able experimenter." (p. 51.)

Similar differences presented themselves in the relations of the membranes of the second class to the several fluids; but we shall only notice the influence of the difference of surface in contact with the liquid, in the case of alcohol, where it is very strongly marked. Thus when the membrane of the stomach of the cat had its external surface (i. e. the surface in contact with the muscular coat of that viscus) turned towards the interior of the endosmometer, and consequently placed in contact with the

alcohol, the elevation of the fluid in the tube was 22 millimeters; whilst it was no more than 2 millimeters in the contrary position of the membrane. The same disparity showed itself when the membrane of the stomach of the dog was employed; the elevation being as much, in one experiment, as 130 millimeters in the most favorable position of the membrane, and only 6 in the least. The stomach of the lamb gave a similar, but rather less striking result. For most of the other liquids, however, the position of the membrane most favorable to endosmose was that in which its free or mucous surface was turned towards the fluid contained in the endosmometer. But a still more curious variation is presented by the fact that when the lining membrane of the gizzard of the fowl is employed as the septum between water and alcohol, the direction of the endosmotic current is altogether reversed, whatever be the position of the membrane; the flow being now invariably from the alcohol towards the water, but being more intense when the internal or free mucous surface was in contact with the alcohol. Of this remarkable fact Professor Matteucci states that he has fully satisfied himself by repeated and careful experiments.

When the membrane of the urinary bladder of the ox was employed, in a fresh state, with a solution of sugar, it was found that the endosmotic current took place most readily from the attached towards the free surface of the membrane; whilst the contrary was the case, when a solution of gum was substituted for the sugar. With the gummy fluid the following curious phenomenon was further observed: that, during the first hour of the experiment, the fluids frequently fell in the endosmometers, though they subsequently rose much above their original elevation. When a solution of albumen was employed, it was found that *no* endosmose took place through the fresh mucous membrane of the urinary bladder, when pure water was on the other side of it. All these curious differences vanish, however, or undergo great modifications, when, instead of fresh membranes, those which have been dried, or which have undergone more or less alteration by putrefaction, are employed. It is evident that the method of experimenting adopted by Professor Matteucci greatly increases the value of his results to the physiologist; since it gives us the nearest possible representation of the operations of *living* membranes under similar circumstances.

It appears, from other experiments devised by Professors Matteücci and Cima, that the elevation of the fluid in the endosmometer cannot be regarded (as Dutrochet supposed that it might) as an absolute measure of the difference between the stronger or endosmotic and the weaker or exosmotic current. Two endosmometers were prepared in the usual way, with the skin of the frog or of the eel; the external surface being directed towards the contained fluid in one instrument, and towards the water in the other. The endosmometer was then filled with a solution of salt, which allowed the subsequent changes to be more readily determined than could have been effected with a solution of gum or albumen, and was immersed in its own bulk of distilled water. After the experiment had continued for some hours, it was found that (as in previous instances) the elevation of the fluid was greatest in that endosmometer in which the internal surface of the skin was in contact with the water. It might be expected that the saline solution in this endosmometer, being now of

greater bulk than that contained in the other, would have experienced more dilution, and be found to be of inferior density. But the contrary was the case; for the saline solution which stood the highest in the endosmometer was found to have the greatest density; whilst the water in which it had been immersed, and of which the diminution in bulk was the greatest, contained a quantity of saline matter much less than that which surrounded the other endosmometer, where the interchange appeared to have been much less. Thus it became evident that the elevation of the liquid in the tube of the endosmometer cannot be regarded as at all indicative of the positive energy of either current; and we agree with Professor Matteucci in thinking that the results of the experiments already cited, in regard to the different influence of the two sides of the membranes employed, seem best explained on the hypothesis that the endosmotic current is the same, or nearly the same in the two positions of the membrane, whilst it is the exosmose which chiefly varies, being weakest in the endosmometer which presents the highest elevation, and *vice versa.*

We think that our readers will agree with us as to the physiological importance of these facts; since it is impossible but that they should have a definite and constant relation with the phenomena of the living body. No one can be more sensible, however, than Professor Matteucci himself appears to be that the subject is at present but very imperfectly elucidated; and that a vast number of new experiments are required, to develope the laws of the process, and the modifications induced in it by the variations in the properties of the several membranes of the body in different animals. That the endosmotic properties of these membranes have a relation to the purpose they have to serve in the living economy is sufficiently obvious from the following considerations:

"The exosmose of solutions of gum, sugar, and albumen, towards water, takes place most readily from the internal to the external surface in all the cutaneous membranes we have examined. It is precisely in this direction that an abundant mucous secretion traverses the skin of the frog, the eel, the torpedo, and other animals. The endosmose of water towards these solutions is less considerable from the external to the internal surface of the skin, than when it takes place in the contrary direction. Consequently, if we do not admit that we may entirely explain on this principle the direction of the mucous secretion, and the feeble absorption of the water in which these animals live,—functions which must always bear a certain relation to one another to take place with regularity,—at least it cannot be denied that these physical conditions are favorable to them. It is evident this function of the skin would be executed imperfectly, or would cease altogether, if, in animals which constantly inhabit water, this membrane acted by endosmose in a direction opposed to that in which we have found it to operate most powerfully.

"We may pass by the opposite result which is shown, when water and alcohol act on one another through the skin of the frog. Alcohol is a liquid which has no analogy among those which are found in the bodies of animals; and the anomalies which we have met with in employing it as an endosmometric liquid, if they be true also of the skin of man, would find their application rather in therapeutics than in physiology.

"The constancy observed in regard to the most favorable direction for endosmose and exosmose through the skin, is no longer experienced when we employ the mucous membranes of the stomach of different animals. But no one is ig-

norant how complicated is the function of the stomach; nor that of the substances which are introduced into the organ, some are not absorbed at all, and others very slightly. Moreover, we again repeat that this subject particularly requires being elucidated by new researches. When we remark that the direction most favorable for endosmose, between water and solution of sugar, for example, is not the same for the stomach of a ruminant and for that of a carnivorous animal, it is clearly demonstrated by this fact that the phenomenon of endosmose is intimately connected with those essential modifications with which the digestive functions are intimately connected in these two orders of animals." (p. 69.)

"I must not conclude without citing to you the recent experiments of Poisseuille, with a view of explaining by endosmose the purgative action of certain saline substances. He found that endosmose took place through the animal tissues from the serum of the blood towards Seidlitz water, solutions of sulphate of soda, common salt, &c. This is precisely what occurs when these medicines are administered internally. The excrements contain an abundant and unusual amount of albumen; and we can scarcely help admitting that endosmose takes place from the serum of the blood to the saline solution introduced into the intestinal tube, through the walls of the capillary vessels of the latter. But to remove all doubt of the justice of this application of the doctrines of endosmose by Poisseuille, it was necessary to demonstrate that endosmose would continue when one of the liquids is in motion, and is continually being renewed. This has been recently proved by Dr. Bachetti, who has shown that the rapidity of the endosmose is considerably augmented when one of the liquids is in this state of continual renewal. This result, moreover, is in accordance with the principles of the theory of endosmose; for the exchange of liquids constantly taking place through the membrane certainly tends as it proceeds to diminish the force of endosmose; or, in other words, the conditions which produce the phenomenon are maintained so much the better, as the liquids remain longer without admixture. Poisseuille also has shown that endosmose ceases to take place through a membrane, after a certain period of activity; but that the property may be excited anew by the agency of other liquids. The most remarkable fact discovered by Poisseuille, is that of the influence exerted by muriate of morphia. This substance, added to saline solutions, considerably weakens the endosmose of the serum towards the solution, and finishes by changing the direction of the current. This fact has been confirmed by Dr. Bachetti." (p. 71.)

How, asks Professor Matteucci, can such a fact as this be overlooked, in the explanation of the agency of morphine and of the preparations of opium in checking diarrhœa and producing constipation? And what a new field of inquiry is opened up to us, when it is shown that these supposed ultimate facts in the physiological action of medicines are so clearly to be referred to physical conditions!

In the application of these principles to the phenomena of absorption in animals and plants, which occupies the fourth lecture, we do not find much that need detain us; since it must be at once obvious to those acquainted with those phenomena, how large a part of them may be referred to physical laws. And it is further apparent, that a proper knowledge of these laws would have prevented the diffusion of many grave physiological errors; such, for example, as the exclusive absorbent power of the lymphatics and lacteals. For the permeability of the animal tissues once being demonstrated, and the influence of the movement of fluid through membranous tubes in attracting towards them fluids external to those tubes having been established, it follows as a matter of course that the sanguiferous as well as the absorbent system must participate in the imbibition of fluid; and that the venous system will be much more con-

cerned in it than the arterial, on account of its more superficial position, the greater extent of surface of its vessels, the comparative thinness of their parietes, and the inferior pressure of the circulating fluid against their walls. The permeability of the animal tissues *en masse* is readily established by such an experiment as the following. If a frog recently dead have its hind legs only immersed for some hours in a solution of prussiate of potass, and it be then cut into fragments, it will be found that if any part of its body be touched with a rod dipped in a solution of sulphate of iron, a blue spot will immediately be formed; showing the universal diffusion of the prussiate of potass through its tissues. If the same experiment be tried upon a living frog, the same result will be obtained. If the immersion be continued, however, for a short time only, there will be found but little indication of the prussiate of potass in the muscular masses of the limbs, whilst it is met with abundantly in the heart and lungs; showing that, in the living animal, the imbibition and diffusion take place most speedily through the circulating apparatus.—The influence of the movement of the fluid within the vessels is shown by the following experiment. If we take a long portion of one of the principal veins of some large animal, and fix it by one extremity to a tube proceeding from an opening at the bottom of a vessel filled with water, whilst we attach a stopcock to the other extremity and if, with this stopcock closed, we immerse the surface of the vein (which is distended with water from the reservoir) in water acidulated with sulphuric or hydrochloric acid, it is some time before the liquid in the reservoir presents any traces of the acid. But if, instead of waiting for the appearance of the acid in the reservoir, we open the stopcock, and allow the water to discharge itself, we immediately discover the presence of the acid in the liquid which has thus been made to flow through the vein. The same result would present itself with a tube of clay or other inorganic substance; the phenomenon being one of a purely physical character.

Professor Matteucci is far from maintaining that *all* the phenomena of the introduction of fluids into the tissues of animals and plants can be explained upon physical principles; and he does not appear to us to have any clear idea as to the point at which the line is to be drawn. For ourselves we are disposed to think that a very large part, if not the whole, of the phenomena of vegetable absorption and of the ascent of the sap may be thus accounted for, the conditions under which they occur being duly kept in view. The vegetable physiologist is well aware how completely the continuance of active absorption by the roots is due to the exhalation of fluid by the leaves; the former process being brought to a stand almost immediately that the latter is checked, and being found to recommence immediately upon the renewal of the latter. Hence it would appear that the rise of the sap in the stem has the same kind of dependence upon the dissipation of the greater part of its fluid through the agency of the leaves, that the rise of the oil in the wick of a lamp has upon the combustive process taking place at its summit. And we find, moreover, that this ascent continues through the upper part of the stem and branches, when they are severed from the trunk and roots, and have the cut surface immersed in water; which shows that the continued ascent is not dependent on any *vis a tergo* in the roots, but on a *vis a fronte* in the leaves. But,

on the other hand, if the roots be immersed in water, a continued discharge of fluid will take place for some hours through the cut surface of the stem in which they terminate; showing that the absorption continues through their spongioles. We are disposed to think with Dutrochet that this is a pure act of endosmose; and it seems to us that the admixture of the viscid elaborated sap with the crude fluid just absorbed, which is known to take place in the roots, is the very condition required for maintaining the density of the fluid within the tissues of the plant, at that point above the density of the fluid in the soil around, which is a necessary condition of endosmose. If such be the fact, it certainly accounts for the cessation of the absorption by the roots, within a certain period after the removal of the upper part of the stem and branches; for when these have been separated, there will no longer be the requisite supply of descending sap, to serve to uphold the density of the contents of the vessels of the roots; the fluids which they contain will be gradually diluted until they are little else than pure water, and the endosmose will cease. All the phenomena of absorption in animals appear to us distinctly referrible either to physical principles or to the laws of cell-growth. The former will sufficiently account for the entrance of liquids and of soluble substances into the tubes of the absorbent and sanguiferous system; whilst the latter seems concerned, as Mr. Goodsir has shown, in the special selective agency of the absorbent vessels.

Professor Matteucci's fifth lecture gives a succinct and accurate account of the present state of our knowledge of the function of digestion; which no well-informed physiologist has any longer the least hesitation in regarding as a process whose explanation upon physical principles is nearly complete, and whose entire elucidation must be looked for in a further application of the same principles. As we have at various times brought this subject under the consideration of our readers, we shall confine ourselves at present to the notice of a few points of novel interest, which recent observations have contributed to settle. The solution of the albuminous or proteine compounds in the stomach, by the conjoint action of a free acid, and of an animal compound to which the name of pepsine has been given, has long been an established fact; although doubts still remain as to the nature of the acid, and the precise nature of the organic compound needs further elucidation. A most important fact has been ascertained, however, by MM. Bernard and Barreswill, namely, that the organic matter of the saliva and of the pancreatic juice (which secretions are normally alkaline) has the same solvent power for albuminous matters, when acidulated, as that which is possessed by the gastric secretion. The solution of albuminous matters being complete, these substances will necessarily be introduced by simple physical absorption into the blood-vessels so copiously distributed upon the walls of the stomach and intestinal canal; and the experiments of MM. Tiedemann and Gmelin, which have been recently confirmed by MM. Bouchardat and Sandras, leave it doubtful whether the albuminous matters found in the lacteals are ever taken up directly from the contents of the intestinal canal, or whether they are not derived indirectly from the surrounding blood-vessels. The relative position of the blood-vessels and lacteals in the villi,—the latter being completely surrounded by the former,—would seem to add support

to this view; and it further seems to correspond well with what we know of the general functions of the absorbent system, which may be looked upon as a great glandular apparatus, its elements being dispersed over the entire body, destined to be continually removing albuminous matter from the blood, and to be restoring it again in the form of fibrine; the absorbents of the intestines having the additional function of introducing fatty matter, in the peculiar form presently to be adverted to.—The experiments of MM. Bernard and Barreswill further show, that the peculiar organic matter of the saliva and pancreatic fluid, when accompanied with an alkali (as in the normal condition of those secretions), has the power of transforming starch into sugar. Thus it would appear that the digestion of amylaceous substances commences in the mouth, during the processes of mastication and insalivation; that it is suspended during their continuance in the stomach, the acid condition of the gastric fluid being incompatible with it; and that it is renewed when the pancreatic secretion is poured into the intestinal canal, being (it would appear) the special object which that secretion is destined to perform. Various recent experiments have thrown light upon the way in which the saccharine matter thus formed is afterwards disposed of. From the researches of Drs. Buchanan and Thomson it would appear that a certain amount of it may be detected in blood drawn shortly after a full meal; but that it soon disappears, being probably burned off by the respiratory process almost as fast as it is introduced. The inquiries of MM. Bouchardat and Sandras make it probable, however, that the greater part of the sugar and dextrine produced by the metamorphosis of starch is converted into lactic acid during its passage along the alimentary canal; and that it is absorbed, and applied to the maintenance of the respiratory process, in this form. If pure sugar be introduced in any large quantity into the blood-vessels of a healthy animal, it is speedily eliminated by the urine, and is thus altogether inert as a supporter of the combustive process; and it is probably to avoid its too rapid absorption, that the production of saccharine from amylaceous matter takes place so slowly in the alimentary canal. The long-disputed question as to the origin of fat, which seemed to have been finally settled in favour of the view upheld by Liebig,—their fatty matter may be produced from amylaceous or saccharine compounds,—now appears likely to be completely resolved by the discovery of the mode in which this transformation is effected; namely, by the agency of the bile. From the recent experiments of H. Meckel it appears, that if bile be mingled with grape-sugar, and be kept for some time at a warm temperature, a much larger quantity of fatty matter is to be found in the mixture, than could have existed in the bile. Thus in one experiment, the amount of fat in an equal quantity of the same bile having been found to be from ·48 to ·54 grammes, the amount contained in the mixture of bile and grape-sugar after five hours was ·87 grammes, and after twenty-four hours was 1·84 grammes.

On the mode in which fatty matters are introduced into the system, Professor Matteucci affords us some valuable information from his own researches. Physiologists are well aware that this introduction takes place chiefly, if not entirely, through the medium of the lacteals; and the experiments of Schwann have rendered it next to certain, that one part of the functions of the bile is to reduce the fatty matters of the ingesta to a

condition fit for absorption. When an animal has been fed on substances in which oily matter exists, the chyle is always found to contain fatty particles, some of which are solitary and of appreciable size, varying in diameter from 1-25,000th to 1-2000th of an inch; whilst others, of dimensions more minute than even the smallest of the preceding, are crowded together in the fluid of the rich milky chyle, and give to it its opalescent character. That these last particles, which form what has been termed by Mr. Gulliver the "molecular base" of the chyle, are chiefly of a fatty nature, is shown by their ready solubility in ether; the addition of which to the chyle causes the whole molecular base instantly to disappear. There is no evidence that any fatty matter exists in the chyle in a state of perfect solution; and as we know that oily substances in general are not capable of endosmotic action, especially when the membrane is moistened with an aqueous fluid, there is a difficulty in understanding how the fatty particles can find their way from the intestinal canal into the lacteal vessels. This difficulty has been in great part resolved by the experiments of Professor Matteucci. He has shown that if water be rendered very slightly alkaline, so as scarcely to act upon test paper, and some oil be then shaken up with it at a temperature of from 95° to 104°, an emulsion is at once formed, which has very much the aspect of milk. The liquid when left to itself, shows a further analogy to milk; for it separates into two layers, of which the upper one is the more opaque, and contains oily particles of appreciable size, whilst the inferior one, though less opaque, still preserves the milky aspect, and has innumerable minute particles of an oily character diffused through it. Thus we see that a very small quantity of alkali, such as is contained in the biliary and pancreatic secretions, though it will not dissolve the oily matter, reduces it to precisely that state of fine division in which we find it in the chyle. The following experiments show that in this condition it may be made to pass through an animal membrane.

"Having filled a portion of intestine with this emulsion, I plunged it into the alkaline solution just described, keeping the temperature at from 95° to 104°. After a certain lapse of time, the latter became turbid, and assumed the characters of the emulsion within; so that it may be fairly concluded that some of the latter had escaped through the membrane, and diffused itself through the liquid around.—The following experiment appears still more conclusive. I filled an endosmometer with a very weak alkaline solution, and immersed it in the emulsion which I have shown you. The membrane employed was, as usual, that of the urinary bladder of the ox; and the two liquids were at the temperature of 86° at the commencement of the experiment. Endosmose took place, and the emulsion penetrated the alkaline solution, so as to raise the column of liquid 30 millimeters in a very short time." (p. 105.)

Thus we see how the slightly alkaline condition of the liquor sanguinis and of the chyle will operate physically in promoting the entrance of the fatty particles; since for their passage through an animal membrane nothing more is required, than that they should be reduced to a state of very fine division (which is accomplished by the admixture of the biliary and pancreatic secretions), and that on the other side of the membrane a slightly alkaline fluid should exist. And it is to be remembered here, as elsewhere, that the constant motion of the blood compensates for the weakness of its chemical actions; a much more powerful effect being

produced by a very weak solution in rapid movement through the vessels, and therefore continually renewed, than by a much stronger one that remains always at rest.

We may pass over the sixth and seventh lectures, which treat of Respiration, Nutrition, and Animal Heat, as not containing any original observations of importance. The physics of these departments of physiology are better understood, and more fully treated in physiological writings, than those of most others. We may stop, however, to notice the valuable discovery made not long since by Valentin and Brunner, that the quantities of oxygen absorbed, and of carbonic acid exhaled in the respiratory process bear a constant relation to each other; and that this relation corresponds so closely with their mutual diffusion-volumes, as determined by the law discovered some years since by Professor Graham, that no reasonable doubt can remain, that the interchange takes place in strict accordance with physical principles. There will always be an excess of oxygen above the carbonic exhaled; the proportion being 1174 parts of the former to 1000 of the latter; and as carbonic acid contains its own bulk of oxygen, there will always be a positive surplus of the latter, amounting to 174 parts in every 1174 absorbed, to be employed in the system for other purposes than the generation of carbonic acid. These other purposes seem to be principally the union with hydrogen, forming part of the water which is exhaled; and the combination with the sulphur and phosphorus introduced with the proteine-compounds, which are excreted in the condition of sulphuric and phosphoric acids. These additional combustive processes, with others, perhaps, still to be discovered, seem to leave no reasonable difficulty in accounting on purely chemical principles for the entire amount of caloric generated in the living animal body.

In the eighth lecture, on the Phosphorescence of organized beings, Professor Matteucci gives the results of several original experiments upon the luminosity of the glow-worm, and the conditions under which this interesting phenomenon occurs. He fully confirms all that had been previously advanced by Macaire, in regard to the effect of oxygen in brightening the light, and of carbonic acid in extinguishing it; and he has shown that the same result happens when the luminous segments separated from the bodies of the animals are exposed to these gases respectively. An increase of energy in the light is produced also in the dead as in the living state, by a high temperature. When several worms are placed in a limited amount of air for some time, the light gradually becomes more dim and is at last extinguished; and on examining the air it is found that the oxygen has nearly disappeared, and is replaced by carbonic acid. If the separated luminous segments alone be placed in a limited quantity of air over mercury, a similar change is produced, though with less rapidity; an appreciable quantity of carbonic acid being generated so long as the luminosity continues. There can be no doubt, therefore, that the phosphorescence is due to a combustive process, taking place in the peculiar substance which is found beneath the integument of the luminous segments. Of this substance, enough has been obtained by Professor Matteucci in a separate state, to be made the subject of chemical examination. The following he states to be its physical and chemical characters. It has a granular aspect and a yellowish colour; and is traversed by great numbers

of tracheæ. It possesses a peculiar odour, somewhat resembling that of the perspiration of the feet; it is neither acid nor alkaline; it desiccates quickly in air; it undergoes a sort of coagulation when treated with dilute acids; it is not soluble in alcohol, ether, or weak alkaline solutions; it is dissolved and changed by concentrated sulphuric and hydrochloric acids, with the aid of heat; but the solution made in this manner does not become blue, which excludes the idea of the presence of albumen. When it is heated in a tube, the ordinary ammoniacal products are set free. Professor Matteucci has several times carefully tested it for the presence of phosphorus, by calcining it with nitre, and treating the dissolved residue with reagents which would indicate the presence of phosphates; but always without success. It hence appears that the luminous matter is a substance *sui generis*, but made up of the usual components of the animal tissues, namely oxygen, hydrogen, carbon, and nitrogen; and there is the less difficulty, therefore, in admitting and accounting for the instances of luminosity in the living human subject, to which Sir Henry Marsh a few years ago directed our attention. (See Prov. Med. Journ. 1842). In these and similar cases, the luminosity was probably due to the production of the luminous matter from the food or from the disintegrating tissues, by a process of chemical transformation which is normal in the luminous insect, though abnormal in man. There is no want of instances of the occurrence of slow combustion attended with luminosity, in organized matters of various kinds, such as decaying wood, greasy cotton, and powdered charcoal. The variations in the light which occur in the living insect, the greater brilliancy which it exhibits when irritated, and the occasional entire extinction of it for a time, appear to be fully accounted for by the control which the animal possesses over the amount of atmospheric air that passes through the tracheæ of the luminous matter. When the spiracles are shut, and no air enters, the light is extinguished; on the other hand, when the respiration is accelerated, more oxygen than usual traverses the air-tubes of the luminous matter, and its brightness is increased.

We now come to those departments of physico-physiological inquiry, in which Professor Matteucci has earned his highest reputation; those namely, which relate to the Electrical phenomena of Living Beings. And as we have not on any former occasion brought these formally under the consideration of our readers, we shall avail ourselves of the present opportunity of doing so. The ninth lecture treats of the muscular current of electricity, our knowledge of which (except in the special case of the frog, to be presently noticed) is entirely due to the inquiries of Professor Matteucci. In order to understand the nature of the experiments to which we shall presently refer, our readers must be first made acquainted with the peculiar indicator of the existence of electric currents, which Professor Matteucci has found most useful. This is nothing else than the leg of a frog recently killed, separated from the body, with the crural nerve dissected out from its junction with the lumbar plexus, and detached from the muscular mass of the thigh, which is altogether removed; the leg, with this nervous cord hanging freely from it, is inclosed in a tube of glass covered with a non-conducting varnish; and the apparatus now forms a galvanoscope peculiarly fitted to determine the existence of weak electric

currents. For if, holding the tube in the hand, we cause the nerve hanging freely from its extremity to touch any body whose electric state we wish to determine, at two points, the existence of even a very feeble electric current between those points will be at once indicated by the contraction of the muscles of the leg. Now if we make an incision into any muscle of a living animal, and insert into the wound the nerve of this galvanoscopic frog, in such a manner that it shall touch both the deep and superficial portions of the wound, we immediately find that muscular contractions are excited in the frog's leg; showing the existence of a current between the different parts of the muscle. This phenomena will manifest itself in the muscles of any animal whatever; and not only in those of the living animal, but in those of animals recently killed, or in muscles lately separated from the body; the current ceasing, however, more speedily in the muscles of warm-blooded animals, than in those of reptiles and fishes.

In order to discover the direction and the intensity of this current, and the conditions under which it presents itself, it is necessary to make use of the ordinary galvanometer; but the employment of this instrument requires many precautions, to ensure freedom from error; which are detailed by Professor Matteucci in his work on the Electro-physiological Phenomena of Animals. By means of this instrument, carefully employed, it may be proved that the current *constantly* proceeds from the internal part towards the external surface of the muscle; and having obtained this knowledge, Professor Matteucci next proceeds to construct a *pile* of muscles, by which the current may be rendered more intense, and its conditions better determined. This pile may be composed of the thighs of frogs cut across, and so connected one with the other, that the external surface of each shall be in contact with the internal surface of the next; or it may be made up of isolated muscles of any other animal, the same precaution being employed. In this manner the intensity of the current is increased, in precise accordance with the augmentation in the number of single portions employed; and a considerable deflection of the galvanometer is the result. No such deviation takes place when the pile is composed of other animal tissues and organs, such as membranes, nerves, the brain, liver, lungs, &c.; the signs of electric disturbance obtained from such being of the feeblest character. The muscular current varies in intensity according to the activity of the nutrition of the muscle. It is at first more powerful in warm- than in cold-blooded animals; though it ceases much earlier. In cold-blooded animals it is very much weakened by previous exposure of their bodies to a low temperature; but this has not the same effect upon warm-blooded animals, on account of their independent calorification. It is remarkable that whilst the muscular current is but little affected when the animals are poisoned by narcotics, by carbonic or hydrocyanic acids, or by arseniuretted hydrogen, it should be almost completely destroyed by sulphuretted hydrogen; both warm- and cold-blooded animals, asphyxiated by this gas, losing nearly all power of manifesting it. In some of Matteucci's experiments with the muscular pile, the successive portions of the frog's thighs were not brought into direct contact, but the communication was established by means of the trunk of the crural nerve dissected out; this did not make any difference in the results, except that the current was weakened by the imperfect

conducting power of the nerve. That the nerve in this case acted simply as a conductor was shown by the fact, that the result was the same, in whichever direction the current was made to pass through it; and that a thread of cotton moistened with distilled water answers the purpose equally well. Other experiments demonstrate that this muscular current is altogether independent of the nervous system, and that it must be produced by the molecular changes which are continually occurring in the muscle itself. The remarkable influence of sulphuretted hydrogen seems readily explicable on this view; since the powerful effect of this gas in checking endosmotic action would bring those molecular changes to an immediate stand.

In the tenth lecture we find an exposition of the interesting phenomena of the Electric Fishes, and of the *courant propre* or (supposed) peculiar current of the frog. We shall not follow our author through his account of the former subject, luminous and concise as it is; but must content ourselves with adverting to those points which have been established by his own researches, and which have the most immediate bearing upon the theory of this remarkable class of phenomena. It is easily shown that all the phenomena of the discharge of the Torpedo (upon which, from the comparative readiness with which it may be obtained, many more researches have been made than upon the Gymnotus) are to be attributed to an electric current generated by the instrumentality of the electric organs. Each surface of these organs possesses a different electrical state; the dorsal surface being positive, whilst the abdominal is negative. Now if the electric organs of a lively torpedo be completely separated from the body of the animal, leaving the nervous trunks undivided, and the surfaces of the organs be covered with prepared frogs, and the conducting wires of a galvanometer be also applied to them, it will be observed, on irritating the nerves, that the frogs and the galvanometer indicate the existence of a current in the electric organ; the manifestations of the current being seen over the whole of the organ if all its nerves are irritated, but being confined to that part of it to which the individual nerve is distributed, if only one of the nerves be affected. There is no difficulty in proving, that the exciting cause of the discharge is an influence conveyed through the nerves from the encephalon. These nerves have their centre in a pair of lobes or ganglia quite distinct from the ganglia of which the fish's encephalon is ordinarily composed; and whilst the irritation or the removal of the latter produce no effect upon the electric powers, the slightest irritation of the electric lobe on either side produces violent discharges in the corresponding electrical organ, whilst the destruction or removal of that lobe totally and permanently destroys the electric power. But it does not hence follow, as some have supposed, that the electricity is generated in the brain, and that the electric organ is simply the instrument of its manifestation. Such an idea is manifestly contrary to all *a priori* probability; for whilst we have, on the one hand, a large and complex apparatus, evidently ministering to the peculiar powers of the electric fishes, being superadded in them and in them alone to the ordinary elements of their system, we find no such enlargement of the nervous centres as would at all warrant the idea that the latter are the real generators of the electricity. The fact appears to be, that the relation of the

nervous system to the electric apparatus is essentially the same as that which it bears to the muscular; namely, that it is the means by which the independent powers of the latter are called into activity. We shall not now recapitulate the proofs, to which we have referred on various occasions, of the existence of the property of contractility as an endowment essentially belonging to muscular fibre, to which innervation is the most direct and powerful stimulus; but we shall content ourselves with remarking that Professor Matteucci's experiments furnish evidence of the same kind, with regard to the generation of electricity in the electric organs of the torpedo &c., and the agency of the nervous system in calling forth its manifestations, rather than in directly occasioning its production.

The structure of the electric organ is very simple and uniform. It is entirely composed of a series of hexagonal prisms of membrane, strongly resembling a honeycomb in their arrangement. Each of these prisms is divided transversely by a number of membranous partitions; so that the whole prism is in fact composed of a pile of flat hexagonal chambers. These chambers are filled with a fluid, which consists of water holding in solution about one tenth part of albumen, and a little common salt; and upon their walls are distributed the nervous filaments proceeding from the electrical lobe. These filaments are believed by Professor Savi (the results of whose anatomical researches are adopted by Matteucci) to terminate in loops both at their central and their peripheral extremities. Now if a portion of any one of these prisms be removed from a living animal, and the nerve of a galvanoscopic frog be brought into contact with it, any mechanical irritation of the fragment, even though it be no larger than a pin's head, will suffice to produce contractions in the frog's leg. When it is remembered that the number of these prisms, in a single organ of the torpedo, was ascertained by John Hunter to be four hundred and seventy, we need not be surprised at the large amount of electricity generated by a mass of which so small a portion can exert a sensible effect. That the electricity is not conveyed by the nerves to the electric organ further appears from the fact, that the discharge is prevented from taking place by *tying* their trunks, as completely as it is by *dividing* them; the former of which operations makes no difference in their power of conducting electricity. In this as in other respects, there is a close analogy between the agency of the nerves on the electric organ, and their influence on muscles. It is important, in any theory we may form of the mode in which the electric disturbance is produced, to bear in mind that the two ends of the prisms are the points whose electric conditions are most opposed to each other. These extremities correspond with the two surfaces of the body in the torpedo; but in the gymnotus, in which the electric organ extends through a large part of the length of the animal, the direction of the prisms is the same; and consequently the parts between which the discharge should be the most powerful are the head and tail, which is exactly the case, the fish always endeavouring to apply these parts to any body through which it is about to discharge itself. Professor Matteucci suggests that the influence of innervation may be exerted simply in separating the two electricities ordinarily balanced in each prism, so that the positive shall be driven to one extremity, and the negative to the other; and he adduces as parallel cases, the agency of heat on the tourmaline and

on some crystalline metals, the effects of chemical action, friction, compression, &c. He explains, on this hypothesis, the independent action of a fragment of one of the prisms, by supposing that the irritation acts primarily upon the nervous filaments it may contain. Altogether, the immediate cause of the electric disturbance remains still obscure; but it is something to know where to look for it, and to find it removed from that convenient repository of causes of all unexplained phenomena,—the nervous system.

The effects of the (supposed) peculiar electric current of the frog were first observed by Galvani; who found that a frog prepared in the ordinary manner would execute muscular contractions, when the lumbar nerves were brought into contact with the muscles of the thigh or of the leg. The existence of such a current, however, circulating from the legs to the body of the animal, was first shown with the aid of the galvanometer by Nobili, who made numerous experiments upon it. Its characters at first appeared sufficiently distinctive to induce Professor Matteucci still to rank it in a separate category, after the discovery of the general muscular current which is common to all animals; but he has been recently led, by several considerations, to regard it as a peculiar form of the latter. He found that its intensity was affected by precisely the same conditions as those which affect the muscular current; and that it might be explained by a due attention to the relative arrangement of the muscular and tendinous structures. When a thick muscle terminates in a slender tendon, this last, being in connexion with the interior or deep-seated parts of the muscle, corresponds with these in its electric condition; and it is consequently in an electric condition opposed to that of the surface of the muscle. This may be shown by examination of muscles of similar character in other animals; such as the pectorales of birds. Now the relative arrangement of the muscles, tendons, aponeuroses, &c. in the frog especially favours the peculiar manifestation of the muscular current which has been just described; and this animal is no longer to be regarded in the exceptional light in which it was long viewed by electricians.

In the eleventh lecture, on the Physiological Action of Gravity, of Light, and of Heat, we find nothing deserving of notice, save the extreme meagerness of the treatment of these important subjects. A few well-known facts are brought together; but nothing like a general view of the operations of these agents on the living body, whether in plants or animals, is attempted. In regard to heat, whose agency is so essential a condition of all vital action, the deficiency is peculiarly striking, when contrasted with the amplification of all that relates to electrical phenomena. As the professor has taken other opportunities of bringing before the public his original researches on the latter subject, we do not think that he has done wisely in making them form so prominent a portion of the present course, to the exclusion of other topics of at least equal importance. The twelfth and thirteenth lectures are devoted to an exposition of the Physiological Action of the Electric Current; and form a complete resumé of all that is certainly known on this subject. Of this we shall present our readers with a concise view; to serve as a guide whereby their therapeutic applications of this agent may be directed.

In the first place Professor Matteucci lays it down, that no effect can be

produced, save by the electric *current;* and that all the supposed results obtained by *static* electricity,—as when a plant or animal is insulated and charged with electricity by the electric machine,—are fallacious. Although this statement may be perfectly true as regards the inertness of static electricity, yet it does not follow that a plant or animal thus electrized should be uninfluenced by this agent; since we apprehend that the constant radiation of electricity from the numerous points of its surface must give rise to a condition that shall correspond with the passage of a current through it. Putting aside this question, however, we proceed to consider the results of the *dynamic* form of electricity, or the electric current. If we place the two conducting wires of a galvanic pile in contact with two points of a nervous trunk of mixed endowments, in a living animal, that part of the trunk being carefully insulated, we find, on closing the circuit, that powerful muscular contractions are induced, whilst at the same time the animal gives signs of pain. Whilst the circuit is closed, however, these phenomena no longer manifest themselves; but they are renewed when it is interrupted, ceasing again in a few moments. They are again renewed when the circuit is again closed; and this whether the current be transmitted in the *direct* course, i. e. from the roots of the nerve towards its periphery, or *inversely* from the periphery towards the centre. It is observed, however, that the movements are most energetic when the direct current is first transmitted; whilst the signs of pain are the greatest when the inverse current is employed. If the current, instead of being transmitted along the nerve, is passed *across* it, scarcely any effect is produced. After experimenting for some time upon the same animal, we find that all these phenomena cease to present themselves, the animal no longer giving any sign of the passage of the current; the period required to exhaust its influence will be shorter as the current is more powerful. If the animal be then allowed to remain for some time at rest, or the current be rendered more powerful, the original phenomena are reproduced. Now if we study the phenomena which present themselves when the manifestations of the current are becoming weakened, but are not yet entirely ceased, it will be observed that when the direct current is interrupted, the contractions of the inferior muscles (that is, of the muscles supplied by the part of the nerve below that at which the current is applied) are feeble; whilst those of the muscles of the back, as well as the agitation and cries of the animal, remain nearly as before. On the other hand, when the current is renewed, it only produces contraction of the inferior muscles. Precisely the opposite is the case in regard to the inverse current; for the contractions of the muscles of the back, the movements of the ears, and the cries, only occur at the moment when the circuit is closed, scarcely any contraction being seen in the inferior muscles; whilst the latter are alone thrown into contraction when the circuit is closed.

"We may then divide into two different periods the action of the electric current which excites the nerves of a living animal. In the first, the irritation of the nerve is transmitted in all directions, towards its central part as well as towards its periphery, both at the commencement and the conclusion of its passage; and this independently of the direction of the current. But in the second period, the excitation of the nerve is transmitted in the peripheral direction

alone, at the commencement of the passage of the direct current, and at the instant when the inverse is interrupted; whilst, on the contrary, the irritation of the nerve is transmitted towards its centre, when the direct current is interrupted, or the inverse one is closed. These results may be expressed more simply by the general statement, that the current acts in the direction of its own course when the circuit is closed, and in the contrary direction when it is interrupted." (p. 232.)

It is easily shown that the muscular contractions thus excited by the electric current are of two kinds: those of the muscles supplied by the nerve below the point of excitation being *direct*, whilst those of all other muscles are *reflex*, being excited through the medium of the spinal cord, to which the irritation is conveyed.

When similar experiments are made upon an animal recently killed, it is found that a very feeble galvanic combination acts only according to the above law; whilst, if a more powerful one be employed, the contractions at first manifest themselves in all the muscles both at the closure and at the interruption of the circuit, in whichever direction the current is transmitted. After a time, however, the contractions of the inferior muscles no longer take place, save at the commencement of the action of the direct current and at the cessation of the inverse. This is extremely well shown by preparing a frog after the manner of Galvani; the two legs remaining connected with the spinal cord only by the crural nerves, and the bones of the pelvis with the lumbar vertebræ being removed. If the two legs be plunged into separate glasses of water, and the two conducting wires of the galvanic pile be plunged into these respectively, a current will pass from one glass to the other through the prepared frog, being transmitted *up* one leg, along its nerve into the spinal cord, and then *down* the other leg. Thus the same current will be *inverse* in one leg, and *direct* in the other. At first both legs contract together, when the circuit is either opened or closed; but after a time that leg only will contract on closing the circuit, in which the current is direct; whilst that leg only will contract on the interruption of the circuit, in which it is inverse. Thus the frog prepared in this manner is not merely a delicate galvanoscope, indicating the existence of a very small amount of electric disturbance; but it also serves as a galvanometer, showing the direction of the current which circulates through its nerves.

It is impossible to give a perfect demonstration of the action of the electric current upon the muscular substance; since the latter cannot be completely isolated from the nerves. But if a current of electricity be passed through a muscle, immediate contraction takes place on the closure of the circuit, whatever be the direction of the current. This contraction, however, only lasts for an instant, and does not return so long as the circuit remains closed; it usually occurs again, though less forcibly, at the interruption of the circuit; but this is not always the case, especially if the current have continued for some time, the interruption of the circuit producing no change in the state of the muscle. In no case does the direction of the current make any difference in its action on muscle; those contractions being the most powerful, and manifesting themselves for the longest period, which take place when the circuit is closed; whilst those are the weakest, and the first to disappear, which occur when it is

broken. This marked departure from the laws of electrical action upon the nervous system, makes it probable that the electricity does not thus operate upon the muscle through the nerves which may remain in it, but directly upon the muscular substance itself.

It is easy to show that the diminution of the excitability of the nerve, by the continued transmission of an electric current through it, does not affect so much the entire nerve, as the part of the trunk interposed between the two conducting wires of the galvanic apparatus. For if the current be thus transmitted through a certain part of a nerve, until muscular contractions can no longer be excited by its means, the transmission of the current through a portion of the trunk at a greater distance from the brain will again call the muscles into contraction; and after this portion of the nerve has lost its excitability, the same effect may be again produced by transmitting the current through a portion of the trunk still nearer the periphery. In the same manner, when the signs of pain have ceased to manifest themselves, they may be again excited by causing the current to traverse portions of the nerve nearer and yet nearer to its central extremity, in proportion as the excitability of the trunk is weakened.

Professor Matteucci did not find any difference in the degree or duration of the excitability of the nerves after death, whether the animals had been killed by hydrogen, azote, carbonic acid, or chlorine; but he found that in animals killed by hydrocyanic acid, or by the reiterated discharges of a large battery through the spinal cord, scarcely any contraction could be excited through the nerves, although the muscles still contracted vigorously when the electric current was directly applied to them. When the animals had been poisoned by sulphuretted hydrogen, no contractions could be obtained without the employment of strong galvanic combinations; and even then they were very feeble and evanescent.

From the fact that the portion of the nerve, through which the electric current passes, loses its excitability whilst the remainder retains it, we may reasonably infer that the influence which passes through the nerve to the muscle is *not* the electric current, but something excited by it; for if it were the former, the excitability of the whole nerve would be destroyed at once. Such an inference is fully confirmed by the effects of ligatures upon the nervous trunk. If the current of electricity be transmitted through the part of the trunk round which the ligament is placed, one of the conducting wires being applied above, and the other below, the ordinary effects are manifested, in a degree somewhat weaker than usual. But if the current be wholly transmitted through the part of the trunk which is above the ligature, no *direct* muscular contractions are called forth, only the *reflex* motions being executed; whilst, on the other hand, if the current be passed through the part of the trunk below the ligature, no reflex movements are excited, but the muscular movements directly produced by the motor agency of that nerve are vigorously executed. From this circumstance it is evident, that the current of electricity transmitted through part of a nerve, is not the immediate cause of either the direct or the reflex movements; for, if it were, these movements would not be prevented as they are by the application of the ligature, which has been proved not to check an electric current. It seems evident that some particular condition of the nerve itself is the immediate stimulus to the

muscular contraction, or to the reflex action of the spinal cord; and that this condition may be called forth most effectually for a time by the transmission of an electric current through a portion of the trunk. We shall hereafter find that other evidence points to the same conclusion,—the non-identity of the nervous and electric agencies.

Professor Matteucci next points out a very remarkable difference in the influence of the electric current upon the excitability of nerves, according as the course of the current be direct or inverse. We have seen that the direct current, if transmitted continuously, completely exhausts after a time the excitability of the nerve through which it passes, a period of twenty or thirty minutes being usually sufficient to effect this; but if an inverse current be made to traverse a portion of a nerve without intermission, even for three or four hours, there generally occurs, at the interruption of the circuit, a violent muscular contraction, which lasts for several seconds, and might almost be called tetanic. It ceases almost immediately upon the closure of the circuit; a new contraction at first taking place, after which the member returns to its natural condition. From this phenomenon it would appear as if the inverse current actually increases the excitability of the nerve; and such would seem to be the necessary inference from the results of other experiments of Professor Matteucci's. For he has found that if the inverse current be continued only for a fraction of a second, the contraction which takes place on breaking the circuit is decidedly less forcible than that which occurs after the current has been transmitted for one, ten, or twenty seconds, or for any more lengthened period. These effects, however, can only be demonstrated on a living animal; we cannot expect the excitability of a nerve to be increased after death. Whatever influence has been exerted upon a nerve by an electric current, a state of repose tends to restore it to its natural condition of excitability; increasing it, if it have been diminished by the direct current; or carrying off the surplus, if it have been increased by the inverse. The greater the change in the degree of excitability, the longer is the period of repose required to restore the nerve to its usual state. But when the excitability of a nerve has been suspended by the direct current, it is speedily renewed by the transmission of the inverse.

With regard to the influence of the electric current on different parts of the nervous system, we do not find anything new of importance; but it may be hoped that Professor Matteucci, having now completed his investigations in several other departments of electro-physiological inquiry will devote himself to this, with all the aid he can obtain from a previous acquaintance with the anatomical and physiological relations of the subjects of his experiments.

The difference between the effects of the continued current, and that of a frequently-interrupted current, are very remarkable; the excitability of the nerves being exhausted by the latter, much more speedily than by the former. This was first pointed out by Marianini; and has been still further substantiated by Masson, who contrived, by means of a spring pressing against a toothed wheel in revolution, to complete and interrupt the circuit many times in a second. The same result may now be obtained much more readily, however, by the electro-magnetic machine, the peculiarity of whose action consists in this continual closure and interruption

of the circuit. If one of the conductors of a galvanic pile consisting of no more than ten pairs of plates, be introduced into the mouth of a rabbit, and the other communicate with the muscles of the back, and an interrupted current be then transmitted by the wheel of Masson, the animal dies within a few seconds, apparently by the exhaustion of the nervous excitability of the spinal cord. Some curious results have lately been obtained by the application of the interrupted current to the trunks of nerves, by Professor Volkmann;* of these we have already given an account elsewhere (see Vol. XXII, p. 279); but we may here notice one or two of the most interesting of his general results, which we should be glad to see confirmed by other experimenters. He distinguishes three forms of muscular contraction as produced by the rotating magnet. The first, which he terms the *continued*, is a state of tonic contraction which lasts so long as the stimulus is applied, but ceases with its withdrawal. The second continues not merely during the application of the stimulus, but even after its cessation; so that it may be termed the *persistent* contraction. In the third form there is an *alternation* of contraction and relaxation; though the stimulus remains constantly applied as before. According to Professor Volkmann, there is a relation between these forms of muscular contraction, and the mode in which the stimulus is applied. The continued contraction is produced by the passage of the current through the motor nerve itself, in the direction of the muscles; and relaxation supervenes as soon as the current ceases. The persistent contraction results from the application of the electric current to the nervous centre from which the trunk originates; and the continuance of the contraction after the cessation of the current would seem due to peculiar changes thus excited in the central organ. On the other hand, the alternation of contractions and relaxations presents itself, when the stimulus is applied to a nerve which produces muscular movement not by direct but by reflex action. If these positions be correct, we shall be supplied with a most valuable means of determining the true centres of action of many nerves, especially those belonging to the sympathetic system; but at present the generalizations of Professor Volkmann require confirmation; and we should much like to have this confirmation afforded by an experimenter on whom so much reliance may be placed as upon Professor Matteucci.

In concluding this part of his subject, Professor Matteucci makes a few observations upon the therapeutic employment of electricity. We could wish that they had been more extended; but they will suffice to show the principles which should, in his opinion, guide the selection of the particular method of applying this agent. It can scarcely be doubted that many cases of paralysis depend upon an exhaustion of the excitability of the nerves, analogous to that which is produced by the continued action of the direct current. In such cases, the action of an inverse current would seem the appropriate remedy. In favour of the employment of the current as a means of cure, he states that its passage often produces contractions in paralysed limbs; and he mentions that if the two sciatic nerves of a living frog be cut, and one of the limbs be left at rest for ten, fifteen, or twenty days, whilst the other is submitted two or three times a day to the action of the electric current, the latter will continue to exhibit con-

* Müller's Archiv, Heft v, 1845.

tractions when the former has altogether lost the power of doing so. Our readers will recollect that Dr. J. Reid tried a similar experiment, and with similar results, some years since; his object being to ascertain if the occasional exercise of a muscle paralysed by division of its nerves would maintain its nutrition and its contractility; which it was found to effect. In applying electricity therapeutically, Professor Matteucci strongly urges the importance of commencing with a very feeble current; having seen a paralytic person seized with violent convulsions of a tetanic character, on the action of a current furnished by only a single pair of plates. The action of the current should be continued for a short time only, particularly if it be energetic. If the interrupted current be employed instead of the continuous, its passage should be checked after every twenty or thirty discharges, and an interval of repose should be allowed to the patients. The application of the interrupted current would seem, in practice as in theory, to be attended with results more decidedly beneficial than those of the continued current. It is the former which is produced by the magneto-electric machine, which is, on many accounts, the most convenient means of applying electricity therapeutically.* Although, as we have seen, Professor Matteucci is sufficiently sceptical regarding many others of the alleged effects of electricity, yet he considers that the accounts of cases of paralysis, cured by the electric treatment, are already sufficiently numerous and authentic, to warrant the employment of it in cases of paralysis, with that perseverance which is necessary to warrant the hope of beneficial results.

One of the maladies in which he has proposed to employ it, is of a character that might have been supposed most completely to contra-indicate its use; namely, Tetanus. Yet theory and practice both seem to countenance its employment. The theoretical ground is simply this;—that whilst the interrupted current produces a succession of convulsive actions, the direct continued current, when sufficiently prolonged, occasions a diminution or suspension of the excitability of the nerve, and might consequently be expected to suspend tetanic convulsions, when passed through the part of the nervous system by whose undue excitability they are produced. This idea was brought to the test by Professor Matteucci upon frogs thrown into tetanic convulsions by narcotics or by hydrocyanic acid; and he states that the tetanus soon ceased under the influence of the prolonged passage of the direct current. Nevertheless the frogs died; but this was from the other effects of the narcotics employed. We should recommend the repetition of the experiment with strychnine, which will produce tetanus uncomplicated with any other change. The results obtained by the application of the electric current in a case of tetanus in the human subject, as published by Professor Matteucci in the 'Bibliothèque Universelle' (May, 1838), appear sufficiently encouraging to induce the repetition of the trial; although a favorable termination did not ensue in that instance. During the transmission of the current, the patient no longer suffered violent convulsive attacks, he became able to open and

* We would particularly direct attention to a form of this apparatus lately constructed by Mr. Hearder, a most ingenious *blind* mechanician of Plymouth. Its great advantage consists in the facility with which the nicest gradations of power may be attained; the simple movement of an index along a scale enabling the operator to increase the force of the current, by as many as thirty distinct steps, from a stream which is scarcely perceptible, to a succession of rapid shocks, of strength sufficient for any therapeutic purpose.

close his mouth, and the circulation and transpiration seemed re-established. Unfortunately, this amendment was not of long duration; for the disorder was occasioned and kept up by the presence of foreign bodies introduced into the muscles of the arm. The issue might be more favorable, where the cause of the tetanus is different; at any rate, we may advantageously have recourse to the application of electricity, for the purpose of diminishing the sufferings of the patient in this terrible malady.

It has been proposed to employ the electric current for the purpose of dissolving vesical calculi and cataract. But in neither of these cases would there appear to be any probability of success; since, even if the disintegration of the substances could be effected, the products would be perfectly insoluble and could not be removed. A cataract would be created by the coagulation of the albumen of the lens, occasioned by the passage of an electric current through its substance; but it could not be removed, since no electric action will cause the re-solution of the coagulated albumen. It has been recently proposed by Dr. Petrequin, of Lyons, to apply galvanism by means of acupuncture needles, for the cure of certain forms of aneurism; his idea being, that the electric current would occasion the coagulation of the albumen of the blood-serum, and would thus partly fill the aneurismal sac. We are not aware that this plan has been ever put in practice.

In the fourteenth and fifteenth lectures, we have an exposition of those researches of Professor Matteucci, which have been made with the view of determining the Nature and Laws of the Nervous Force. He very properly remarks that, although such a subject may appear misplaced in a treatise on the "Physical Phenomena of Life," yet there is just ground for ranking it in the same category with those other agents,—light, heat, and electricity,—which have so close a relation to each other, although it does not appear identical with any one of them. We cannot but regard the question of the identity or non-identity of the nervous force and electricity, as settled, for the present at least, by the researches of Professor Matteucci. In addition to the positive results to which we have already alluded, he has obtained many, which though of a negative character are yet very important, from experiments devised to test, in every possible way, the presence of an electric current in nerves which were exciting muscles to vigorous contraction. Although these experiments were made upon the largest nervous trunks of living animals of considerable size, and with the most delicate galvanoscopes, and altogether under circumstances most favorable to the detection of an electric current if it had any real existence, yet not the least indication of any such could be obtained. The phenomena of *induced contraction*, which were discovered by Professor Matteucci some years since, equally indicate that nervous force and electricity are two distinct agencies; at the same time that they prove the very close analogy which exists between them. Of these phenomena, the following is a brief account. If we lay the nerve of a galvanoscopic frog, (see p. 396), upon the exposed muscles of one or both thighs of a frog prepared in the ordinary manner, and cause the latter to contract by an irritation of any kind applied to the nerves or to the spinal cord, the muscles of the leg of the galvanoscopic frog are also thrown into contraction. The experiment is rendered still more striking, by arranging three

or four galvanoscopic frogs in such a manner that the nerve of the second shall lie upon the muscle of the first, the nerve of the third upon the muscle of the second, and that of the fourth upon the muscle of the third. Upon irritating the nerve of the first, and throwing its muscles into contraction, the muscles of the second, third, and fourth will also be thrown into contraction, each having its action *induced* by that which precedes, and *inducing* the action in that which follows; just as in a succession of bodies, the first of which is subjected to a disturbance of its electric equilibrium. If the nerve of the galvanoscopic frog be laid, not upon the muscle but upon the nerve of that which precedes it, no contraction is induced. And if the irritation of the nerve does not produce muscular contraction in the first animal (it is not essential that *frogs* should be employed), no action is induced in the succeeding. Now as the induced contraction takes place as well when the nerves of the first animal are excited by any other stimulus, as by electricity, it is obvious that the induction in the second cannot be explained by the passage of electricity from the first, unless that electricity be generated either in the action of the nerve or in that of the muscle. The experiments already cited prove that the action of the nerve cannot be concerned; and by a long series of experiments, in which particular care was necessary to avoid the sources of fallacy occasioned by the muscular current, Professor Matteucci has satisfied himself that no electricity is generated by muscular contraction. Moreover he found that by interposing a thin layer of Venice turpentine between the thighs of the inducing frog and the nerve of the galvanoscopic frog laid upon them, he could prevent the passage of an electric current, without interfering with the action induced from the muscle to the nerve.

After submitting the question to every test that he could devise, he has been led to consider the phenomenon of contraction induced in muscles by the simple contact of their nerve with another muscle in the act of contracting, as a phenomenon altogether *sui generis;* and as indicating that the action of the nervous power on a muscle produces a peculiar condition in the latter, which is capable in its turn of inducing a state of nervous excitation in the trunk of a nerve brought into simple contact with it. Thus we should be led to refer the nervous power to the same category with electricity and other polar forces, by which similar phenomena of induction are produced; and there is this further analogy, that one of these forces may induce another. For the results of the previous experiments would lead us to the conclusion, that when muscular contractions, sensations, and other actions of the nervous system are performed in respondence to the electric stimulus, the electricity does not act directly on the muscle or on the sensorium, but excites or induces the nervous force, which is the immediate cause of the phenomena; just as heat induces an electric disturbance in the tourmaline or in certain metallic combinations, or as light induces magnetism. The nervous force appears to have a further analogy to those with which it has been here classed, in being originated by chemical changes; the combination of the oxygen of the arterial blood with the carbon and hydrogen of the nervous matter constituting a principal part of these changes. And when a comparison is made between the mechanical power of the human nervo-muscular system and that of any machine that man has devised, we find the former

to be so much greater, in proportion to the amount of chemical change which has taken place, as to confirm the belief that the nervous force has a character altogether peculiar to itself, its mechanical operations being executed solely through living muscular tissue, but being exerted through this instrument far more powerfully than those of any other force developed by an equal amount of chemical change.

The remaining five lectures are occupied with the phenomena of muscular contraction and with the mechanism of its application in the animal body; the circulation of the blood; the action of the vocal apparatus; and the physical phenomena of hearing and sight. All these subjects are discussed, with more or less fulness, in our ordinary physiological works; and as Professor Matteucci has evidently derived the greater part of his information from these sources, especially from the laborious and detailed investigations of Müller, it is unnecessary for us to follow him through his exposition of these topics; more especially as our analysis has already reached its due limits. We shall only say that, although these lectures contain no novelty, they embrace a very clear and complete account of the several subjects embraced in them.

On the whole, then, we commend this little work to the careful study of our younger readers, with the conviction that they cannot fail to profit by it in more ways than one. It will give them a well-digested summary of the greater number of the physical actions going on in the living body; and if this summary is less complete on some points than could be desired, it is peculiarly full upon others; and whilst the former topics are for the most part of a kind on which ample information may be obtained elsewhere, the latter are such as will be almost entirely new to them, and are not readily accessible in any other form. But a still greater advantage will be derived from its perusal, if it be studied as an example of the proper method of scientific investigation, and if the success which has been attained by its philosophic author become an inducement to others to follow in the same path of inquiry with a sagacity and perseverance at all approaching his. Nor should such an example be lost upon any who are desirous of elevating the character of physiological and medical science, and whose inclinations and opportunities lead them rather to the investigation of the purely vital phenomena of living beings, than to those of a mixed or of a purely physical character. It is only by keeping constantly in view, as we have frequently urged, the general principles of scientific investigation, and by exercising yet greater caution in the analysis and comparison of results than is elsewhere necessary, in accordance with the greater complexity of the conditions under which vital phenomena occur, and the numerous sources of fallacy to which our inferences are liable, that such researches can acquire any real value; and it is consequently the more desirable that a thorough acquaintance should be first gained with works like the one now before us, which gives as good a practical lesson as could be desired on the art of observing, experimenting on, and generalizing from, the phenomena presented by living animals.

Art. V.

Medico-Chirurgical Transactions. Vol. XXVIII.—*London,* 1845. 8vo, pp. 623. Six plates.

We have written so many prefaces to the notices of the preceding volumes of this work, that we can find nothing new to say on the general subject. We shall, therefore, proceed at once to review the contents of the present volume, giving the titles of all the papers, and commenting on such as most call for or best admit of commentary. And it must not be supposed that the papers not commented on do not contain matter of value or importance. All deserve perusal, and several not commented on by us will be found highly interesting.

I. *Case in which the vena cava inferior was obstructed from the commencement of the common iliac veins, and its cavity entirely obliterated between the entrance of the emulgent and hepatic veins;* by T. B. Peacock, M.D. Physician to the Royal General Dispensary, Aldersgate Street.

II. *On the classification, structure, and development of the echinococcus hominis, showing reasons for regarding it as a species of cysticercus;* by Erasmus Wilson, F.R.S., Consulting Surgeon to the St. Pancras Infirmary; Lecturer on Anatomy and Physiology in the Middlesex Hospital.

III. *A case of aneurism of the popliteal artery, cured by compression of the femoral artery;* by E. Greatrex, Esq. Surgeon, and W. T. C. Robinson. Esq. Assist. Surgeon of the Coldstream Guards.

IV. *On extravasations of blood into the cavity of the arachnoid, and on the formation of the false membrane, which sometimes envelopes these extravasations;* by Prescott Hewett, Esq. Curator of St. George's Pathological Museum.

The effusions, which are the subject of this last paper, are found in four different states. In the first and simplest, the blood recently extravasated, is found either liquid, or coagulated in the form of clots or of a thin membranous layer, covering a greater or less extent of the surface of the brain. If life be prolonged for any time, this membranous layer becomes partially, or almost altogether deprived of colouring matter, and presents the appearance of a false membrane, which may be found loose in the arachnoid cavity, or, which is by far the most common, be pretty firmly connected with the parietal arachnoid. This is the second state. When thick the membrane is often divisible into distinct layers. Clots of blood, and cysts containing serum and blood, or serum alone, are occasionally met with in the substance of the adventitious tissue. An effusion of this kind has been frequently mistaken for the result of inflammatory action. In the third state, the extravasated blood is fixed to the free surface of the parietal arachnoid by a fine, delicate, transparent membrane, apparently possessing all the characters of the serous membrane itself, of which it at first sight appears to be a part. The blood in these cases, may either be collected in one cavity, or disseminated in patches over the surface of the serous membrane, the intervening parts of which present a natural aspect. In the fourth division, the blood is contained in a completely closed bag, which

may be detached from the parietal arachnoid, and with its contents removed unbroken. In the great majority of cases, such encysted collections correspond to the upper surface of the cerebral hemispheres. If the disease be of some standing, the cavity of the cyst may be perfectly smooth and polished, or it may be intersected with fibro-cellular bands, running in various directions; sometimes fibrinous clots are found adhering to this internal surface. After a certain period, the walls of the cyst become fully supplied with blood-vessels, and the cyst itself possesses all the physiological characters of an original serous membrane.

How are these cysts and attaching membranes formed? It is commonly believed that the irritation of the effused blood gives rise to an exudation of lymph; but this explanation, according to Mr. Hewett, is altogether unsatisfactory as regards the majority of cases, seeing that any evidence of inflammation, beyond their presence, is altogether wanting. He believes they are formed by the blood itself; and in support of this view brings forward several examples in which similar effusions, in various localities, have become thus surrounded without the supervention of any inflammatory process.

V. *On the colostrum of the cow;* by John Davy, M.D. F.R.S., Inspector General of Army Hospitals, F.R.S.

VI. *Case of obstruction of the large intestine, in which the ascending colon was opened with success, the patient dying three months afterwards of another disease;* by Samuel Evans, Esq. of Derby.

This is a very interesting case. The advanced stage of the disease and the general condition of the patient rendered success most doubtful; yet the patient recovered perfectly from the effects of the operation, and had regained a good measure of health and strength, when imprudence in diet and exercise brought on an attack of peritonitis, with diabetic urine, which proved fatal. The operation performed was Callisen's modified by Amussat.

VII. *On the mortality in prisons, and the diseases most frequently fatal to prisoners;* by W. Baly, M.D., Physician to the Millbank Prison, and Lecturer on Forensic Medicine at St. Bartholomew's Hospital.

We beg to direct the special attention of our readers to this very valuable communication. In it the author has most conclusively shown, from extensive statistical researches, that the state of imprisonment induces a rate of mortality much higher than that which prevails among the same class of persons unconfined; that this mortality, in some few instances, is the result of certain endemic influences, but that the development of tubercular disease, under various forms, is by far the most efficient operative cause.

VIII. *Observations on cleft palate, and on staphyloraphy;* by W. Fergusson, Esq. Professor of Surgery in King's College, London.

After a few observations on the various forms of cleft palate, and a rapid sketch of the different operations which have been performed for its cure, the author describes the mode of proceeding which he has been led to adopt from a consideration of the anatomy and physiology of the parts. He believes that the chief mechanical obstacles to the junction of the margin in the mesial line, are the levator palati and palato-pharyngeus muscles, and he consequently advises the division of these, by which means

all motory power in the soft palate is, for the time being, destroyed. The following is a short account of his operation:

"With a knife whose blade is somewhat like the point of a lancet, the cutting edge being about a quarter of an inch in extent, and the flat surface being bent semicircularly, I make an incision about half an inch long, on each side of the posterior nares, a little above and parallel with the palatine flaps, and across a line straight downwards from the lower opening of the Eustachian tube, by which I divide the levator-palati muscle on both sides, just above its attachment to the palate. Next I pare the edges of the fissure with a straight blunt-pointed bistoury, removing little more than the mucous membrane; then, with a pair of long blunt-pointed scissors, I divide the posterior pillar of the fauces, immediately behind the tonsil, and, if it seems necessary, cut across the anterior pillar too; the wound in each part being about a quarter of an inch in extent. Lastly, the stitches are introduced by means of a curved needle, set in a handle; and, the threads being tied so as to keep the cut edges of the fissure accurately in contact, the operation is completed. These different incisions may be made in the order here detailed, or possibly it may be found most convenient to divide the palato-pharyngeus first, next the levator palati, and then to pare the edges when the muscular action has been taken off."

Two cases are narrated in which the operation was practised with success. The plan seems worthy of full trial, being founded on correct scientific principles.

IX. *On the pulsating tumours of bone, with the account of a case in which a ligature was placed around the common iliac artery;* by Edward Stanley, F.R.S., Surgeon to St. Bartholomew's Hospital.

There are three different sources of pulsation in these tumours. Most commonly the proximity of a large arterial trunk is the cause. Of this condition Mr. Stanley mentions several instances. In more rare cases the pulsation is dependent upon the development of blood-vessels and blood-cells, constituting a sort of erectile tissue within the tumour. The case in which the common iliac was recently tied, at St. Bartholomew's, was of this nature. It originated in the ilium, and was supposed to be an aneurism. The man died from peritonitis on the third day. Still more rarely the pulsation is caused by enlargement of the arteries of the osseous tissue.

The most interesting point in respect of these tumours is the extreme difficulty of diagnosis. They simulate aneurisms so closely that it is sometimes altogether impossible accurately to determine which disease the surgeon has to treat.

X. *Description of a malformation of the duodenum, with notices of analogous cases;* by Robert Boyd, M.D., Resident physician, St. Marylebone Infirmary.

The malformation was discovered in the body of a still-born male infant. The organ was somewhat of an oval shape, six inches long, and two inches in diameter at the lowest and widest part, at the termination of which the canal was completely closed by a transverse membrane. Two inches and a quarter above this a valve extended across, nearly half closing the gut; it proceeded from the concave side, the central attachment of the septum being opposite the mesentery. The stomach was natural. The remainder of the small intestines were very small, and the large intestines were not fully developed. The contents of the cranium were natural.

XI. *Case of remarkable hypertrophy in the fingers in a girl, with a notice of some similar cases;* by T. B. Curling, Lecturer on Surgery, and Assistant Surgeon at the London Hospital, &c.

XII. *On the ophthalmia of puerperal women;* by Robert Lee, M.D. F.R.S., Fellow of the Royal College of Physicians, London; Physician to the British Lying-in Hospital; and Lecturer on Midwifery at St. George's Hospital.

In this paper Dr. Lee brings forward several cases in proof of his opinion that this formidable and rapidly destructive affection of the eyes, is connected with, and consequent upon, uterine phlebitis. His views appear to us important, and consonant with truth.

XIII. *Additional observations on obstructions of the pulmonary arteries;* by James Paget, F.R.C.S., Lecturer on Physiology, and Warden of the College at St. Bartholomew's Hospital.

This is an appendix to a paper in the last vol. of the Transactions. It contains the account of an extremely interesting case, of a man under treatment for stricture of the urethra, who presented no symptoms of pulmonary obstruction, had been only heard to cough once very slightly, and casually mentioned that at times his breath was momentarily rather short. He died suddenly, while in the apparent enjoyment of perfect general health; the fatal event seemingly having been induced by the exertion of defæcation. No disease of any moment was found either in the heart or lungs, but nearly all the branches beyond the primary divisions of the pulmonary artery contained clots of blood, which Mr. Paget conceived to be from three to ten days old. Some very interesting questions are suggested to the mind by such cases as these. How can life be prolonged with so much comfort, while the circulation through such an important organ is so greatly obstructed? Mr. Paget believes this is effected by the proportionally diminished rapidity of the systemic circulation, which follows upon the decreased quantity of blood conveyed to the left ventricle from the lungs. But as the original causes continue in operation, the blood ceases, or nearly ceases, to flow in the systemic vessels; the brain and heart become deprived of their natural stimulus, and sudden or gradual death is the result. But how is the coagulation effected when there is no external cause of obstruction? Mr. Paget is of opinion that it depends upon an altered condition of the blood itself, by which its adhesive properties are augmented, and he points attention to the remarkable fact, that in many of the cases on record, (in the present among others,) the kidneys have been found more or less affected with Bright's disease.

XIV. *Two cases of anæsthesia and loss of motory function of the fifth nerve;* by James Dixon, Esq., Assistant Surgeon to the Royal London Ophthalmic Hospital.

These cases are carefully observed and well narrated, and illustrate some interesting points in physiology.

XV. *Account of a case of external and internal cephalhœmatoma, complicated with fracture of the right parietal bone in a new-born infant;* by Charles West, M.D., Lecturer on Midwifery at the Middlesex Hospital, Physician to the Royal Infirmary for Children, and Physician Accoucheur to the Finsbury Dispensary.

The subject of this case, a female infant, lived for twenty-five days, and

then died in convulsions, having exhibited no head symptoms until two days before death. The blood was effused under the pericranium, and between the skull and dura mater; it had evidently been poured out in successive layers. The internal clot was nearly surrounded by a ring of newly-formed bone. There was no reason to believe that the tumour resulted from unnatural violence.

XVI. *Large opening into the anterior part of the urethra, caused by sloughing, and attended by considerable loss of structure, successfully treated by operation, with remarks;* by F. Le Gros Clark, Esq., Assistant Surgeon to St. Thomas's Hospital.

This opening was in the portion of the urethra anterior to the scrotum, and extending a very little way into the scrotal division. Its length, when the penis was turned up, was about 1¼ inch, and the two margins were so far separated as to leave the upper wall of the urethra entirely exposed. The following is Mr. Clark's account of the operation, which is a modification of that proposed by Dieffenbach:

"A large-sized (No. 10) elastic catheter was passed into the bladder, and the stilet being removed, an assistant was directed to hold the penis against the pubes; with a small scalpel I then first removed an inverted portion of skin which bounded the opening posteriorly at the scrotum. I next made, in succession, four incisions through the integument, each commencing at a little distance from the extremity of the urethral deficiency: two of these extended, in contrary directions, upwards and outwards on the sides of the penis, the other two downwards and outwards on the scrotum. Each pair of incisions thus included a nearly rectangular piece of skin between them; and a flap on each side was thus marked out, which grew broader as it extended outwards. I then proceeded (*without* paring the edges, on which I did not at all depend for adhesion) to dissect up these flaps, commencing from the margin of the urethra and advancing outwards, so as to raise the two flaps in question. This being effected, I waited for ten minutes or a quarter of an hour, until the surface-bleeding had been arrested, and then undertook the concluding step of the operation, which was to bring these flaps over the urethra, and apply them *surface to surface*, not edge to edge. For this purpose I had prepared four small quadrilateral pieces of stiff leather, about half an inch square, and perforated. These were applied, two and two, opposite each other, and kept in position by platina wires, which I passed through the leather of each side and the intervening folds of skin, and fastened by twisting the ends together. In this way the flaps were sustained in relation, forming a ridge which projected a little above the pieces of leather, a small intervening portion being left uncompressed. The operation was completed by a semilunar incision on either side of the penis, parallel to the flaps, in order to obviate the effects of any after tension; and three or four sutures were employed at the extremities of the wound."

The cure was eventually complete, and we think the case reflects great credit on the surgeon.

XVII. *The pathology of mental diseases;* by John Webster, M.D. F.R.S., Consulting Physician to St. George's and St. James's Dispensary.

This is a sequel to a paper, which we formerly noticed, in the 26th vol. of the Transactions, and contains the account of a number of further dissections of persons who died insane, which furnished the following general results. The number of dissections was 36. In 33 cases the pia mater was infiltrated. In 30, there was turgidity of the blood-vessels of the brain and its membranes. In 26, effusion of water had taken place in

the ventricles. In 16, there was thickening and opacity of the arachnoid. In 12, fluid was met with at the base of the brain. In 9, the consistence of the brain was altered from its normal condition. In 8, patches, or bloody points, appeared on the cut medullary surfaces. In 5, the colour of the medullary, or cortical substance, was altered from its healthy hue to a pink, mottled, or rosy tint; and in 4, blood was effused in the brain. There were also other morbid changes of structure of a less important character.

Dr. Webster gives, likewise, some tables which show that the prevalence of insanity increased with the increased temperature of the season, and that the greatest number of cures took place during the autumnal and winter months.

XVIII. *On some of the causes of pericarditis, especially acute rheumatism and Bright's disease of the kidneys; with incidental observations on the frequency and on some of the causes of various other internal inflammations;* by John Taylor, M.D., Professor of Clinical Medicine in University College, and Physician to University College Hospital.

This is one of those productions that defy analysis. The facts are too numerous for condensation, and too necessary for the argument to allow of omission or abridgment. We have rarely perused any paper that conferred more honour on the writer, and we feel that we are doing an act of simple justice in urging our readers, not merely to go through, but to study its contents. Expanded, and further illustrated, it would form one of the most valuable monographs we possess, and we sincerely hope the author will give it to the profession in this shape. The main object in view is to establish the very important fact, that by far the most frequent causes of pericarditis are two blood diseases, viz. acute rheumatism and Bright's disease of the kidney. This doctrine is supported by extensive statistical records, and, in our opinion, is conclusively proved. The final conclusions at which the author arrives are thus briefly stated. 1. That acute rheumatism and Bright's disease, *in its advanced stages*, have an *equal* tendency to produce pericarditis and endocarditis. 2. That acute rheumatism has a greater tendency to produce pericarditis and endocarditis than has Bright's disease *when in its earlier stages.* The originality of these views will be at once evident to all. They are worthy of the deepest attention, and open up a wide and most interesting field for future investigations.

XIX. *Case of excision of the upper end of the femur, in an example of morbus coxarius*; by W. Fergusson, esq., Professor of Surgery in King's College, London.

The case terminated most favorably, being only the second of the kind which has had a like result in Britain.

XX. *On the minute structure of the lungs, and on the formation of pulmonary tubercles;* by G. Rainey, esq., M.R.C.S., Microscopist to St. Thomas's Hospital.

The lungs are made up of bronchial tubes, bronchial intercellular passages, and air-cells. The intercellular passages are the continuation of the bronchial tubes, the distinction between the two being the deficiency of the mucous membrane, which terminates abruptly about one eighth of an inch from the surface of the lungs, the remainder of the passage and the

air-cells being lined by a very fine, delicate, and distinctly fibrous membrane which has no epithelium. The intercellular passage terminates in an air-cell, which is not dilated, as stated by Reisseissen, but of about the same diameter as the passage. We can bear testimony to the accuracy of this observation.

The air-cells are small, irregular, but generally four-sided, cavities of various sizes, the most central being smallest and most vascular; they communicate freely with each other, and with the intercellular passage. The walls or partitions, by which they are separated from one another, consist of a single plexus of vessels, inclosed in a fold of membrane. The structure, above described, can be seen in the fœtal lung; and, therefore, the cells are not produced, as Mr. Addison describes, by dilatation of the "lobular passages."

According to Mr. Rainey's observations, tubercular matter is poured into the cells from their lining membrane, gradually fills them, compresses the septa, and obliterates the vessels, and thus gradually becomes disintegrated. He denies the inflammatory origin of this morbid product.

XXI. *Two cases of aneurism, in which there was neither pulsation nor abnormal sound;* by T. A. Barker, M.D., Physician to St. Thomas's Hospital.

XXII. *An account of a singular case, in which there was a black secretion from the skin of the forehead and upper part of the face;* by W. Teevan, esq., M.R.C.S.

Art. VI.

Guy's Hospital Reports. Second Series. Nos. IV, V, and VI; Oct. 1844; April and Oct. 1846.—*London.*

We feel ourselves in the same predicament with these Reports, as with the Medico-Chirurgical Transactions previously noticed. We shall, therefore, proceed at once to our task without further ceremony.

No. IV.

I. *Case of poisoning by opium;* by A. S. Taylor. This case is interesting from the quantity of poison taken, and the length of time that elapsed before death. The subject was a male child, aged 14 months: it must have had at least three grains of opium, and probably much more, and yet it lived for eighteen hours. No poison could be detected in the contents either of the stomach or intestines.

The narration of the case is followed by some valuable remarks on the various tests for opium, and the method of employing them. As a *trial-test,* Mr. Taylor gives a decided preference to the solution of sesquichloride of iron, which will detect the meconic acid contained in the 160th part of a grain of opium. "In order to employ it, a portion of the suspected liquid, if viscid, should be slightly diluted with water; if coloured, it should be so diluted that any change of colour, on adding the test, may be at once perceptible. The test should be saturated and neutral, or as nearly so as possible; it should be added guttatim to the suspected liquids, an equal portion of the untested solution being placed by the side of the glass, for the sake of comparing the effects. If opium be present, a dark

red colour will be immediately brought out (permeconate of iron); and it will be found that this red colour is not destroyed by the addition of a few drops of a solution of corrosive sublimate. If no change of colour should be produced by the sesquichloride, and the liquid be, at the same time, in such small quantity as to admit of no further reduction in bulk by concentration, then it will be useless to seek for opium; as the quantity, if any be present, will be too small for any known process of separation."

It is a question often asked by juries,—how small a quantity of such and such a poison can you detect? To supply an answer to this question in regard to opium, Mr. Taylor performed a series of experiments with the following results.

In the tables below, the first column represents the absolute quantity of the substance detected; the second, the quantity of opium to which it is equivalent; the third, the weight of water or liquid compared with the absolute weight of the substance tested; and the fourth, the actual quantity of liquid used in the experiment:

	1 Mur. Morph.	2 Opium.	3 Dilution.	4 Water.
Nitric ac. . . .	the 15th gr.	(=1·2 gr.)	300	20 drops.
Sesquichlor. iron .	11th	(=1·6 gr.)	231	21
Iodic acid . . .	100th	(=0·18 gr.)	1300	13

From this it is clear that iodic acid is the most delicate test for morphia, but it is open to the greatest number of objections, for an account of which we must refer to the original.

The next table shows how small a fractional quantity of meconic acid Mr. Taylor has succeeded in detecting by means of the sesquichloride of iron. The columns have the same significance as in the former one.

1 Meconic ac.	2 Opium.	3 Dilution.	4 Water.
the 220th gr.	(= ·075 gr.)	880	4 drops
377th gr.	(= ·043 gr.)	13572	35
570th gr.	(= ·028 gr.)	14820	25
880th gr.	(= ·018 gr.)	14080	16
2640th gr.	(= ·006 gr.)	15840	6

It must be remembered that in these experiments the substances used were pure.

II. *On the action of digitalis, and its uses in diseases of the heart*; W. Munk, M.D., Physician to the Tower Hamlets Dispensary.

This is one of those performances which we hail with the greatest satisfaction; were they more numerous we should be in a better position to contend with disease, and (what is of far smaller importance, but still of some moment) to answer the frequent taunts of our homœopathic opponents.

Dr. Munk has drawn his conclusions from upwards of 400 experiments with this drug, made with care, and recorded with accuracy; and he has, we think, established some very important points.

It is well known that digitalis exerts its influence specially on two organs—the heart and the kidneys. Now it appears from the researches before us, that these results depend very much upon the preparation employed—the *tincture* affecting the heart—the *infusion* acting upon the kidneys. If it be desired to lower the action of the heart *decidedly*, as in cases of hypertrophy, the tincture should be given *alone*, in

moderately full doses. If we wish to relieve the palpitations, dyspnea, &c. which form so large a portion of the distress of those who suffer from valvular disease, dilatation, &c. the tincture should be given in combination with camphor, assafœtida, musk, or other antispasmodics. In either case the patient should abstain from all exertion of mind or body. A plethoric condition is unfavorable to the action of the drug, and should be removed before its administration.

When the diuretic action is required, the *infusion* should be given in doses of from half an ounce to an ounce every six or eight hours, and the patient should take moderate exercise, and have the loins warmly clad, avoiding the production of diaphoresis.

Dr. Munk suspends its use if the pulse falls below 60, and does not persevere longer than a week, if the medicinal effects are not readily produced. With these precautions he has rarely seen any injurious effects from its employment.

III. *Remarks on the pathology of iritis;* by J. F. France, Assistant Surgeon to the Eye Infirmary.

The principal object of this paper is to establish the reality of the distinction between *syphilitic* and *arthritic* iritis, to point out the chief diagnostic marks, and the modification of treatment required.

In well-marked cases of syphilitic iritis, the first morbid changes in the membrane itself are commonly observed at or close to the margin of the pupil, where the iris is found not only to have its mobility impaired and its brilliancy tarnished, but the sharpness of that margin lost; and, as the disease advances, the natural colour of the iris, at one or more inflamed points, which appear thickened, is seen to give place to the development of a dusky reddish-brown hue. This colour, as the disease and tumefaction advance, becomes more striking, but it is sometimes obscured to a certain extent by fibrin effused into the cavity of the anterior chamber. Generally, however, there is only sufficient exudation from the free surface of the aqueous membrane to attach parts of the edge of the pupil to the capsule of the lens, and perhaps cover, more or less, its contracted aperture. In some cases, very minute dark-brown circular specks, with well-defined margins, appear in the lower segment of the cornea, and these, when present, are strongly presumptive evidences of the nature of the disease. The rusty colour of the tumefied spots on the iris Mr. France has found to depend upon a network of distended vessels ramifying over them.

In arthritic iritis the dullness of the anterior chamber is much more extensive and considerable; and, if the disease be not quickly checked, the entire circle of the pupillary margin is soon fixed by lymph to the capsule of the lens, and even the whole area of the pupil may be thickly overspread with this effusion, which presents very generally a white or yellowish-white appearance. The surface of the iris is not partially discoloured in rusty-looking spots, but has an uniform greenish or yellowish hue. In fact, in this form, the lining of the anterior chamber and the serous covering of the iris are, in common with the sclerotic, the principal seat of disease, while, in the syphilitic form, the inflammation chiefly attacks the parenchymatous structure.

In treatment of the arthritic variety, antiphlogistic measures must be

more sparingly used than in the syphilitic, and the constitutional effect of mercury is not required. Conium sometimes exerts great influence over this form of disease when given in doses of five or ten grains thrice daily. An early recourse to tonics and alkalies is generally requisite.

The paper is illustrated by several well-narrated and instructive cases.

IV. *Account of a specimen of partial fracture of the neck of the thigh-bone, and of the proper source of nutrition of the head of the bone;* by J. Wilkinson King. With two plates.

V. *On paracentesis thoracis. Additional cases;* by H. M. Hughes, M.D. The whole number of cases now amounts to 25. Of these 13 have fairly recovered, 2 have at least partially recovered, and ten have ultimately died of other diseases, generally connected with that for which the operation was performed, but entirely independent of its performance.

VI—and No. VI, VIII. *Select clinical reports, with observations;* by George H. Barlow, M.A., M.D. We join these two valuable communications together, because the facts and arguments in both are directed to the establishment of the same general principles; and we may state, *in limine,* that we have rarely met with a better specimen of the application of sound inductive reasoning to the elucidation of any point in pathology.

The first portion is occupied with the narration of two cases of fatal obstruction of the bowels. In the first case, the seat of obstruction was in the colon, the sigmoid flexure of which twisted upon itself, so as to cause two points of constriction. In the second, the jejunum was compressed by a band of false membrane, which also narrowed the cavity of a portion of ileum passing under it. In neither case was the cavity of the gut entirely closed. But, though the pathological conditions were so similar in nature, the symptoms varied in some important respects. In the first case there was little or no abdominal pain at the commencement; in the second it was one of the earliest symptoms. In the first case there was great flatulent distension of the abdomen; in the second there was none. In the first the urine was abundant in quantity; in the second that secretion was almost entirely suppressed. In the first case the circulation was well maintained in the extremities, until symptoms of perforation manifested themselves; in the second there was early collapse. Now these differences were clearly dependent upon the variation in the seat of obstruction, and accordingly, from a consideration of them and the results of other case, Dr. Barlow has grounded upon them the following rules of diagnosis in these often obscure cases:

"It appears, then, that in cases of constipation, where there is great abdominal distension, with little or no sickness till a late period of the disease, and an abundant secretion of urine, the seat of the obstruction is in the colon or rectum; and if there be no great degree of pain or tenderness, this obstruction is probably dependent upon some mechanical cause. When, on the other hand, the abdomen appears empty, or but moderately distended—when there is early vomiting and great deficiency of urine—and, more especially, when to these symptoms there are added considerable pain, with a tendency to collapse, the affection is probably in the course of the small intestines, and, for the most part, near the pylorus; but when the last-named symptoms are present, with the exception of deficient urine, the disease is either in the cæcum or lower part of the small intestine."

In respect of treatment, Dr. Barlow most wisely deprecates the perse-

vering use of drastic purgatives, as being inefficient in themselves, and calculated to produce serious mischief.

"Supposing, then," he says, "in a case of constipation which has lasted several days, that it appears probable, from the distension of the abdomen, the absence of sickness, and an abundant secretion of urine, that the obstruction is situated in the lower bowels, the first step should be to exhibit some efficient but not irritating purgative, such as a full dose of calomel with half a grain of opium, followed, in a few hours, by castor-oil, which may be again repeated—or four or five grains of compound extract of colocynth, with one of extract of hyoscyamus, may be given every hour, for six or eight hours; or, should time be allowed by the absence of any very urgent symptoms, both the above means may be tried. Copious enemata should also be administered once or twice in the twenty-four hours; and where there exists the slightest suspicion of any inflammatory action, a full bleeding should be early had recourse to.

"Should no evacuation be obtained by these means, it may be well to explore the rectum with a finger or a bougie, or both, with a view to ascertaining the existence of any obstruction in that part of the bowel. The flexible tube should next be cautiously introduced, and the lower part of the bowel well washed out by copious injections of warm water, which may be drawn off by the stomach-pump. After this has been done two or three times, there will generally be an opportunity of feeling the extremity of the tube through the abdominal parietes, by which its direction and the position of the bowel may be ascertained. . . . . . . Should there be a simple distension, it is probable that the upper portion of the intestine will empty itself into the distended part; and, with a view of promoting this, a moderate purgative may again be administered, and a turpentine enema thrown up by the flexible tube. But, should no evacuation be procured in this manner, it will become probable that there is a second obstruction above the distended portion. The farther exhibition of purgatives by the mouth will then be useless, or even injurious, and our efforts should be limited to the persevering washing out of the lower bowels, and the occasional exhibition of calomel and opium; as it may happen even yet, that, by diminishing the distension, the pressure at the points may be so far diminished as to allow of the passage of the contents of the bowel."

Should, however, all these means fail, it will be advisable to open the colon in the loins.

We now come to the inquiry, how does obstruction of the upper part of the intestinal canal influence the secretion of urine? And this leads our author into a very interesting discussion, in which he attempts, and we think successfully, to prove the truth of the following proposition:

"If a sufficient quantity of water cannot be received into the small intestines, or the circuit through the portal system into the vena cava ascendens, or thence through the lungs and heart into the systemic circulation, be obstructed, or if there be extensive disorganization of the kidneys, the due secretion of urine cannot be effected."

Into the steps of his argument we have not space to enter: they consist of a series of cases, illustrating in succession the effects of obstruction in the several parts of the circuit above noticed, and the existence, in each, of an abnormal condition of the urinary secretion. Thus, in the case already alluded to, the sickness prevented the reception of a sufficient quantity of water into the system; in case 4, the ascending cava was filled with morbid deposit; in cases 5 and 6, there was obstruction to the circulation of blood through the lungs; in case 7, obstruction to the return of blood from the lungs; in case 8, obstruction to the passage of blood

through the heart; and in case 9, obstruction to the passage of blood from the heart. In all these the one constant symptom was a great deficiency in the quantity of urine. The series is completed by the narration of a very remarkable case, in which, from extensive disorganization of the kidney, scarcely any solid matters were eliminated by that organ, and the patient died comatose, though a large quantity of watery fluid was passed daily. We strongly recommend these papers to the careful attention of our readers. They are illustrated by five plates.

VII. *Medical reports from the books of the clinical wards, with remarks;* by J. C. Brereton. These are interesting, and well worth reading; but do not admit of analysis.

VIII. *Report of cases of injuries to the abdomen.*

IX. *An abstract of the two half-yearly Reports of the Clinical Society, for* 1843; by E. L. Birkett, M.B.

No. V.

I. *On the pathology of phthisis;* by T. Addison, M.D., Senior Physician to the Hospital. With five coloured plates.

In this paper the author pursues the course of his former investigations, bringing forward additional evidence in support of his position, that indurations of the substance of the lungs may give rise to all the ordinary signs of tubercular phthisis, and prove fatal, without there being a single tubercle in any part of those organs. To this form of disease he gives the name of *pneumonic phthisis.*

This pneumonic phthisis may be acute; the deposits and inflamed tissue softening down and disorganizing at once, without any attempt whatever being made at induration or repair, thereby constituting one form of acute or galloping consumption. It may be *acuto-chronic;* of which there are three varieties. 1. The inflammation, though more or less acute, is slower and more insidious in its course, and manifests some attempts at repair, as indicated by various stages and degrees of induration. The induration, nevertheless, is not complete; the pulmonary tissue continues to be friable; and sooner or later, that is to say, in a few weeks or months, softens down and gives rise to excavation; most frequently by a slow ulcerative process; more rarely by an actual slough of greater or less portions of the indurated but still friable pulmonary tissue. 2. Inflammation may supervene upon or around ancient induration, leading to disorganization either of the newly-inflamed tissue, of the old induration itself, or of both at the same time. Lastly, pneumonic phthisis may be *chronic;* of which, also, there are two varieties: first, that in which old indurations undergo a slow progress of disintegration, giving rise to vomicæ; and, secondly, that very rare form of the disease, in which an insidious inflammation proceeds very slowly to convert a considerable portion of pulmonary tissue into gray induration, without any necessary excavation whatever.

Tuberculo-pneumonic phthisis. By this is meant a very common form of the complaint, in which, although tubercles are present, the really efficient cause of the phthisical mischief is pulmonic inflammation.

Dr. Addison distinguishes two forms of simple pulmonary tubercle, which he designates by the names *sthenic* and *asthenic:* the former being

usually of a gray colour, and semi-transparent; the latter, from the very first, and however minute, of an opaque white, or boiled-rice colour, sometimes with a faint tinge of yellow. It is the sthenic variety that usually preponderates in the form of phthisis we are now considering.

"Pulmonary tubercle has its seat in the delicate filamentous tissue which forms the slight filmy parietes of the air-cells, and bears the same relation to these parietes that tubercle does to serous membrane.

"The apparent growth and increase of size of simple pulmonary tubercle depend upon changes taking place in adjacent cells, either from the development of additional tubercles, or from inflammation. The so-called enlarged tubercles, therefore, are in reality either aggregations of simple tubercles, or simple tubercles inclosed in the products of inflammation. To the former I would apply the term *compound tubercles*, which, of course, are made up of two distinct elements, viz. the abnormal product, tubercles, and the tissue of the air-cells in which that tubercle is developed. The latter element of compound tubercle, although little considered, probably plays a by no means unimportant part in some of the most serious changes observed to take place in tuberculated lungs. Compound are at all times more liable to disintegration than simple tubercles.

"When tubercles are present, and especially when numerous, in clusters, or compounded, but still quiescent, the neighbouring pulmonary cells pretty uniformly afford indication of compensatory or excessive function; the cells being more or less enlarged, as observed in pulmonary emphysema. This compensatory change probably increases the difficulty of recognizing simple tubercles in the lungs; it being rarely, if ever, possible to detect them, either by dullness of sound or percussion, or by diminished respiratory murmur, as determined by auscultation—unless, perhaps, they exist in great numbers, and in groups of considerable size.

"In general, the only physical indication worthy of the smallest confidence at a very early period is a certain degree of inequality in the respiratory murmur; it being a little more loud or puerile at certain points, or in one lung than the other.

"When tubercles are present in the pulmonary tissue, there exists a great proneness to increased vascular action, congestion, or inflammation in the lungs themselves, and their appendages, the larynx, trachea, bronchi, bronchial glands, and pleuræ. This tendency is, however, most strongly marked in the immediate vicinity of the tubercles. Unless subjected to causes calculated to aggravate such tendency to congestion or inflammation, an individual may experience little or no inconvenience from the presence of tubercles;—a pathological principle applicable to sthenic tubercles generally, whether situated in the lungs, the pleuræ, peritoneum, or arachnoid. The inflammation which supervenes upon tuberculated lungs is commonly of the low and insidious kind observed in scrofulous and cachectic constitutions; and, unless under aggravation, is rarely attended with sufficient serous exhalation to produce perfect pneumonic crepitation.

"The commencement of this inflammation, either in the pulmonary tissue itself, or in the bronchial tubes, is the ordinary commencement of tuberculo-pneumonic phthisis; and hence it is that most of the symptoms and physical signs of phthisis may manifest themselves, without evidence of a primary change in the tubercles themselves, being discoverable after death. When the pulmonic inflammation is more considerable than usual, the characteristic heat of skin often remains uninterruptedly for days together, whether accompanied by diminished, increased, or varying perspiration.

"The physical signs which become apparent as the disease advances, viz. feebleness or absence of respiratory murmur, bronchophony, tubular respiration, and dullness of sound on percussion, are more the results of this inflammation than of the extension of tubercles; and may occur from a similar cause, in pneumonic

phthisis, without a tubercle ever having existed in the lung at all. These is often a well-marked relation to be observed between the degree and permanency of the characteristic pneumonic heat of skin when present, and the rapidity and extent of these local changes, as ascertained by auscultation and percussion.

"Although the red hepatization occurring in tuberculo-pneumonic phthisis very often passes quickly into softening, and the consequent formation of a cavity, nevertheless, when actual albuminous matter is thrown out, it, like that resulting from pneumonic inflammation without tubercle, usually manifests some attempts at repair; as indicated by more or less hardening and contraction of the deposit itself, and of the pulmonary tissue into which it is effused. These results of inflammation, have been very commonly, but erroneously, regarded as mere varieties of tubercular infiltration. It is the contraction of these deposits that chiefly occasions the diminished size of the lung and flattening of the ribs. Pleurisy, doubtless, very often contributes to the same result; but mere excavation of a portion of a lung has no such effect as diminishing its size. In the worst and most excavating forms of emphysema there is no shrinking of the lung.

"The attempts at repair, or induration, of the pneumonic deposits occurring in tuberculo-pneumonic phthisis are commonly very imperfect, and not durable; so that the deposits for the most part, sooner or later, undergo a second change, by which they soften down and produce excavation. This softening, however, may take place days, weeks, months, or, I believe, even years, after the original deposition. When the disease has proceeded to excavation, the natural cure of the ulcer thus produced consists in the formation of a more or less permanent lining membrane—the true cicatrix of such ulcers. Although this membrane may perhaps, now and then, remain passive and harmless for years, it most commonly happens that the cicatrization is imperfect and incomplete; the efforts at repair fail, the albuminous material, which ought to form the membrane, softens down, and with it successive portions of the pulmonary tissue furnishing it; and thus the ulceration proceeds, till, exhausted by unceasing irritation and imperfect nutrition, the patient dies."

In treating this form of phthisis, our main object should be to prevent the deposition of tubercles, by improving the general health; and when they are found to oppose the progress of inflammatory action, by moderate general and local bleeding, active and continued counter-irritation, &c., and sometimes by the use of mercury.

Tubercular phthisis is characterized by a preponderance of compound asthenic tubercles; and, in Dr. Addison's opinion, is incurable. His remarks upon it are most judicious; but as this is the form of phthisis with which our readers are most familiar, we shall be contented with simply directing their attention to the paper itself.

Dr. Addison's papers claim the best attention of the pathologist and practical physician.

II. *Cases in medical jurisprudence, with remarks;* by A. S. Taylor.

This communication is occupied with the subject of poisoning by prussic acid, and is full of interest. The author has, we think, conclusively shown that the *shriek*, concerning which so much has been written and said of late, is no necessary accompaniment of poisoning by this drug; that the same is true of convulsions; that, after even a large dose has been taken, there may be sufficient time and consciousness for the individual to cork and put away the bottle, and arrange the bedclothes; that the absence of odour is no proof of the absence of the poison; and that, on the other hand, the odour may often be detected when chemical analysis gives only negative results.

In regard to tests, the nitrate of silver and the prussian-blue tests, are of nearly equal value in practice, though the former is in reality the most delicate. We must quote a short passage illustrative of the way in which the silver may be used successfully as a trial-test, even when the poison is extremely diluted.

"A small quantity of the diluted liquid was placed in a watch-glass, another watch-glass, containing one drop of a solution of nitrate of silver, was inverted over it. On holding the two glasses in the warm hand for a few minutes, a fine film of cyanide of silver was deposited over the upper glass in and about the spot wetted by the nitrate, care being taken that there should be no contact of the two liquids. This experiment shows not only the extreme volatility of the poison, even when mixed with 10,000 times its weight of water, but also that the nitrate of silver is an extremely delicate trial-test, which may enable us to detect the poison, even in a liquid which has no odour. It is, however, absolutely necessary, in such a case, that we should take advantage of the volatility of the acid, and not apply the test at once to the diluted liquid."

III. *Illustrations of some of the forms of sudden death from disease, as it occurred to nineteen Individuals in the Manchester Workhouse, with remarks bearing upon the diagnosis and pathology of such cases;* by D. I. T. Francis, M.B.

This is a useful paper, and well arranged. It will repay perusal.

4. *Reports of cases of diseases of children, treated at Guy's Hospital in* 1843-4, *with remarks;* by Golding Bird, A.M., M.D.

The cases narrated include remittent fever, croup, infantile syphilis, and scarlatinal dropsy. We have only space to remark that the occurrence of the last-named affection is attributed by Dr. Bird to the retention of some of the morbid poison of scarlatina in the blood, the free action of the skin being at the same time impeded, or suspended by want of due care. We must also transcribe his account of a very simple and easy process for the detection of urea in the blood and serous fluid:

"Allow the blood to coagulate, decant the serum, and agitate it violently with its own bulk of rectified spirit; a dense deposit of albumen occurs, and the mixture may be set aside for subsequent examination, or, if time permits, this may be proceeded with immediately. For this purpose throw the whole on a filter, and evaporate the filtered fluid slowly to a drachm or two; then add to it an equal bulk of dilute nitric acid of the Pharmacopœia, and once more filter. The filtered fluid, collected in a watch-glass, may be slowly evaporated to a few drops, and, on cooling, feathers of nitrate of urea will form in the liquor. Should the crystallization be imperfect, the deposited nitrate may be redissolved in a few drops of water, the solution decanted, and once more slowly evaporated."

In the second, non-inflammatory stage of hooping-cough, Dr. Bird has found alum a most valuable remedy, diminishing the secretion and relieving the cough. The following is the formula he has generally used for children of two or three years: ℞. Aluminis, gr. xxv; Extr. conii, gr. xij; Syr. rhæad., ʒij; Aq. anethi, ℥iij. M ft. cochl. j; med. 6tâ. quâque horâ.

5. *Report of cases of Injuries to the Chest.*

No. VI.

I. *Two cases of labour, protracted by insuperable rigidity of the os uteri, with remarks;* by John C. W. Lever, M.D.

In the first of these cases the os and part of the cervix uteri were forced

off during a pain. The patient died of puerperal fever. In the second, the rigid and unyielding os was incised on each side, and the patient did well. Dr. Lever believes this to be the best method of procedure, when the obstacle will not yield to the effects of bleeding, antimony, opium, warm bath, &c. He advises the incisions to be made at the sides, and during a pain. Artificial dilatation he considers most dangerous. We agree with him that in such extreme cases it is not advisable; but we are confident, from the results of considerable experience, that when the rigidity is of a minor degree, *gentle* dilatation with the finger is a most valuable adjunct to the other means, and we have never seen any injury produced by its judicious employment.

II. *Trial for murder by poisoning with arsenic, Berkshire Lent Assizes,* 1845. *Post-mortem appearances; chemical analysis; detection of the poison as sulphuret, twenty-eight days after interment;* by Alfred S. Taylor.

III. *Case of ossification and dislocation of the crystalline lens, with remarks;* by J. F. France.

This is an account of an extremely rare morbid change, resulting from injury received twenty years before the patient was seen. The dislocation took place spontaneously after that prolonged period. The lens was analysed by Dr. Rees, and found to contain, carbonate of lime, 0·5 gr.; phosphate of lime, 0·3 gr.; animal matter, 0·2 gr.

IV. *Case of popliteal aneurism, and of ligature of the femoral artery;* by John Nottingham, M.D., Liverpool.

A needlessly elaborate record of a very ordinary case.

V. *On the pathology and treatment of fracture of the neck of the thigh-Bone*; by Bransby B. Cooper, F.R.S.

The object of this paper is to demonstrate the impossibility of osseous union of fracture within the capsule, and, consequently, the uselessness and impropriety of employing the various contrivances which have, from time to time, been proposed for this purpose. The most novel portion is the record of some analysis of the proportion of earthy matter in the neck and shafts of old bones; an abstract of which we subjoin:

AVERAGE.

1. Fractured neck of the femur yielded per cent. of bone earth	23·9	
Shafts of same bones		50·1
2. Unfractured neck of old bone, not buried	33·5	
Shafts of same bones		55·5
3. Unfractured neck of unburied bone of middle-aged person	50·1	
Shaft of same bone		56·7
4. Unfractured neck of old bones, very dry and exhumed	61·4	
Shafts of same bones		64·9

VI. *Abstract of the two half-yearly reports of the Clinical Society for* 1844. Part I. Medical Report; by E. Lloyd Birkett, M.B. Part II. Surgical Report; by Robert Gosset, Surgical Secretary to the Clinical Society.

VII. *Two cases of extra-uterine fœtation;* by Dr. Oldham.

The first was an instance of tubal pregnancy; the second Dr. Oldham regards as an example of interstitial or parietal extra-uterine pregnancy. It presented a very curious anomaly; the right Fallopian tube having apparently received an ovum from the left ovary, and transmitted it to the

artificial sac, formed in the substance of the right horn of the womb. This result was probably brought about through the medium of false membranes, which were very numerous; but some degree of uncertainty hangs over the whole case, from the fact that the ovum was not found. Death was caused in both instances from the rupture of the large vessels in the walls of the containing sac. In both the uterus was lined with decidua.

These Reports are in the hands of so many of our readers, that we the less regret being compelled, for want of room, to give so imperfect a notice of them.

Art. VII.

Inaugural Dissertation on Yellow Fever, and on the Treatment of that Disease by Saline Medicines. By G. F. Bone, M.D., Assistant Surgeon to the Forces. With an Appendix, *On the Principles to be observed in providing Barracks and Hospitals for Troops in the West Indies.* By Hugh Bone, M.D., Inspector-General of Army Hospitals.—*Edinburgh,* 1846. 8vo, pp. 85.

Inaugural dissertations, generally speaking, have a very characteristic amount of schoolboy orthodoxy about them. It cannot be expected of them to contain much practical matter gleaned from experience; and it is only rarely that the student adventures any original investigations that are other than crude, or superfluous, or commits himself to research deeper than that of culling facts and phrases from the leading text-books of the day. We should, from internal evidence, have inferred that the pamphlet before us had a collegiate origin: it is so redolent, in places, of Hippocrates, Celsus, Gregory, and practice of physic-lectures. But, beyond these graduation merits, it possesses others of a different stamp, for which the author is indebted to the practical experience and judgment of his father. We cannot help thinking, however, that Dr. Bone, sen. would have done himself more justice, and medical science more service, had he presented a monograph on yellow fever to the profession, altogether his own production, and embodying the results of his own practice, instead of confiding the task to one who is manifestly a very inexperienced writer. Though there are no less than twenty "*corrigenda*" in this *brochure* of eighty-five pages, there are many sins against literary taste and skill that still remain uncancelled; and many of these faults not easily to be overlooked in the present age of literary and technical precision.

Dr. Bone's definition of yellow fever occupies eleven lines and a half. He says, "it is not brief, and is perhaps redundant, but it is reciprocal (?) with the disease defined, for whoever has these symptoms has yellow fever, and if I have failed in making this definition, I have failed in a bold attempt, for neither Dr. Good, nor Dr. Jackson attempted to define yellow fever." We are afraid Dr. Bone has not read these authors with the care that is due to them. He gives Dr. Good the precedence, though Jackson's work on fever appeared some twenty-four years before the first edition of Good's 'Study of Medicine'. Now, if Dr. Bone will take the trouble of turning to page 179 of Jackson

on Fever, edit. 1798, and over the rest of the pages intervening between this and page 205, he will find that they are all occupied with definitions of the disease. Twenty-six pages of definition, and not moderately minute, Dr. Bone considers nothing, whilst eleven lines and a half of his own he esteems redundant! As regards Good's account of the symptoms of yellow fever, if Dr. Bone will consult pages 635, 641 et seq. of the first volume of the last edition of the 'Study of Medicine,' he will find definition enough, at least in comparison with his own.

Dr. Bone believes that yellow fever is, under all circumstances, non-contagious; and he gives some striking illustrations of the fact:

"It is not the diluting breeze that stops the spread of yellow fever, but the removal from foul air, whether stagnant or rushing with great force. That this plan is successful, is proved by the following examples among many that might be quoted. The white troops in St. Kitt's were removed from Brimstone Hill and encamped on healthy ground in August 1840, and in February 1843, and in July 1844, and uniformly the yellow fever stopped making progress among them. The white troops, except 13 men of the Royal Artillery, were brought from St. Kitt's to Barbados in August 1844, and did not import the disease into Barbados. The 33d regiment were moved from the stone-barrack, Barbados, to Gun-hill encampment ground, on the 3d December, 1841, and immediately became healthy." (p. 18.)

"Fifty of the patients were yellow, and eleven of them died, and were all carefully dissected by him (Dr. Bone, sen.) or by his assistants, yet none of them caught yellow fever, nor any of the other patients in the quarantine hospital, nor any of the hospital servants or washerwomen, nor any of the patients or servants in the hospital from which the yellow patients were taken. He calculated that 700 persons had been exposed to the influence of the disease, yet none caught yellow fever. The cordon of troops did their duty, the British strictly, the Spanish with ferocity, but could not prevent all intercourse with the quarantine hospital. One evening, when one of his assistants, Mr. Williams, assistant surgeon, 23d regiment, made his visit to the female ward, he discovered that one of the female patients had her sweetheart, a sergeant from the depôt, in bed with her. She was then yellow as an orange, and the disease was running its course, but the sergeant did not catch yellow fever. There was in the same ward another female who contrived to see her sweetheart while she was affected with the disease, and he did not catch yellow fever." (pp. 19, 20.)

A still more striking case illustrative of the incommunicableness of yellow fever, than any Dr. Bone has given, is recorded by Moreau de Jonnès. A soldier named Guyon, of Fort Royal, Martinique, had the filthy hardihood to wear for twenty-four hours, a shirt completely saturated with the sweat of a yellow fever patient suffering from the worst form of the disease; he was then inoculated in each arm with the matter discharged from suppurating sores; he slept for six hours and a half in a bed soiled by the excrement, and stained by the sweat of a patient who had just died in it; and consummated a variety of other similar brutalities, by drinking a couple of ounces of the black fluid taken from a dead man's stomach. This man or beast, however, did not take the fever.

All this may be true, and no doubt is true of the ordinary yellow fever of the West Indies and coast of Africa; and yet yellow fever may be contagious for all that—at certain times and in certain modified forms. There can scarcely be a doubt that in all the ordinary localities of the non-contagious yellow fever, a contagious epidemic yellow fever *does* sometimes

show itself, presenting, for the most part, the ordinary phenomena—only in an aggravated degree, and springing, in the first instance, apparently, from the ordinary sources. Whether, in such a case, we have to deal with an entirely different disease—*a nova pestis*, or merely with a modification or *degeneration* of the old,—is a question yet unsolved. We greatly desiderate a true discriminating history of *all* the maladies—endemic and epidemic—that have ravaged our colonies, and our fleets and armies, under the one name of yellow fever.

In the *treatment* of yellow fever, Dr. Bone is strongly averse to the practice of many of his predecessors. Dr. Rush, regarding all remittent fevers within the tropics as dependent on local inflammation or congestion, used the lancet unsparingly, and (as he thought) with the best effects. Jackson followed his plan, and with like success. Dr. Bone says, "my experience during many years does not confirm this notion. In 1820, in Tobago, I employed the bleeding practice: it failed totally, and since then I discourage the abstraction of blood." (p. 46.)

Even local depletion he disapproves of.

> "Leeching the temples, cupping the nape of the neck, &c., are not necessary measures, as may be understood from the observations already made. The cause of the headache is in the abdomen, and only secondarily or by sympathy, in the brain; it is therefore to be cured by acting principally on the bowels by purgatives, and on the skin by diaphoretic medicines; but leaves applied to the brow to excite perspiration are useful, and the hands and feet may be placed in hot water." (p. 47.)

Mercury was once so favorite a remedy in yellow fever, that Chisholm said of it—"Let it never be forgotten, that, at whatever period of the disease salivation is excited, whether the supposed signs of putrefaction have appeared or not, the accession of it is the certain signal of cessation of disease, and of returning health." This opinion has had many able supporters, even up to the present day; but Dr. Bone entirely disapproves of it. "Salivation," he says, "neither cures nor prevents yellow fever, but, on the contrary, by rendering the body more sensible to impressions from currents of cold air, *predisposes* to yellow fever." (p. 48.)

Dr. Bone lays great stress upon the use of salines, and saline aperients in this disease. Seidlitz powders, Rochelle, Cheltenham, and Epsom salts, cream of tartar, and such like, he chiefly recommends.

> "During the first twenty-four hours, the aperients and diluents are to be assiduously and skilfully urged until twelve or twenty stools are procured, or until *the stools become nearly the fluids drunk*; and usually from two to four ounces of the neutral aperient salts, and from one to two gallons of diluent liquors, are about the average quantities necessary to produce a solution of the disease." (p. 39.)

This is diluent, deobstruent, and solvent practice with a vengeance. It may possibly be not so injurious as the bloodletting and mercurial heroisms; but we should ourselves have greatly preferred the "one to two gallons of diluent liquors" *minus* the hydragogue accompaniments.

The Appendix contains some very judicious, and seemingly practicable suggestions, for "providing hospitals and barracks for troops in the West Indies."

ART. VIII.

Bidrag till den Tuberkulösa Lungsoten's Nosographi och Pathologi. Af. PEHR ERIK GELLERSTEDT, Med. Doctor, &c., och Sjukhus Läkare vid Kongl. Allm. Garnizons Sjukhuset i Stockholm.—*Stockholm*, 1844.

Contributions to the Nosography and Pathology of Tubercular Phthisis. By PETER ERIK GELLERSTEDT, M.D., and Physician to the Royal Garrison Hospital in Stockholm.—*Stockholm*, 1844. 8vo, pp. 127.

WE have recently had the pleasure of introducing to the notice of our readers the writings of a Swedish physician, who, though as yet comparatively a young man, has obtained considerable eminence as an acute and diligent observer in his own country. In the Clinical Reports of Dr. Huss, to which we allude, we had occasion to regret that we could not present to our readers the opinions of that author upon many important diseases, from the circumstance of his having treated of them in other of his annual volumes, to which unfortunately we had not access. The present work, in a manner, supplies a part of this deficiency, by laying before us the opinions and experience of one placed in the most favorable position to observe, and who is, if we may judge from the work before us, fully competent to the task. Our knowledge of phthisis, as it appears in various countries of Europe, has of late years been very much extended, but we have not seen a single recent work, even from the laborious press of Germany, that gave any account of this disease as it exists in the Scandinavian peninsula. Nor must we look upon the present essay as completely filling up the hiatus we complain of; for it pretends only to be the result of observations made in the Military Hospital at Stockholm, where soldiers alone are admitted. The general results of Dr. Gellerstedt's researches cannot therefore be of much avail in the statistical history of phthisis. But our author, at the very beginning, expressly assures us that it is not the general history of phthisis which he pretends to write, but merely to illustrate certain symptoms and features of the disease as it has occurred in his own practice.

Dr. Gellerstedt believes that phthisis in Sweden is upon the increase. The fact, if a fact, may perhaps be accounted for on the supposition that more luxurious habits are beginning to invade the stern regions of the north; but it is more probable that, as the means of diagnosis are now so much improved, the disease is only more readily detected. In the garrison at Stockholm, consisting of about 3500 men, the average mortality is about 80 deaths per annum, and of these 28 die from phthisis, while many more, affected with the same malady, are dismissed as incurable from the regiment, and return to die in their own homes. This gives a per centage upon the total deaths from all diseases of 35 per cent. in favour of phthisis.

The researches of our author during his five years of service in the hospital, have been chiefly directed to the pathology of the disease, and especially to the earliest commencement of the organic changes in the lungs. This, it is acknowledged by all, is the most interesting and important point in its whole history, nor can we say, in spite of the elabo-

rate researches of Louis, Fournet, and others, that the subject is in any way exhausted.

In a brief preface, our author gives us, as preliminary to the other matters, the result of his own researches on the intimate structure of the lungs. He differs from Reisseisen and others as to the mode of termination of the bronchi. That author believed that each ultimate division of a bronchus was terminated by a single cell; Dr. Gellerstedt, on the contrary, inclines to the opinion of Dr. Hodgkin, that the bronchi diminish till their diameter is equal to that of two air-cells, and then really terminate. For, on slicing dried portions of the pulmonary parenchyma, and placing them under the microscope, numerous air-cells may be seen opening directly into the parietes of the bronchial tube, which tube cannot be traced beyond that point. In a word, as our author vividly expresses it, these air-cells line the walls of the ultimate bronchial divisions (*tapetsera dess väggar*). After briefly analysing the rest of Reisseisen's well known descriptions, Dr. Gellerstedt states, that he has recently, for very fine microscopical injections, employed blood deprived of its fibrine:

"I inject this," says he, "through the pulmonary artery, till it pours out from the opening of the pulmonary veins; I then tie both vessels, and, inflating the lungs, place a ligature on the bronchi, to prevent the escape of the air. After this, the whole mass is sunk in a vessel containing dilute sulphuric acid, in which it is allowed to lie for one or two hours, until the colouring matter of the blood is entirely coagulated throughout the whole lung. It is then taken out and slowly dried, until it becomes sufficiently hard to allow of its being cut into very thin slices."

The lining membrane of the ultimate division of the bronchi scarcely, he thinks, can claim the appellation of a mucous membrane, as the muciparous glands are wholly wanting; nor is the fine villous coat, so characteristic of that tissue, any longer to be found.

From these short, but interesting, notices of the pulmonary parenchyma, our author passes to the more immediate subject of tubercular degeneration, and first discusses the interesting point of the nature of *miliary tubercle*. He admits with Laennec, that the gray semitransparent granulations are of tubercular origin, but he has not generally met with a yellowish softer point in the centre of these small bodies. Dr. Gellerstedt has, however, frequently found portions of the miliary tubercle to be of a different colour from the appearance it generally presents, but he considers this to result rather from *engaged portions of the parenchyma of the lungs*, than from any change operated in the granulations themselves. Andral and Lombard's opinion that these granulations are the result of a croup-like exudation into separate air-vesicles, he does not consider to be supported by microscopical examination, for the granulations are too large to be contained in single air-vesicles, and, besides, they are seen to contain numerous engaged fragments of the pulmonary parenchyma, which, were their theory correct, could not be the case.

Crude tubercle, properly so called, is distinguished from the gray granulations by its opacity and by its yellow colour. It has not, observes Dr. Gellerstedt, the well-rounded form that generally distinguishes the miliary variety. Our author describes also a round isolated tubercle of not very frequent occurrence, which is of a firm wax-like consistence, and often

attains, unaltered, the size of a hazel nut. It is surrounded with a firm cyst, and is generally found isolated in the lungs: it has been observed by Dr. Gellerstedt in 24 cases, in most of which the patients had died of tubercular phthisis.

Tubercular infiltration of the lungs is minutely described. Dr. Gellerstedt admits of two varieties of this alteration: the one he terms isolated tubercular infiltration (*insulär tuberkel infiltration*); the other is that general form, so well delineated by Rokitansky. We do not remember to have seen any better description of the former variety, and we therefore introduce it here from Dr. Gellerstedt's work:

"Insular tubercular infiltration occurs in masses, from a pea to a walnut in size, and is of a blueish-gray colour, with a smooth and almost fatty aspect when cut. The tissue of the lungs, when this occurs, has entirely lost its accustomed porous appearance, without assuming the character of hepatization, or that given by aggregated miliary tubercles. At the edge of these gray masses we observe many small yellowish-white points of a looser consistence, and which consist of softened tubercular matter. These points are sometimes so abundant that they surround the gray infiltrated masses as with a ring. It is rare that they are to be found within the mass itself."

Insular or isolated infiltration of the lung occurred in 17 cases out of 119, or in 14 per cent. of those who died of phthisis, and in 11 per cent. of those whose death was occasioned by other maladies.

Caverns or excavations in the lungs occurred, as might be expected, very frequently, but in only 4 instances out of 119 did they preponderate, or appear only in the *lower* lobes of the lungs. The larger bronchi are generally described as being abruptly cut off at their entrance into these excavations, but Dr. Gellerstedt states that a slightly swollen ring is usually formed around the orifice by the mucous membrane lining the air-passages. He has only once observed a *single cavern* in patients who died of phthisis, but he has met with single excavations seven times in those who had succumbed to other maladies. Dilatations of the bronchi were frequently observed, and in one instance, where the patient did not die of phthisis, so large a cavity had been formed in this way, that it abutted upon the pleura, which was adherent to the ribs, and threatened to form a communication with the external parietes. The bronchi, too, are frequently compressed in phthisis; Dr. Gellerstedt has traced a bronchus thus flattened, and forming a compact ligament of two lines in diameter, till it ended in a double-headed mass containing a calcareous concretion.

Ulcers in the larynx, &c. In speaking of ulcerations of the trachea, Dr. Gellerstedt observes, that the mucous membrane surrounding these ulcers is frequently covered with a white albuminous layer, similar to that described by Bretonneau under the name of diphtheritis. True tubercle or tubercular matter he has never met with in the *trachea.* These ulcerations appear to supervene at an advanced stage of the disease, for out of 45 cases in which they were observed, 42 were so far advanced, that caverns already existed in the lungs. The ulcerations in the larynx, so common in phthisis, are, however, believed by Dr. Gellerstedt to be truly of tubercular origin, while those of the trachea here described, he regards, with Louis, as of an aphthous nature, arising from the constant irritation of the cough.

In two instances the pericardium contained tubercular matter in the form of calcareous concretions. One of these was about the size of a walnut, and consisted of a cyst with thick walls, containing a substance in consistence and appearance exactly resembling whitewash.

The intestinal canal presented very frequent lesions, tubercular deposit having occurred there in 97 cases out of 119. It appeared in various forms: *a.* As *granulations;* in size and consistence resembling miliary tubercles, but differing from these in their colour, being whitish-yellow and opaque. They were not confined to the solitary or agminated glands, though they were rarely met with in the large intestines, their seat being almost always in the submucous cellular tissue of the ileum. *b.* As *ulcerations;* occurring in the usual form: but we cannot think our author justified in separating these two varieties, as it is most probable that tubercles always form where ulcerations are found, but have subsequently softened and have been carried away.

Tubercular ulceration of the *stomach* is of rare occurrence; Dr. Gellerstedt has observed this alteration not less than five times in 119 cases of death from phthisis. It remains, we think, to be proved that these ulcerations were really tubercular.

The mesenteric glands presented no other form of disease than tubercular infiltration, and were less frequently affected than the intestines, in the proportion of 44 to 97. Our author met with but five cases out of 46 in which the mesenteric and the bronchial glands were simultaneously tuberculous. He rarely observed the fatty degeneration of the liver so accurately described by Louis.

It was only in two instances out of all his dissections that Dr. Gellerstedt found tubercular matter in other parts of the body when it did not exist in the lungs. In both these cases its site was the bronchial glands, and in one a cyst full of tubercular matter existed in the pericardium.

It will be thus seen that the proportion of cases of tubercular degeneration of the intestinal canal is much greater in Dr. Gellerstedt's reports than in those of Louis or of Lombard. Our author accounts for this, by supposing that the intestinal canal is more frequently affected in Stockholm than in Geneva or Paris, yet the statistics of typhoid fever give a contrary result.

Tubercles of the lungs accompanied Bright's disease of the kidney, in the proportion of 7 per cent.

From these minute but interesting results of his post-mortem examinations, Dr. Gellerstedt next passes to the obscure subject of the pathology of phthisis, and attempts to lay before us a full history of the origin and development of tubercle. With the physiologists of the present day our author believes tubercle to commence like other tissues, healthy or diseased, as a cytoblastema, yet with Vogel, he believes that something more is wanting to produce tubercular matter, some vitiation of that vital influence that develops the cytoblastema into healthy tissue, but which, when weakened in any way, causes the same cytoblast to pass into pseudo-organization, such as tubercle or schirrus. The germination (if we may use the term) of a cytoblast, is the same for all the tissues of the body, but the cells which are developed as nuclei in the original vesicle, first show the different tissues to which they are to be adapted, as a chemical [?]

change takes place in their barietes, and under a vitiated influence they are altered sometimes to tubercle, which in its highest stage of development consists of cells, with more or less of granular matter lying between them. This hypothesis will, of course, be well known to many of our readers, and we have therefore merely noticed it as the doctrine which is adopted by the writer of this essay. Dr. Gellerstedt believes that tubercle may appear under different forms, either as miliary tubercle (the gray semitransparent granulation of authors), or as crude tubercle, nearly, if not quite opaque, and of a yellowish white colour. He does not, however, seem to infer that from either of these two forms we can trace the peculiar appearance termed tubercular infiltration. It will be known to most of our readers, that of late the opinions of the best pathologists have tended to regard this alteration as of inflammatory origin, as being, in fact, the result of a species of scrofulous pneumonia. Dr. Gellerstedt allows that this may possibly be the case, in so far as that pneumonic hepatization may in scrofulous subjects be converted into tubercular hepatization, which in due time may soften, and leave excavations in the lungs.

"But," he adds, "I have never observed such a process to give rise to granular tubercle, except in those cases where the tubercular disease has advanced rapidly with symptoms of pneumonia; and I have then invariably found the tubercular deposit to be of a yellowish white colour, granular, and indistinctly circumscribed, and bearing a strong resemblance to the hepatization and abscesses occurring in the lung after phlebitis."

We have already seen that our author is opposed to the doctrine of the inflammatory origin of tubercle, but he acknowledges that the formation of tubercle and the process of inflammation present many points in common; though, if we trace them carefully, we always find that their mode of development is totally distinct and dissimilar.

The signs of inflammatory action discovered by the microscope in the form of exudation-corpuscles, are easily, he thinks, explained by the fact that tubercles act as a foreign body, and irritate the surrounding parenchyma. The condition of the blood, too, in phthisis, has been much insisted on by the advocates of the inflammation theory; but the cause of the increase of fibrine in that fluid is undoubtedly, he believes, the presence of the tubercular masses, which are already sufficiently far advanced to irritate and inflame the surrounding parenchyma. In fine, Dr. Gellerstedt candidly acknowledges that the influence determining the formation of tubercle in the body has as yet eluded observation; and with other pathologists, he takes refuge in the doctrine of a depression of nervous or vital influence to explain its origin.

In every instance he has found tubercle to occur only as infiltrated into the parenchyma of the various organs wherein it occurred, nor has he ever seen it lying loose in the bronchi, as described by Dr. Carswell.

As to its mode of increase, tubercle has been generally supposed to grow by juxtaposition, and not as in organized bodies by intussusception. Dr. Gellerstedt considers tubercle to be of a low standard of organization, and therefore to be incapable of developing a union between itself and other tissues. The further progress of tubercle from the original cell is produced, he thinks, by the neighbouring blood-vessels, from whence a

plasma is thrown out, and this, partly from the influence of the neighbouring tubercular matter, and partly from the peculiar degenerated vitality of the neighbouring tissues, is converted into tubercle. Thus the growth of tubercle bears, he thinks, a close analogy with that of certain other organized products which are destitute of blood-vessels, such as the epithelium, the hair, nails, &c.

In conjunction with Dr. Malmster, who is also, we believe, the author of a recent Essay on Phthisis in Sweden, Dr. Gellerstedt has carried on a series of microscopical observations on the constituents of tubercle, and his conclusions coincide entirely with those of Vogel, in so far as that he never found tubercular matter to consist of anything but an indistinct granular mass, or else of an aggregate of cells; but he did not discover nuclei in the latter. The cells were about the size of $\frac{2}{3}$ of a blood-corpuscle, indistinctly rounded, with ragged edges; and the last seems to be their most constant characteristic. Their contents varied much: sometimes they were filled with a granular matter, at other times they contained many distinct points, and again they occasionally seemed to be entirely empty. Sometimes, too, the cells were numerous; on other occasions they were only thinly dispersed through the granular mass. This appearance of cells was most frequent while the tubercle was as yet gray and semitransparent, but the mass became more amorphous when it had assumed the yellowish-white, opaque aspect, and was then mixed with many small granules, and numerous crystals of cholesterine. Later, when the tubercle was softening, he found it to consist, under the microscope, of a semitransparent fluid, in which floated the above granules and crystals, with globules of fat and also of true pus, and the latter became more numerous as the softening of the tubercular mass advanced.

To the chemical history of tubercle our author has made no additions.

The possibility of the healing of tuberculous disease, he regards as an established fact. He states that tubercle may really be absorbed, but that more frequently it diminishes in size, and becomes surrounded with a cyst, which isolates it from the surrounding parenchyma; at other times it is changed into a calcareous mass.

Softening of tubercle. Laennec's idea that tubercle always softened from its centre was, no doubt, formed in accordance with his doctrine of its origin, but Dr. Gellerstedt has always observed them to soften from several points at once, as is also maintained by Dr. Hodgkin. From this last named author, too, he quotes the doctrine, that tubercle preserves its consistence by means of its own special vitality, but when deprived of this essential requisite it rapidly softens and falls away. To this opinion our author assents, as also to that promulgated by Reynaud, that caverns in the lungs may heal in two different ways: in one case, the parenchyma surrounding the cavern becomes indurated, and the bronchi appear as hard white ligaments traversing the indurated portion; in the other form, there is less positive induration of the tissue round the excavation, but it shrinks and draws together, forming a long, firm, white cicatrix. If the pleural surfaces have been previously united by adhesions, these are lengthened and become thinner by the shrinking of the lung, and may perhaps entirely disappear. (?) Dr. Gellerstedt, however, doubts whether all the

cicatrized-looking portions at the summit of the lung, are to be referred to the healing of tubercle, and in this doubt we are certain that many of our readers will coincide with ourselves.

The fibrous bands which traverse and surround the pulmonary parenchyma in phthisis, exhibit a similar tendency to contract, and thereby to compress the tissue of the lung, which no doubt, if such, will materially assist the healing of the excavations. The occurrence of these fibrous bands in the lungs is ascribed by Rokitansky to a species of chronic inflammatory action, and they are often observed in connexion with dilatation of the bronchi.

Dr. Gellerstedt fully corroborates the doctrine of Rokitansky and Corrigan, that the dilatation of the bronchi is secondary to, and occasioned by the condensation and contraction of the surrounding tissues. Laennec, it is well known, supported the opposite theory, viz. that the dilatation of the bronchi compressed and condensed the surrounding parenchyma, and Dr. Gellerstedt thinks that this view is not in all cases untenable, as he has occasionally found the bronchi dilated in the lower lobes of the lungs, without any consolidation of the neighbouring tissues.

We regret much to have devoted so short a space to this most interesting portion of Dr. Gellerstedt's essay, but we have throughout endeavoured to pass over, or very briefly notice, those theories already promulgated by other writers, and have confined ourselves as much as possible to the author's original observations. In this, to a certain extent, we feel we do injustice both to Dr. Gellerstedt and to our readers, for much of the value of this essay consists in the judicious arrangement of the opinions of other writers, both those of Sweden and of the other nations of Europe.

The second portion of our author's essay refers to the symptoms of phthisis observed during life; and of these (he remarks) only a few need to be particularly noticed, as they have been from the earliest time the chief objects of attention. Much, however, may still be accomplished in the pursuing of new modes of investigation, and by well-grounded statistical researches into the causes of the malady, and those influences that retard or accelerate its development.

The study of the expectoration our author looks upon as comparatively of small value in phthisis. The most characteristic feature it presents, is the irregular streaked sputum with small whitish granules interspersed. This, however, does not occur till the disease is considerably advanced; at an earlier period he does not think it possible to distinguish the sputa of phthisis from those of common catarrh or bronchitis. Later in the disease, the microscope affords us valuable aid, and is of great benefit in establishing the diagnosis in doubtful cases.

The following are the results of Dr. Gellerstedt's microscopical examination of the sputa: (*a*) Besides the well known presence of a grayish serous looking fluid, in which float numerous epithelial cells with ciliæ and exudation-corpuscles, pus, and mucous globules, none of which are peculiar to phthisis; we find also very minute granules, apparently the nuclei of tubercular cells, but varying greatly in amount. These nuclei are insoluble in ether and in alcohol, but are dissolved by alkalies, and probably consist of albumen. They do not vary either in form or appear-

ance. (*b*) Melanotic matter in the form of small granules, irregularly interspersed or aggregated, (but he has never seen melanotic granules inclosed in cells, as has been described by Bühlmann.) (*c*) Tubercular matter is now recognized by all writers (excepting Gruby) as being found in the expectoration of phthisis when far advanced. It resembles in character that taken from the lungs after death, excepting that the cells so constantly observed in the latter case, are rarely present, and the masses usually consist of aggregated granules, as described by Vogel. The peculiar spherical bodies with concentric rings, discovered by Gruby, and by him regarded as characteristic of phthisis, are, according to Dr. Malmsten, nothing more than *starch-cells* resulting from the diet of the Viennese hospitals, where Gruby's observations were carried on. The various crystals which occur in the sputa, are considered by Dr. Gellerstedt to be of no pathological importance. From all that has been written on this subject, we must conclude with our author, that the microscopical examination of the sputa can be useful as a means of diagnosis only after tubercle has begun to soften, and to be expectorated from the lungs.

Duration of the disease. The results of the tables here given, exhibits a much shorter duration of the malady than is allowed by Louis, or by Bayle, or Clark; but we must remember that Dr. Gellerstedt's observations were made upon the soldiers in the garrison, and that in many of these the disorder had perhaps silently advanced for some time before their entrance into the hospital. The average duration of phthisis in Dr. Gellerstedt's patients was not more than four months and eight days; and the mortality of six months gives 88 per cent. Our author observes that perhaps phthisis is really more rapid in Sweden and among the Swedish soldiers, than in France or England; but he tells us, on the other hand, that many of the men persevere in their duties to the last, and enter the hospital only to die. The length of previous service as a soldier did not appear to retard or hasten the progress of the malady; but the influence of season of the year is undoubted, as has been already ably illustrated by Sir James Clark and others. From want of space, we have abstained from giving any of the valuable tables interspersed in this essay, but the short one below we venture to insert as it exhibits considerable diversity from that of the eminent author just quoted; in reference to the seasons of the year when the mortality from phthisis is highest.

Sir J. Clark.	Dr. Gellerstedt.	Mortality of each month. Dr. Gellerstedt.
March	June	19
February	May	16
December	October	14
January	November	14
April	April	10
May	March	8
November	February	7
June	July	7
October	August	7
July	January	6
September	December	6
August	September	5

It will be seen from this table that the latter months of spring, and the autumnal season, are the most perilous in Sweden for phthisical patients; but we must ever remember that death does not always occur in those

months which have exercised the most injurious influence on the health of of the patient.

Treatment. Our author expresses himself very cautiously in respect to the *cure* of phthisis. He allows, however, that cases certainly do occur where every symptom of tubercular disease exists, but the patients nevertheless recover their original health. On the one side, we cannot as yet rely with positive certainty on the symptoms indicating phthisis in the living subject, much as the diagnosis has been improved by auscultation, percussion, and the microscope; and on the other, the appearances in the lungs after death may be fallacious, as to the previous existence and subsequent cure of tubercular disease. In many cases the external symptoms will cease for a time, contemporaneously with diminution of secretion from the caverns, and these last we have seen, Dr. Gellerstedt thinks may really heal, but there is ever danger to the patient, unless the tubercular diathesis be entirely removed. Have we, as yet, discovered the means of effecting this most desirable object?

In general Dr. Gellerstedt has found both lungs affected in those who have died of phthisis, and he has also remarked, that the instances where only one lung was the seat of disease, were precisely those where the progress of the malady was slowest, and where Nature exhibited the strongest tendency to effect a cure. Dr. Gellerstedt does not even deny the possibility of the complete reabsorption of the tubercle:

"I do not consider tubercular matter to be so totally dissimilar in character to those other abnormal tissues which form in the body, and which we know are frequently reabsorbed, as to exclude the possibility of tubercle being acted on in like manner. In spite, however, of the recent wonderful improvements in diagnosis, I do not consider that our means of ascertaining the existence of this disease are so perfect, as that no mistake can be committed regarding the real nature of the malady. Thus we may be easily led to regard as tubercular phthisis, a consolidation of the parenchyma and interstitial cellular substance of the lungs, the result of chronic inflammation, and the more so as this disease, like tubercle, has its seat in the upper lobes, and is often accompanied with dilatation of the bronchi, giving rise to sounds closely resembling those heard in true phthisis."

Causes of phthisis. The great mistake of most writers on this point has been, as Dr. Gellerstedt believes, the constant endeavour to find out some special and uniform influence to originate the malady. We allow that too many authors have fallen into this common error, but we still maintain, that, by the highest authorities, it is acknowledged that the most various and opposite causes may here produce the same effect. We shall briefly analyse our author's opinions on this important point:

a. Climate. That the climate of Sweden, from its position so near the polar regions, from its vast forests and numerous lakes, would be unfavorable to phthisical constitutions, will naturally be expected; but we were scarcely prepared to find so large a mortality from this disease among the troops in Stockholm. The highest average of deaths from this malady in our army, is that among the black regiments in the West Indies, viz. 14 per thousand, and in the Swedish army at Stockholm, we have an actual mortality from phthisis of 8 per thousand, and an average of 6 more per thousand who are yearly dismissed, at their own request, to die at their own homes. The provinces of Sweden which have, during the last five years, afforded the greatest number of cases of phthisis in the garrison of

Stockholm, are West and North Bothnia, Westmannaland, and Skåne, and the Dales (Dalarna). A carefully-constructed table of these is given by Dr. Gellerstedt, specifying the number of soldiers from each province, their age, &c., but he acknowledges that its value is only approximative.

b. Trades and professions. Our author's observations in regard to these are naturally limited, but he considers the life of a soldier, for many reasons, to predispose to phthisis. By an ingeniously constructed table, he attempts to show in one view the trades in which the men were employed previously to entering the service, as also the duration of their military life, and the period of death from tubercular disease.

c. Age. The results in respect of age completely coincide with the statements of Sir James Clark, and with the general opinion of medical writers, from Hippocrates to the present time. The highest mortality among the soldiers at Stockholm was between the ages of 20 and 40, being not less than 80 per cent., but of course we must remember that the majority of the military are in service during that period of their lives.

Having thus briefly recorded our author's opinions and experience in regard to the more general causes of phthisis, we will now glance at those which he considers of a more immediate character, and these he divides into—1, such as tend to excite and develop a cachexia already existing but still dormant in the system; and, 2, those which directly favour the outbreak of phthisis.

a. Hereditary predisposition is fully acknowledged by Dr. Gellerstedt, who, however, adds nothing new in this respect.

b. Constitution and temperament. Dr. Gellerstedt admits to the full extent the existence of the two varieties of habitus phthisicus described by Clark and others, and he further remarks, that among the soldiers of the garrison, a decided predisposition to phthisis was observed in the tallest men.

c. Mode of life. Many conditions of a soldier's life have probably an unfavorable influence on his health, and assist in the development of phthisis, but it is difficult to fix on any one circumstance from which these evil results can be said to arise. The barracks inhabited by the Swedish soldiers are in general lofty and well ventilated, excepting those of the 2d Royal Life Guards, but it is precisely in this regiment that the fewest cases of phthisis have occurred during the last five years. Our author very properly calls in question the opinion of Baudelocque, that contaminated air is the chief cause of tubercular disease, and shows that phthisis is very frequent in many provinces of Sweden where the inhabitants spend the whole day in the open air.

d. The food, clothing, and exercise of the Swedish soldiers leave, according to Dr. Gellerstedt, nothing to be desired.

e. Venereal excess, he observes, does not seem to have any great influence on the development of the malady, but combined with the abuse of alcoholic fluids, we often find that thereby the cachectic condition is produced, which ends in tubercular phthisis. Drunkenness does not seem to prevail to any very great extent in the Swedish army; in the course of five years, 310 cases of delirium tremens have occurred in the garrison of 3000 men, and of these so affected 11 afterwards died of phthisis.

f. Preceding maladies undoubtedly predispose a patient to phthisis, by deteriorating the constitution. Dr. Gellerstedt doubts the accuracy of

Vetter's assertion, that gout and hemorrhoids may give rise *directly* to phthisis, though these two maladies, with their congener dyspepsia, often reduce the constitution to that cachectic condition, which is eminently favorable to the development of tubercular disease. In this part of our author's essay an ingenious tabular view is given of the amount of previous disease of all kinds in 119 patients, who ultimately died of phthisis. From this it appears that 44 out of 119 had previous hemoptysis, and 42 had bronchitis, and that the two maladies above named frequently occurred in the same individual. Bronchitis is considered by our author to be a frequent cause of the rapid development of phthisis in those already labouring under the tubercular diathesis, and he believes that acute affections are at all times peculiarly perilous to such constitutions.

Hemoptysis he regards as a symptom, and not as a cause of the tubercular disease; he has never met with it among the military without its being accompanied with more or less distinct evidence of the existence of tubercle in the lungs. *Pneumonia* is not, according to our author, a direct excitant of phthisis, but with Rokitansky and others, he believes that the hepatized lung, instead of returning to its natural condition, occasionally becomes the seat of tubercular deposit, and, moreover, that repeated attacks of pneumonia will hasten the development of tubercular cachexia. *Pleurisy* is regarded by Dr. Gellerstedt in the same light; the effusion induces a depressed vitality of the system, by mechanically obstructing the respiration, and thus favours the deposition of tubercle in the lung, but not necessarily in that portion of the pulmonary tissue which has been subjected to pressure from the accumulated fluid. In the same manner he explains the effects of fevers and dyspepsia in producing phthisis, though he allows that the researches of Todd and others have greatly benefited the early history of the disease, by pointing out the influence of dyspepsia in producing a cachectic condition of the system. Is not the dyspepsia, that seems to produce phthisis, sometimes a consequence of the debility of the digestive organs arising from an already existing tubercular diathesis? We are somewhat surprised to find that our author entertains opinions totally opposed to those of Boudet and others, in regard to the influence of intermittent fever upon tubercular disease. So far from believing that ague gives any immunity from phthisis, our author expressly regards it, from its depressing influence on the organs of nutrition, as one of the most cognizable causes of this disease. Perhaps the effects of ague under the climate of Sweden may be different from its agency under the milder sky of Algeria, where M. Boudet's investigations were principally made. But we are already exceeding our limits, and, in conclusion, we can only find space to present our readers with the short *general* table of antecedent diseases given by Dr. Gellerstedt:

	In 119 Cases.	Per cent.	In 191 Cases.	Per cent.	In 310 Cases.	Per cent.
Bronchitis	42	35·29	37	19·37	79	25·48
Hemoptysis	44	36·97	84	43·97	128	41·29
Pneumonia	22	18·48	51	26·70	73	23·54
Pleurisy	17	14·28	28	14·65	45	14·51
Nervous fevers	10	8·40	12	6·02	22	7·09
Ague	27	23·52	49	25·65	76	24·51
Dyspeptic symptoms	10	8·40	32	6·02	22	7·09
Diarrhea and Dysentery	23	19 32	33	17·27	56	18·06

The figures in the first, third, and fifth columns denote the number of individuals who had suffered from these antecedent maladies, first of those 119 who died of phthisis, then among 191 persons affected with this disease, and lastly among these two classes conjointly, while the per centage of each affection is given in the second, fourth, and sixth columns.

We would gladly have extended our analysis of this interesting essay, but we trust that enough has been here detailed to satisfy our readers that the productions of the Swedish school are entitled to our respect and attention; and that, while our learned brethren of Scandanavia emulate the best British and French writers in attentive personal investigation, they are inferior to none in their acquaintance with the progress of medical science throughout the world.

Art. IX.

Observations on the Nosological Arrangement of the Bengal Medical Returns; with a few cursory Remarks on Medical Topography and Military Hygiene. By Fred. J. Mouat, m.d., Assistant Surgeon, Bengal Army, Professor of Materia Medica and Medical Jurisprudence in the Bengal Medical College.—*Calcutta,* 1845. 8vo, pp. 64.

Having been long deeply impressed with the advantages to be derived from the application to medicine of those strict rules of observation under which the exact sciences have made such rapid progress, we hail with pleasure the indications of an increasing attention on the part of the profession to that desirable object. The pamphlet now before us gives us good grounds to hope that the wide field of investigation, presented by our Indian possessions, is about to be cultivated with that zeal, and in that manner, which its extent and importance justly claim for it, and that a race is springing up who will turn to good account the magnificent opportunities they possess of studying the influence of climate upon, and the progress of disease among various races. An old author observes that "it is no shame not to know that which one has not had an opportunity of learning, but it is scandalous to profess knowledge and remain ignorant," and this remark may be justly extended to those who have opportunities and neglect them.

When the very able statistical Returns of the Army were presented to Parliament, Mr. Hume moved in the House of Commons for the production of similar returns from the army of the Hon. East India Company, which were ordered, but it was found on investigation, that no documents existed which were calculated to furnish materials for constructing comprehensive statistical tables. To remedy this for the future, and remove the stigma which cannot but attach to them for having so long neglected the opportunity of collecting materials for the advancement of medical science, the Medical Board in Bengal issued a new form of returns which we must say is the most artificial and *unpractical* (if we may coin a word) we have ever seen. The object of the present pamphlet is to point out some of the imperfections which attach to them.

In the statistical reports which have been already published in this country, three different nosological arrangements have been adopted; the navy having followed that of Cullen, while the army and the registrar-general framed systems for themselves. We presume that Cullen's classification was followed by the navy authorities, simply because it was at the time of its adoption the best in common use: every one knows that it is a very defective one. Diseases are brought together which have no affinity to each other, either as regards their seat, or the causes likely to produce them. For example we find ophthalmia in the same class with lumbago, phthisis with hæmaturia, syphilis with icterus, aneurism with hernia humoralis, luxatio with hernia and prolapsus ani, &c.

In the military statistical reports, the arrangement of Cullen, which was used in the returns, was abandoned, and one of a more practical nature adopted. If we may judge by the benefits derived from the adoption of the suggestions founded on the results of these reports, the classification appears well suited to the investigation of the diseases of bodies of men at the period of life during which soldiers usually serve.* The statistical nosology followed in the reports of the registrar-general, is well adapted for the returns of a general population, but there are many peculiarities in the condition of the soldier which would lead us in any inquiry into the health of this class to prefer that employed in the military reports.

The classification adopted in the new forms issued by the Medical Board in Bengal, appears to be that of Dr. Mason Good. The diseases are divided into seven classes, viz. those of the digestive, respiratory, sanguineous, nervous, sexual, and excernent functions, and those from external violence. We will not stop to inquire with Dr. Mouat, what is meant by sanguineous *function,* digestive *function,* &c., but briefly notice a few of the objections to the classification.

The only disease under the head of "the respiratory function" is asthma. "This is an excellent illustration," says Dr. Mouat, "of the difficulty, if not impossibility, in the present state of our knowledge, of classifying diseases by any physiological arrangement such as that attempted in this table. It has a *primâ facie* appearance of philosophic accuracy, which on closer investigation will not stand the test of rigid analysis, and is not found to answer any beneficial purpose." After adverting to the various theories regarding the nature of asthma, he adds, "no conjectures or theoretical speculations are hazarded in placing it under the head of diseases of the respiratory organs. For the same reason, catarrh, pneumonia, pleuritis, bronchitis, and phthisis, ought to have been placed in the same order, as affecting this system,"—an opinion in which every practical man will probably concur.

We cannot afford space to follow our author through the remarks on the various classes, but we think it will be admitted that, for practical purposes, the classification must be very deficient which includes in the same category, fevers, ophthalmia, rheumatism, phthisis, syphilis, and ulcers; which associates cataract with delirium tremens, and psora with

* For the arrangement adopted and the practical advantages which have arisen out of these Reports, see British and Foreign Medical Review for April, 1844, p. 313.

dropsies ; and which exhibits gonorrhea as the only disease of the "sexual function!" We must also join in protesting against the use of the term "alii morbi ;" whatever classification may be adopted, medical officers should be imperatively required to return every case of disease under its proper head. The lumping of several diseases in this way, diminishes materially the value of the returns, and is at best but a poor economy of stationery.

It is most justly remarked by our author, that

> "It cannot fail to be a source of regret that the system of returns required from the Indian army, should not be identical with that adopted in Her Majesty's service : the value of such uniformity in extending our knowledge of the medical economy of troops serving in every quarter of the extended empire subject to Great Britain ; of the closer approximation to truth that would result from calculations spread over so wide a field, and deduced from numbers that would constantly tend to diminish the amount of error arising from local and accidental causes in smaller bodies of men, is almost self-evident to any one who has studied attentively the important subject of statistics."

The advantages to be derived from this uniformity in the returns have been brought under the notice of the authorities since Dr. Mouat's paper was written, and we trust will be duly considered in the arrangements which are to be made in India for collecting military statistics.

Dr. Mouat has appended to his paper, a brief analysis of the various works that have been published on the medical topography of some of the stations in the Bengal Presidency, which may prove useful to those desirous of ascertaining what information is extant on this subject. In noticing one of these, he quaintly observes "much valuable matter and careful registers of the hygiène of prisoners may possibly exist in the records of Government, but as they have never been published or made known, the information is of no practical use."

We are at all times happy to have it in our power to notice the labours of our brethren in India, because we consider them entitled to much more credit for their exertions under all the disadvantages and discouragements to which they are exposed, than we are who enjoy the bracing air of a temperate clime. We believe there is no want of zeal on the part of the officers of the Indian medical department, but that they are anxious to promote the cause of science, and contribute their quota to the advancement of their profession ; and we trust the authorities will by liberal arrangements soon enable them to produce a series of statistical returns worthy of being ranked with the best works of this nature. Dr. Mouat, who is but young in the profession, is entitled to great praise for the able and zealous manner in which he has endeavoured to direct the attention of the ruling powers to the important subject treated of in his pamphlet.

Art. X.

Sicheres Heilverfahren bei dem Schnell gefährlichen Lufteintritt in die Venen und dessen gerichtsärztliche Wichtigkeit. Von Dr. Ch. Jos. Edl. v. Wattmann, k. k. n. ö. Regierungsrathe, Leibchirurg, o. ö. Professor der praktischen Chirurgie und der ersten chirurgischen Klinik, Vorsteher der Operations-Institutes an der k. k. Universität in Wien, &c.—*Wien*, 1843.

A Certain Remedy in rapidly dangerous Entrance of Air into the Veins, and on the Medico-legal Importance of that Event. By Dr. Ch. Jos. von Wattmann, Councillor to the Imperial Government and Surgeon to the Emperor of Austria, Professor of Practical and Clinical Surgery, and Director of the Institute for Operations in the Imperial University of Vienna, &c.—*Vienna*, 1843. 8vo, pp. 188.

The subject both of the artificial introduction and of the spontaneous, or rather accidental, entrance of air into the veins has been several times considered in this Journal, especially in the Number for October 1838, pp. 455 et seq. Dr. Wattmann's publication again brings this very interesting question before us, so far as regards the spontaneous or accidental admission of air into the veins of man. It might indeed have been expected that little room for discussion would remain after the numerous experiments, and the nearly as numerous disputations, both oral and written, which the subject has called forth; and it certainly is to be regretted that a phenomenon of such unquestionable importance, and of which so many alleged examples have occurred in the practice of surgery, should still be a matter of doubt and dispute.* As an example of how unsettled opinions are respecting it, we may mention that in four systems of surgery, now in course of publication in Paris, three different doctrines are advanced; M. Nelaton admits, M. Ph. Boyer denies, that the spontaneous entrance of air into the human veins has ever proved fatal; while MM. Bérard and Denonvilliers take a middle course between these extremes, and think "that the solution of the problem remains doubtful, and is not decided either affirmatively or negatively." In this last opinion M. Vidal de Cassis coincides, in the second edition of his work on Surgery, which has just issued from the press. We must, however, observe that M. Vidal, the most recent of the writers just mentioned, has scarcely devoted sufficient attention to the subject. He states that MM. Bérard and Denonvilliers have collected almost all the recorded cases purporting to be examples of spontaneous entrance of air into the human veins during an operation: the fact being that they have not collected one fourth of them. He thinks it a remarkable and happy circumstance, that no new cases of the accident have been heard of since the publication of M. Bouillaud's Report, and the academic discussion thereon (examined in this Journal, Oct. 1838), and thence insinuates an argument against the reality of the event. A glance at the table of cases we have drawn up will show how much M. Vidal is mistaken in this respect. He states that the influence of exhaustion from hemorrhage in accelerating the fatal consequences of air admitted into the veins of dogs has been established by M. Gerdy particularly. M. Gerdy has,

* Since the above lines were written, we have seen that one of the questions to be submitted to the Medical Section of the Scientific Congress of France, at its meeting at Marseilles, 1st September, 1846, is as follows: "Fixer autant que possible nos idées sur l'influence de l'introduction de l'air dans les veines pendant les opérations."

on the contrary, endeavoured to show that the alleged effect of exhaustion is at the least doubtful. M. Vidal further implies that surgeons have invented spontaneous entrance of air into the veins, as an excuse for the sudden death of their patients during the performance of operations, inasmuch as no one, he says, has ever spoken of its occurring except consequent on wounds inflicted by a surgeon. In this also M. Vidal is utterly mistaken; the presence of air in the veins has been observed after decapitation, and also in some cases of suicide, and death has in some of these latter cases been attributed to its influence; again Sir C. Bell relates (Practical Essays, 1841, p. 11), that Baron Larrey, on looking over his Sketches of the Wounded at the Battle of Waterloo, "fixed with interest on the case of a young man who had been wounded in the lower part of the neck. Well, I know, said this excellent surgeon, how that man must have died. I have seen so many wounded during my campaigns, and die from air drawn into the veins." We fully admit that there has been a great deal of exaggeration respecting the entrance of air into the veins; like many other things, it had a run not merely with the profession, but even became for a time, we might almost say, a fashionable source of apprehension out of the profession. The wife of an eminent orator and leading member of the French Chamber of Deputies was bled at the arm, and having been enjoined to observe the strictest quietude, was left alone in her bedroom: after a short time hemorrhage came on, and, in the absence of timely help, the lady lost a quantity of blood sufficient in her condition to prove fatal. The catastrophe was, however, attributed to entrance of air into the veins, and this explanation attaining due publicity in the newspapers of the day, produced such an impression, that for some time, in certain circles, few physicians cared to propose venesection, and still fewer patients to submit to the operation. But though we agree with Blandin, Velpeau, and many others, that sudden death during operations has been erroneously attributed in several cases to entrance of air into the veins, we shall examine whether there are not other recorded cases which establish both the reality and fatality of the phenomenon. It may seem, from the title of Dr. Wattmann's book, that we are about to travel beyond the limits of his inquiries, but such is not the fact, for though he purports only to consider how the effects of the admission of air into the veins is to be remedied, and the medico-legal import of such an occurrence, he, in point of fact, enters into a consideration of the whole subject as a question of physiological surgery.

As we have to deal solely with *spontaneous* entrance of air into the veins, we shall refer but little, and that only incidentally, to the experiments on injecting air into the venous system. But we may say thus much, that many of the results and theories of modern experimenters were anticipated by some two hundred years. Not only did Woepfer, R. J. Camerarius, Redi, Bohn, Ant. Heydin, Brunner, Harderus, Valisneri, and Sproegelius kill animals by injecting air into their veins, but Camerarius, Brunner, and Valisneri ascertained that a certain quantity of air might be thus injected with impunity. Valisneri also discovered that the effects of air thrown into the veins varied in different animals, as dogs were more easily killed by it than sheep. The diversity of symptoms produced, did not escape notice. Brunner observed opisthotonos, Sproegelius slight convulsions, while Camerarius never witnessed either rigidity or spasms of the muscles, but, on the contrary, found them relaxed. Camerarius and

Harderus ascertained by dissection the presence of air in both the arteries and the veins, and Sproegelius thought the blood was always preternaturally liquid. Of the theories advanced in late years to explain how air in the organs of circulation causes death, three at least were anticipated. Brunner, Camerarius, Harderus, and Sproegelius attributed death to suspension of the circulation, from the heart being so distended that its contraction is prevented, and Harderus further held the opinion, that the fibres of the heart were weakened or paralysed by extreme distension, just as is the case with the urinary bladder in certain cases of retention of urine. Morgagni too agreed that death resulted from disturbance of the functions of the heart. Finally, Bohn conceived that air proved fatal by acting as a powerful and rapid poison. (Morgagni De Sed. et Caus. Morb. Epist. v. sect. 21 et seq.)

The discovery of the spontaneous entrance of air into a wounded vein, is comparatively, indeed absolutely, of recent origin. It is true Mery observed that if the abdominal cava of a living animal is punctured, the vein becomes filled with air as it empties itself of blood, but he thought the air was contained in the radicle veins, and flowed from them towards the cava, and thence to the heart. (Mém. de l'Acad. des Sc. 1707, p. 167.) Littré not long after found air in the blood-vessels after death from hemorrhage. (Ibid. 1714, p. 330.) Redi and Caldesi had seen air circulating with the blood in some cold-blooded animals, as vipers and tortoises, and thought it existed naturally in their blood-vessels, but Haller pointed out that air was not present, unless some vessel had been opened. Nysten remarks that he several times saw the veins and right auricle of persons who had been decapitated distended with air (Recherches de Physiolog. et de Chim. Patholog. 1811, p. 5), but failed like his predecessors to surmise its real origin. Verrier, a veterinary surgeon, seems to have been the first who, in 1806, recognized the spontaneous entrance of air into the veins of an animal. Magendie subsequently (Journal de Physiolog. 1821) inferred the occurrence of the event in man from M. Beauchene's case, and sought to establish it by experiments on animals; but as he allowed the air to enter through a tube passed into a vein, its admission in those experiments cannot be termed spontaneous. Subsequent to Magendie's first researches, several cases were observed in which air was supposed to have accidentally entered the human veins, but those cases did not attract very much attention at the time, and their interpretation was and still is disputed. M. Amussat, we believe, was the first who actually demonstrated the spontaneous passage of air into an open vein. His first communication on the subject is a short and incidental, but very explicit, passage in his memoir on traumatic hemorrhage, read to the Academy of Medicine in 1835, and published in their Memoirs, t. v, pp. 68 et seq. For his subsequent researches we refer to the Number of this Journal already mentioned, and to his 'Recherches sur l'Introduction accidentelle de l'Air dans les Veines,' &c.

We would now willingly proceed to examine the alleged cases of this accident in man, but as their import must be tested by comparing how far the circumstances attending them agree with the causes which produce entrance of air into the veins, and with the phenomena observed in animals that have undoubtedly died from its effects, we must first advert to those causes and phenomena, the more especially as considerable difference of opinion and statement exist respecting both.

The experiments of Barry, modified and confirmed by those of M. Poiseuille, fully establish the influence of atmospheric pressure on the venous circulation. There is no question that during inspiration, the flow of venous blood towards the thorax is facilitated in the great venous trunks in the vicinity of that cavity, while, during expiration, a partial reflux occurs in the same vessels. It is further generally assumed, from the experiments of Poiseuille, that the suction thus exerted on the contents of the veins extends to but a very short distance from the superior aperture of the thorax, because the veins being flexible tubes,—with very delicate yielding parietes, incapable of contracting in the direction of their length,—communicating with a cavity which tends to exhaust them by suction,—and exposed to atmospheric pressure external to that cavity, the suction cannot act beyond certain limits. For just as when we attempt to empty with a syringe a flexible tube full of liquid, and fixed at both ends, a small quantity of liquid only enters the syringe, because atmospheric pressure forces the sides of the emptied portion of the tube into contact, so do the veins when similarly circumstanced collapse. The diastole of the right ventricle of the heart is admitted by some to exert an influence on the venous circulation, quite similar in kind to, but less in amount than that of the expansion of the thorax during inspiration. MM. Gerdy, Velpeau, and others, indeed deny this influence of the heart, because opening one side of the thorax has been found to immediately arrest the entrance of air into an open vein, but we think that its reality is established beyond a doubt. In M. Bouillaud's Report, already referred to, it is expressly stated that the flux and reflux of blood was sometimes observed to be synchronous with the action of the heart, and in some experiments the characteristic sound of air entering the veins coincided for a time with inspiration, but then became more rapid, and was obviously isochronous with the action of the heart. M. Barthélemy independently made similar observations, (Bull. de l'Acad. t. i, p. 904, and t. ii, p. 368); and Dr. Wattmann, in one case in man, distinctly observed the venous pulse in the internal jugular vein coincide with the action of the heart. (p. 114.) But whatever may be the influence exerted by the action of the right cavities of the heart, the aspiration of the venous blood does not extend beyond certain limits, and M. Poiseuille having found that the external jugular vein of a dog when laid bare collapsed to within 4 centimeters (1½ inch) of the thorax during inspiration, this was thought to give an approximate measurement of the sphere of the phenomenon. Wherever then this suction, manifested by the venous pulse, exists, it is admitted that air may and frequently does enter a vein when wounded; and as the obstacle to the entrance of air into a vein beyond the limits of the venous pulse, is the obliteration of the canal of the vein by atmospheric pressure, the sphere within which air can enter a vein may obviously be extended if the collapse of the vein is prevented. As regards a considerable extent of the internal jugular vein, the whole length of the subclavian vein and the upper half of the axillary vein, M. Bérard has shown (Archiv. gen. t. xxiii, pp. 169-71) that they are united to the adjacent bones and muscles by fibrous attachments, which prevent their collapsing under atmospheric pressure, and consequently they are liable to admit air when opened. Again, the committee appointed to report on M. Amussat's experiments, found that air entered the axillary vein beyond the limits of the venous pulse when

the parietes of the vein were kept tense and the wound of the vein was kept open, and it is admitted that the limits of aspiration may be extended if the collapse of a vein is prevented by any cause,—such as disease of its coats,—adhesion to a diseased structure,—or if traction is exerted on it during an operation, as when, for example, a tumour in which a vein is involved is put on the stretch to facilitate its dissection. How far the limits of aspiration may be extended by those or similar causes, has not been precisely determined. MM. Velpeau, Gerdy, Blandin, and others, deny that it can be propagated very far, or to such veins as the external jugular, the mammary, or the subscapular; while M. Amussat and many others maintain that it may extend to the branches of the internal jugular, subclavian, and axillary veins. This is a point of great importance; for, according to our conclusion respecting it, many of the alleged cases of entrance of air into the veins must be absolutely rejected on the one hand, or admitted to be at least possible on the other.

It is obvious that, unless something counteracts the effects of atmospheric pressure, superficial veins, such as the external jugular, must collapse when suction is exerted on them, and that they cannot consequently either exhibit the venous pulse, or admit the aspiration of air through them, unless atmospheric pressure is obviated. But M. Barthélemy (loc. cit.) has several times observed distinct undulations, isochronous with the action of the heart, extend to a considerable height in the external jugular vein, which filled from below upwards, and collapsed from above downwards, and he has also seen air enter the vein when venesection was performed on it in those cases. M. Pellis also distinctly saw a flux and reflux of blood in the external jugular vein of the human subject, in a case referred to in the table inserted further on. But M. Barthélemy farther found that air may enter the external jugular vein when no such undulations exist, and at a point higher up than they ever extend to. Thus in one horse, air entered the external jugular vein, opened above its lower third as for ordinary venesection, three times in 2½ minutes, and produced the usual symptoms, and in a similar experiment on a second horse, air entered freely without any blood flowing externally; air likewise readily entered a canula, (not as in Magendie's experiments, and those mentioned in Bouillaud's Report passed down a vein to the region of the venous pulse, but) simply inserted into an opening in the upper third of the jugular vein; and finally, M. Barthélemy found air, without the aid of a canula, enter the external jugular vein immediately below the facial vein in sufficient quantity to cause death. M. Boulley and several others have also seen air enter the external jugular vein of the horse during venesection, but the foregoing facts sufficiently demonstrate that air may enter a vein far beyond the limits assigned by M. Velpeau, or even by M. Amussat, and that its admission into the external jugular vein, even at its upper extremity, is not constantly prevented by atmospheric pressure. Sir Charles Bell has, we think, offered the true explanation of this fact. During inspiration, the platysma myoides, the sterno-cleido mastoid, and the anterior portion of the trapezoid muscles, are, he says, "lifted off the veins of the neck, and, by removing the atmospheric pressure from them, allow them to dilate" (Op. cit. p. 17); and the same effect is produced by exertion, by raising the shoulder, and by certain motions of the head and chest. Sir C. Bell, it is true, seems to confine this explanation to the

internal jugular and subclavian veins, but it also applies to the other veins of the neck, to the external jugular, for example, which lies beneath the platysma myoides muscle. The action of the muscles of the neck then, during a sigh, a deep inspiration, a constrained posture, or a sudden motion of the head or shoulder, may render the entrance of air into the external jugular vein possible, and it will be seen that in one case in man (that recorded by M. Pellis), the alleged or, as we think, actual admission of air into this vein occurred at the moment the patient threw his head back and inspired deeply. This consideration also explains the difficulty with which air entered the veins in the experiments recorded in M. Bouillaud's Report; for in those experiments the veins were laid bare to a considerable extent, and the obstacle afforded by the muscles to the action of atmospheric pressure was consequently removed. A circumstance dwelt on by Sir Charles Bell must not be overlooked. We shall quote his words.

"When water flows through a tube, the tube being gradually larger at its further extremity, and a lesser tube inserted into it, water will not flow from the larger tube into the smaller, but from the smaller into the larger. This corresponds with the course of the blood in the veins; for the lesser veins are inserted into a series of trunks gradually enlarging in their diameters, till they reach the heart. In these circumstances, a hole in the side of the tube will not discharge water, but will admit air." (Op. cit. p. 15.)

This hydraulic law will not of course apply to the veins, as they are not rigid tubes, unless they are prevented from collapsing under atmospheric pressure by some of the influences already adverted to. Another circumstance which Dr. Wattmann particularly notices may greatly favour the entrance of air into veins which lie above the horizontal level of the heart; the veins so situated are seldom completely full, they rarely convey as much blood as they are capable of containing, and when a vein so circumstanced is opened, and its parietes are at the same time prevented from collapsing, air must readily enter the vessel only partly filled with blood. This is the reason why the vertical posture favours the entrance of air into the veins, and the influence of previous hemorrhage in favouring the phenomenon, or at least in hastening death, may probably, partly at least, arise from the admission of air being facilitated by the diminution of the quantity of blood in the venous system.—The conditions necessary for the occurrence of the admission of air into small veins lying beyond the ordinary limits of aspiration may not frequently exist, but this does not invalidate their reality. The experiments of M. Barthélemy, and the observations of MM. Verrier, Boulley, Gerard, and Leblanc (referred to in the table we have constructed), establish, we think, the fact of the spontaneous entrance of air into the external jugular vein; that fact, we also think, is explained by the considerations just adverted to; and the same explanation applies to other small veins of the neck.

The signs and *symptoms* of spontaneous entrance of air into the veins as observed in animals, are generally as follows. A noise, variable in kind and intensity, is heard at the orifice of the vein, from which either immediately or after some time frothy blood sometimes issues, and a bruit de souffle with or without gargouillement is heard, on auscultation, in the right cavities of the heart. There is agitation, with indications of anxiety, the breathing becomes accelerated and laboured, and the pulse frequent. From this condition the animal often recovers, but if the symptoms

become aggravated, the respiration and pulse get weak, the animal trembles, totters, and falls, the urine and fæces are passed involuntarily, the limbs are stiffened, or convulsed, or affected with tetanic spasms, and the breathing and pulse cease to be perceptible. The animal may still gradually revive from a state of apparent death, or death may occur within a period varying from perhaps a minute to some hours. Of those symptoms some are constant, viz. the acceleration and embarrassment of breathing and frequency of the pulse, and also, in aggravated cases, suspension of respiration and of the action of the heart, with insensibility. But almost all, if not all, the other symptoms are liable to variations which it is necessary to notice.

The noise caused by air entering a vein is compared in dogs to the sound made by those animals in lapping, and in horses to a gurgle (*glou-glou, gargouillement*). But this sound may be absent, thus, in the 7th experiment contained in M. Bouillaud's Report, no sound was at first heard after the subclavian vein had been opened, but air had nevertheless entered the vein as appeared from the embarrassed and accelerated respiration, and also from the more positive evidence of bubbles of air mixed with blood issuing from the vein. Again, in the 5th experiment, a "*lapement*" was heard at first, but after a time ceased, though large bubbles of air continued to enter the wound, and in the 12th experiment, on the contrary, the air entered noiselessly at first, but afterwards with its appropriate sound. The sound seems to be most commonly and most distinctly heard during inspiration, but in the 1st and 2d experiments of the commission, so often referred to, the entrance and exit of air, at first synchronous with inspiration and expiration, subsequently corresponded to the pulsations of the heart; again, though the sound is usually intermittent, it may be continuous, at least nothing is said of its intermitting in the 8th, 9th, and 10th experiments, and we may directly infer that it was continuous in the 11th, as we are told that it was louder during inspiration. In experiments 1 and 2, no urine or fæces were evacuated. In experiments 1, 2, 6, 11, 32, and 33, there were no convulsions. Sometimes the animals uttered cries, sometimes they did not. The rapidity of death in the fatal cases, also varied greatly. The most rapid death occurred in three minutes, in the 11th experiment, in which the animal, a dog, was held in the vertical posture. It is to be observed that none of the experiments of the commission on animals in the erect position were fairly conducted, except the foregoing one; in all the others, the dogs were placed in the horizontal posture once or oftener during the course of the experiment, and in some the orifice of the vein was also closed. Thus, in the 14th experiment, apparent death supervened, *almost immediately* when the animal was placed upright, but on replacing it horizontally and closing the orifice in the vein, it revived until again placed vertically. In many instances, too, the access of air was interrupted by the formation of clots on the opening in the vein, and obstruction from this cause, occasioned by the greater coagulability of the blood of dogs, seems to be one reason why dogs often resist the action of air better than men. But if we go beyond the experiments of the commission, we shall find that death may occur in the dog as rapidly as in any of the alleged cases in man; for M. Segallas states that in one experiment, which he performed on the jugular vein of a dog, death was almost instantaneous. (Bull. de l'Acad. t. ii, p. 427.) The period within which death occurs

varies moreover, there is some reason to believe, in different animals, Valisneri states that dogs die more rapidly than sheep, and the experiments in M. Bouillaud's Report indicate that death occurs in horses not sooner, but in a much less variable period than in dogs. The erect position undoubtedly hastens death, the rapidity with which the air enters, has of course an immense influence, as have also, though in a much less degree, the conformation and state of health of the individual, for M. Barthélemy found that horses with a small thorax or any affection of the lungs are easily killed by injecting air into the veins. (Bull. de l'Acad., t. ii, pp. 370-5.)

As regards the post-mortem appearances after spontaneous entrance of air into the veins, M. Bouillaud states that the right cavities of the heart were, in the experiments on which he reported, *constantly* distended with air; air was found in the left cavities of the heart of one dog only. In horses, on the contrary, air was found not only in the right heart, but also almost constantly in the left cavities of the heart, in the aorta, and other arteries, and in the vessels of the brain; a difference which M. Bouillaud thinks probably arises from the ultimate ramifications of the pulmonary vessels being larger in horses than in dogs, and consequently more readily permitting the frothy blood to pass to the left side of the heart. In three dogs killed, after 4, 4½, and 8 days respectively, air was found in the right cavities of the heart and in the arteries of the second animal, and in the arteries of the third animal, but no air was any where discovered in the first animal, whose heart, especially the right cavities, was remarkably flaccid.

Even if this account accurately applied to every case, it follows that the post-mortem appearances present some variety. The presence of air in the left side of the heart and in the arteries of the horse, evidently does not arise from those animals living longer after admission of air into the vascular system than dogs do, for it was found in those situations in the 35th, 36th, 31st, and 37th experiments, in which the horses respectively survived but 12½, 13, 14, and 16 minutes, which was not the case with dogs, that lived upwards of an hour; when, however, dogs survive a considerable time we have already seen that the air makes its way to the arterial system. But the appearances after death are by no means constantly such as are above described, and it is not unimportant to notice some of the occasional discrepancies. M. Bouillaud, we have seen, describes the presence of air in the right cavities of the heart as a *constant* phenomenon when death occurs soon after entrance of air into the veins, and M. Amussat further maintains that this air is never free, but is always mixed into a froth with the blood, which has assumed a bright arterial colour; this, it is said, is so constantly the case, that we may thereby determine whether air entered the vascular system during life or after death, in the former case the appearances are those just mentioned, but if the air is free and unmixed with blood, it gained admission after death. This distinction is said to be important in a medico-legal point of view, and also in determining the real cause of death in some of the alleged fatal cases of entrance of air into the veins during an operation. (Bull. de l'Acad. t. ii, p. 277.) Now in the first place, the heart does not constantly contain air after death from its admission into the veins. M. Poiseuille in some instances, found the right cavities of the heart contain blood only, without a particle of air. (Gaz. Méd. de Paris, 1837, p. 728, note.) Neither is the air, when present, always mixed with blood; in the 7th experiment

mentioned in M. Bouillaud's Report, "*pure*" air and liquid blood were found unmixed in the right auricle, and in the 6th experiment there was free air unmixed with blood in both the right cavities of the heart. Again, when air and blood are mixed, they do not always occur as froth, in some instances liquid blood preponderates. (Exp. 15.) In others (Exp. 16) the blood forms a coagulum with bubbles of air disseminated through it, a condition which we shall find existed in one case in the human subject.

Before we compare any of the recorded cases in the human subject with the symptoms and appearances observed in animals, it is convenient to give a tabular view of all the alleged cases of the kind which we have been able to collect; and we have added to this table several cases in which air has entered the external jugular vein of the horse accidentally during venesection. In order to economise space, we have occasionally thrown matter under the head of observations which should rather be found in another column. Some of the cases which we have inserted should, perhaps, have been omitted. Thus the three first cases are certainly not examples of death from air entering the veins: in Klein's case there is nothing to justify the presumption that death arose from this cause; as to the second case, Dr. Busse (Gaz. Méd. de Paris, 1838, p. 114) informs us that at the time of its occurrence, it was mooted as an hypothesis whether death might not have been the result of air entering the veins, but that dissection showed this conjecture was unfounded. Mr. Barlow's case is not recorded by that gentleman as an example of the accident in question, though Velpeau refers to it as if it was. The three next cases are quite unauthentic. As regards the first of them (4th in the table), Dr. Lodge wrote a very improbable and vague account of it from Paris, to Dr. Warren in America; no one knew anything of it in Paris, its authenticity has been often impugned, and never, so far as we know, vindicated by Dr. Lodge: indeed it seems scarcely possible to credit the alleged circumstances of the case. Dupuytren, we are told, when about to divide an enlarged saphena vein, expressed some apprehension lest air might enter the vessel, he nevertheless divided it, the peculiar sound of the admission of air was heard, and the patient fell dead; now besides that, M. Poiseuille has shown that the pressure of the blood in the femoral vein is greater than that of the atmosphere, whence entrance of air into a vein of the lower extremity is impossible, it is incredible that Dupuytren, if apprehensive that air might enter the vessel, should have failed to compress it above the point at which he divided it. The fifth case is also valueless; all we know of it is that Dr. Warren, in Hays's 'American Cyclopædia,' &c., says that Dr. Stevens "witnessed" death from air entering a vein during an operation, and again in his surgical observations on tumours, states that such an event occurred "within Dr. Stevens's knowledge." We know not from what source the report of the case said to have occurred in Sir A. Cooper's practice originally emanated, nor have we anywhere found it mentioned, save as a rumour, respecting which no particulars are given, and for which no authority is cited; it is thus M. Ollivier, to whom we refer in the table, mentions it. We have nevertheless tabulated all those cases, because they have been all referred to more or less frequently as alleged examples of the accident in question. Many of the other cases are also more or less doubtful, and several of them, we think, are quite inconclusive, but to this we shall presently return.

CASES.	REFERENCE.	REGION OR VEIN.	SYMPTOMS AND RESULT.	DISSECTION.	OBSERVATIONS.
1. Klein, 1814.	Graefe and Walter's Journal, 1820, pp. 120-6.	Extirpation of thyroid gland.	Death in less than a minute.	No dissection.	
2. Graefe, 1822.	Gazette Méd. de Paris, 1839, p. 261.	Amputation of breast; gland in axilla.	Syncope, and death during removal of gland in axilla.	No important organ wounded. Heart and great vessels natural.	
3. Barlow, 1830.	Med.-Chir. Trans., 1830, vol. xvi, p. 19.	Varicose vein on cheek.	Copious hemorrhage. Syncope. Recovery.		
4. Dupuytren (Lodge and Warren).	American Journ. of Med. Sci., Aug. 1832; Gaz. Méd., 1833, p. 227.	Internal saphena vein.	Peculiar noise. Rapid death.	No dissection.	
5. Stevens (Warren).	Hay's Amer. Cyclopædia of Med. and Surg., vol. i, p. 263.				
6. Sir A. Cooper (Ollivier).	Dict. de Méd., 2d ed., t. ii, p. 70.				
7. Schuster, 1837.	Gaz. des Hôp., 1843, p. 33.	Median basilic vein (venesection).	Flow of blood suddenly ceased. A hissing sound. Arm grasped above and frothy blood expressed from the puncture. No general symptoms.		Bandage had been so tight as to impede arterial circulation. On loosening it blood flowed again as usual.
8. Simon, 1841.	Ann. de la Chirurg. Fr. et Etrang., 1842. p. 380.	Median basilic vein (venesection).	A *snuffling* noise; a bubble of air seen at puncture. Insensibility, spasmodic respiration. Puncture closed. Cold aspersions. Recovery in a minute and a half.		The blood ceasing to flow, the bandage had been loosened and a second puncture made. When patient revived the bandage was found quite loose.
9. Mott, 1828.	Amer. Journ. Med. Sci., Nov. 1828, p. 127; Med.-Chirurg. Trans., vol. xvi, p. 32.	Facial vein.	Gurgling noise. Embarrassed breathing. Irregular violent action of the heart, face distorted, general convulsions. Hemiplegia. Apparently impending death. Recovery in about half an hour.		The hemiplegia continued half the day.
10. Amussat, 1837.	Bull. de l'Acad. de Méd., 1837, t. i, p. 894.	Amputation of breast; vein below clavicle.	A whizzing sound. Patient expressed fear of dying, and became faint. Pressure on wound. Repeated pressure on sternum. Recovery.		

11. Duval (Toulmouche).	Bull. de l'Acad., 1837-8, t. ii, p. 146.	Amputation of breast.	Sound like a prolonged inspiration. Syncope. Apparent death. Recovery.	The arm was abducted when the symptoms occurred.
12. Clemot, 1830.	Gaz. des Hôp., 1830, t. iv, No. 24, p. 95.	Small vein, supposed to be external jugular (tying subcl. artery).	Sound of aspiration ceasing and recurring as finger was placed on or removed from vein. No general symptoms. Vein tied. Recovery.	
13. Pierre and Cadougnes, 1835 (Wattmann).	Sicheres Heilverfahren, &c., p. 11.	External jugular vein (venesection).	Sluggish flow of blood. Sudden whistling sound. No general symptoms. Vein immediately closed. Recovery.	
14. Rigaud, 1836.	Quelques Faits de Pratique Chirurg., p. 166.	External jugular (tying subcl. artery).	Sound of aspiration repeated three times. No general symptoms. Recovery.	
15. Malgaigne, 1836.	Gaz. Méd., 1836, p. 167.	External jugular vein.	Distinct gurgling sound. No general symptoms. Both ends of vein tied. Recovery.	
16. Koeple.	Provinc. Med. and Surg. Journ., 1842, p. 200.	External jugular vein.	A hissing sound. Patient uttered a cry and *instantly* fell senseless. Immediate compression on vein. Ligature round base of tumour. Recovery in ten minutes.	
17. Warren, 1830.	Surgical Obs. on Tumours, p. 298.	Vein near carotid artery.	Bubbling sound. Bubbles of air seen in wound. Face livid. Pulse slow. Stertorous breathing. Convulsive spasms. Vein compressed. Temporal artery opened. Recovery.	
18. Delaporte, 1836.	Bull. de l'Acad., t. i, p. 132.	Tumour on right side of neck.	Distinct hissing sound. Gurgling in thorax. Patient exclaimed she was dying. Syncope. Recovery.	The mouth was drawn to one side, and numbness felt in both hands, which continued in the right hand a month after.
19. Mayor, 1838.	Bull. de l'Acad., 1839, t. iii, p. 934.	Ditto.	*Bruit de ronflement.* Immediate insensibility. Apparent death. Instant pressure on wound. Repeated compression on lower third of sternum. Recovery in a few minutes.	The tumour passed under the posterior border of the sterno-mastoid muscle, which was divided. The symptoms came on when the deep attachment of the tumour was being separated.

CASES.	REFERENCE.	REGION OR VEIN.	SYMPTOMS AND RESULT.	DISSECTION.	OBSERVATIONS.
20. Wattmann, 1825.	Sicheres Heilverfahren, &c., pp. 7 and 119-20.	Branch of axillary vein.	Whistling sound. Immediate syncope. Pressure on wound. Axillary vein tied. Recovery.		The extremity continued cold and numb till the collateral circulation was established.
21. Ditto, 1826.	Ditto.	Ditto.	Ditto.		Ditto.
22. Clemot.	Gaz. des Hôp., 1830, t. iv, No. 24, p. 95.	Ditto.	Sound of aspiration. Syncope. Vein through which air entered tied. Recovery.		The symptoms were so sudden and alarming, that the assistants fled and left M. Clemot alone with the patient.
23. Capellati.	Provinc. Med. and Surg. Journal, 1842, p. 200.	Axilla.	A hissing noise. Patient exclaimed she was dying. Pulse irregular. Breathing laborious. *Apparent* insensibility. Compression on wound. Usual remedies in syncope. Recovery in ten minutes.		The patient remained conscious the whole time, but was unable to speak, and
24. B. Cooper, 1843.	Med.-Chir. Trans., 1844, vol. xxvii, p. 41.	Amputation at shoulder joint.	Gurgling sound. Instant collapse, threatening immediate death. Pulse fluttering. Respiration quick. Feeble sighing Urine and fæces passed involuntarily. Wound closed. Usual remedies in syncope. Horizontal posture. Recovery.		An hour elapsed before the patient could be removed to bed with safety.
25. Warren.	Surg. Observ. on Tumours, p. 182.	Internal jugular vein.	A few bubbles of air entered open mouth of vein, but were arrested and forced back by finger. No general symptoms. Recovery.		The vein was intentionally divided and the finger previously placed on it below.
26. Wattmann, 1823.	Sicheres Heilverfahren, &c., pp. 5 and 110.	Ditto, *right* side.	Hissing sound. Immediate syncope. Recovery on placing finger on vein, on removing, recurrence of the sound and of the syncope. Dull, rustling sound heard in thorax. Horizontal posture. Cold aspersion. Orifice in vein pinched up in forceps, and ligature tied round it. Gradual recovery.		The orifice in the vein was distinctly seen. No blood flowed from it, and the flux and reflux in it corresponded to action of heart. The hissing sound ceased on the lips of the orifice in the vein being separated. Both the *left* extremities were paralysed for some days.
27. Erichsen.	Edinb. Med. and Surg. Journal, No. 158, p. 19.	Internal jugular vein.	Sound of air entering vein. No other symptom. Recovery.		Sound heard when vein was raised to tie it below a wound inflicted in attempt at suicide.

28. Velpeau, 1837.	Bull. de l'Acad., 1837, t. i, p. 896; Gaz. Méd., 1838, p. 116.	Internal jugular vein.	A hissing sound, followed by bubbling in the wound. Patient exclaimed she was dying and fainted. Compression deep in wound. Usual remedies in syncope. Recovery.		Velpeau does not state what vein was opened in his first account of this case: in his second report he says the internal jugular vein was wounded.
29. Begin, 1837.	Presse Médicale, 1837, p. 463.	Ditto.	A gurgling sound. No bad consequences.		
30. Asmus, 1842.	Br. and For. Med. Rev., Oct. 1842, p. 556.	Ditto.	A bubbling sound. Patient exclaimed all was over. Convulsions, alternating with opisthotonos. Syncope. Bubbling repeated. Pressure on vein. Stimulants. Recovery in about 12 minutes.		The moment after the bubbling was heard, the patient said he was well. There was *no venous hem* till after the bubbling was heard the second time. The entrance of air was both seen and heard during the convulsions.
31. Mussey, 1837.	Amer. Journ. Med. Sci., Feb. 1838, p. 390; Gaz. Méd., 1838, p. 394.	Subclavian vein (amputation of scapula and clavicle).	Gurgling sound. A cry of distress. Pulse imperceptible. Insensibility. Compression on vein. Usual remedies in syncope. Recovery in eight or ten minutes.		Bubhles of air were seen entering the divided vein.
32. Maugeis.	Gaz. Méd., 1838, p. 117.	Median vein (venesection.)	When about eight oz. of blood were abstracted patient uttered a feeble cry, and died instantly.	No dissection.	
33. Dupuytren, 1820 (Duportail).	Gaz. des Hôp., 1837, p. 422; Gaz. Méd., 1838, p. 116.	Axilla.	No sound heard. Patient expressed uneasiness, became pale. Died in a few minutes.	Ditto.	
34. Warren, 1831.	Observations on Tumours, p. 259.	Subscapular vein.	Slight venous hem: indistinct gurgling or bubbling sound. Face livid. Apoplectic breathing. Axilla compressed. Laryngotomy, artificial respiration. Death in a few minutes.	Ditto.	The vein was isolated by dissection, opened an inch from axillary vein, and its coats were not diseased.
35. Goulard, 1833.	Gaz. Méd., 1833, p. 834-5.	Supposed to be axillary vein.	Very slight hem. Face convulsed. Almost immediate death.	Ditto.	
36. Barlow, 1830.	Med.-Chir. Trans., 1830, vol. xvi, p. 28.	Tumour on neck; cutaneous vein.	Hissing gurgling sound. Sudden death without sigh or convulsions.	Ditto.	The sound is stated to have evidently rushed from a large divided empty vein.
37. Anonymous (Amussat).	Recherches sur l'Introduction de l'Air, &c., p. 151.	External jugular vein (venesection in cholera).	No blood issued: a tube was passed into the vein towards the heart. The patient uttered a feeble sigh and died.	Ditto.	

CASES.	REFERENCE.	REGION OR VEIN.	SYMPTOMS AND RESULT.	DISSECTION.	OBSERVATIONS.
38. Wattmann and Dumreicher.	Sicheres Heilverfahren, &c., p. 122.	External jugular vein.	Hissing sound. Palor. Syncope, and convulsions. Vein compressed. Symptoms ceased in half an hour after vomiting. Death on twenty-eighth day.	No dissection.	Aneurism by anastomosis in a child aged 14 months. Death was caused by pneumonia.
39. Mirault.	Gueretin, Thèses de Paris, July, 1837.	Right internal jugular vein.	Whizzing sound repeated three times. Tetanic spasms. Patient revived in eight minutes. Sudden death three hours after the operation.	Ditto.	Twelve ounces of blood were not lost during the operation (extirpation of tumour of the neck).
40. Clemot.	Gaz. des Hôp., 1834, No. xxiv, p. 95.	Amputation of breast.	Death some hours after operation.	Air found in veins between wound and heart, and in right cavities of heart.	
41. Delpech, 1830.	Mém. des Hôp. du Midi, 1830, pp. 231-641; Gaz. Méd., 1834, p. 349.	Amputation at shoulder, *before* joint was opened.	A snuffling noise twice heard. Syncope. Death by termination of operation.	Body opened under water. An immense quantity of bubbles of air escaped on opening vessels and right cavities of heart.	A case of fungus hematodes. The vessels were greatly enlarged, but very little blood was lost.
42. Roux, 1832.	Journ. des Connaiss. Médic. Chirurg. 1836; Gaz. Méd., 1837, p. 482.	Ditto.	Whizzing sound, *thought* to be heard by some. Syncope. Death by termination of operation. Symptoms came on *while deltoid muscle was being divided*, and the axillary vessels were compressed before they were divided.	Right ventricle elastic, opened under water, and eleven cubic centimeters of air collected from it. Air in coronary veins and vena cava. No air in veins of neck, axilla, or in brain.	All the air in the right ventricle could not be collected. Air on examination was found to be atmospheric. The symptoms came on while the deltoid muscle was being divided to form a flap.
43. Castara, 1826.	Saucerotte, Thèses de Strasbourg, 1828.	Vein joining the subscapular vein (tumour on right shoulder).	Several rapid gurgles seeming to come from bottom of wound. Insensibility, eyes upturned, palor. Cessation of pulse and respiration. Two deep inspirations. Death without convulsions.	Right cavities of heart distended with blood, mixed with air. A little blood mixed with some bubbles of air in left cavities of heart. Blood and air in right subclavian vein and superior vena cava. No air in inferior cava. Bubbles of air in subscapular vein and veins of right arm to bend of arm, interrupting the column of blood here and there.	The body was examined twenty-four hours after death, and presented no trace of decomposition. The vein that was opened adhered to the pedicle of the tumour, on raising which the wound in the vein was seen as a round open orifice, not a line in diameter, from which bubbles of air issued on pressing the subscapular vein.
44. Anonymous (Putegnat).	Thèses de Paris, 1834, No. clvi.	External jugular vein (venesection in apoplexy).	Sudden death.	Air in right auricle of heart.	

45. Pellis, 1838.	Bull. de l'Acad., t. iii, p. 939.	External jugular vein (suicide).	Flux and reflux of blood distinctly observed in vein, which bled freely. Patient threw his head back and inspired deeply, when a gurgling noise was heard, followed by immediate death.	Heart removed: the vessels being tied, it swam on water. Air in ail the cavities, especially the right. On incising the cavities, air escaped and the heart sauk.	The air in the heart was collected, analysed by M. Bischoff, and found to be atmospheric air.
46. Dupuytren, 1822.	Archiv. Gén. de Méd., t. v, p. 430.	Posterior and lateral region of neck.	Sound like air entering an exhausted receiver. Patient exclaimed she was dying. General tremors. Sunk insensible and died. Cold aspersion and artificial respiration tried.	Right auricle distended with air, which escaped on opening it; contained some liquid blood. Blood mixed with air in other cavities of heart and in vessels of trunk, limbs, and brain.	The body, examined 24 hours after death, presented no trace of decomposition. The tumour was kept on the stretch and moved from side to facilitate the dissection, when the symptoms supervened.
47. Beauchene, 1818.	Magendie's Journal, t. ix, p. 80.	External jugular vein, within a line of subclavian vein.	A whizzing sound, which ceased on application of finger, and returned on removing it. Patient exclaimed she was dying. Peculiar rustling sound heard in thorax. Convulsive motions. Insensibility. Death fifteen minutes after operation, which lasted half an hour.	No blood or air in any of the cavities of the heart. Numerous bubbles of air in all tbe vessels of the brain, in the vena cava inferior, iliac veins, aorta and crural arteries. Dissection eighteen hours after death.	The symptoms occurred when the clavicle, which had been divided at the centre, was pulled forward to facilitate its separation. The patient was a man, aged 23, had lost little blood, and did not appear weakened.
48. Graedener.	Prov. Med. and Sur. Journ., May 14, 1842.	Tumour on the neck.	A hissing sound. Immediate death.	Cavities of heart distended by a great quantity of air.	
49. Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	
50. Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	
51. Roux, 1832.	Gaz. Méd., 1833, p. 498; Journ. Hebdom., May, 1833.	Internal jugular vein.	Sound like air entering an exhausted receiver. Patient uttered a plaintive cry. Agitation. Embarrassed breathing. Apparent death. Wound compressed. Patient. A ligature round tumour. Death, *seventh* day.	Internal jugular vein divided. Cavities of heart empty. Air mixed with blood in aorta and iliac arteries. No air in veins or in vessels of brain. Lungs œdematous. Frothy fluid in bronchiæ.	The symptoms were coincident with putting the tumour on the stretch, to facilitate the dissection.
52. Ulrich, 1834.	Journ. des Connais. Méd. Chirurg., t. ii, p. 91.	Ditto.	A whizzing sound, *thought* to be heard by some. Not a drop of blood flowed from vein, which remained patulous, like an artery. Soon some hem from lower end of vein. Syncope. Slight convulsions of face. Opisthotonos. Death in one minute.	Dissection fifty-two hours after death. Right auricle distended with alr, and contained no blood. Blood and no air in right ventricle.	

CASES.	REFERENCE.	REGION OR VEIN.	SYMPTOMS AND RESULT.	DISSECTION.	OBSERVATIONS.
53. Gorré, 1842.	Annal. de la Chir. Fr. et Etrang., Nov. 1842, p. 305.	Ditto, near clavicle.	A gurgle from wound towards heart. Pallor. Quick respiration. Patient uttered a plaintive cry, said she was dying, and died within a minute. An attempt was made to close the orifice of the vein and compress the thorax.	Much frothy liquid blood in right cavities of heart and internal jugular vein. Some bubbles of air in superior cava, subclavian, axillary, brachial veins; veins of brain. In aorta and iliac arteries.	The hemorrhage was trifling. The body was examined 24 hours after death. The orifice in the vein was oblong and gaping. The symptoms occurred when the tumour was put on the stretch. No air or blood in right heart.
54. Handyside, 1837.	Edinb. Med. and Surg. Journal, 1834, vol. ix, p. 209.	Left facial vein; right external and internal jugular veins (suicide).		Air in both internal jugular and thyroid veins, both venæ cavæ, veins of liver, spleen, and kidneys. In left carotid aorta, femoral, popliteal, brachial arteries. Some air and no blood in all the cavities of the heart.	Dissection 26 hours after death in very dry cold weather. Left spinal accessory nerve and right pneumo-gastric nerve divided. A pound and a half of blood had been lost.
55. Verrier, 1806.	Bull. de l'Acad., 1837, p. 364.	External jugular vein of horse (venesection).	Loud gurgling, followed by usual serious symptoms on ceasing compression below to terminate operation. Recovery.		
56. Bouley, 1821.	Magendie's Journ., t. i, p. 197.	Ditto.	On removing pressure below puncture horse fell as if thunderstruck. Blood allowed to flow again. As it issued horse gradually revived and recovered.		
57. Gerard.	Gaz. Méd., 1838, p. 117.	Ditto.	Ditto.		
58. Bouley, 1838.	Bull. de l'Acad., 1838, t. iii, p. 465.	Ditto (left side).	While securing puncture, hurried laboured breathing. Pulse ceased. Tremors. Death in a few minutes.	Frothy blood in left jugular vein. Air mixed with coagulated blood and some liquid blood in right cavities of heart. Frothy blood in pulmonary artery and its minutest branches. Clot with globules of air in left heart. Air in veins of abdomen and brain.	M. Bouley was absent when the air is supposed to have entered the vein. The groom did not perceive any sound. One lung was hepatized.
59. Ditto, 1841.	Bull. de l'Acad., 1841, t. vi, pp. 178-83.	Ditto.	Embarrassed breathing. Pulse imperceptible. Blood, mixed with numerous bubbles of air, flowed from vein. Death in seven hours.	Frothy blood in right cavities of heart, pulmonary artery, and right cavities of heart, in veins of abdomen and brain.	Ditto. Dissection two hours after death.
60-61. Ferries.	Ditto, p. 183.	Ditto.	Death soon after the operation.		No further particulars given.
62 to 67. Leblanc.	Gaz. Méd., 1838, p. 117.	Ditto.			No particulars given. Not stated whether any of the animals died. It is merely said that M. Leblanc saw air enter the vein during venesection in six horses.

It would lead us to tedious and unnecessary prolixity were we to minutely analyse each of the cases in the foregoing table, but we must consider the value of the objections made to some of them at least. To begin with the simplest case, Velpeau disposes of the 9th, 10th, 22d, and 40th, by alleging that the veins opened were beyond the limits of danger; but not only is this an assumption without any proof, but it is certainly erroneous, at least as regards the 10th, 22d, and 41st cases, for the axilla (case 22) is within the limits of danger, and as in the 41st case, air was found in the veins between the wound and the heart, and in the right cavities of the heart, it is proved that the veins divided during the amputation of a breast may admit air. Velpeau admits that air may have entered the veins in the 14th, 15th, and 18th cases, but if so, he says, we can only conclude from them what is already known from experiments on animals, that the accident is by no means uniformly fatal. We, of course, readily admit this truism, but confess we cannot understand how it militates against the fatality of the accident in other cases; unless, indeed, the fact that some animals survived entrance of air into the veins is also held to prove that none of the animals submitted to experiment died from this cause. The 37th and 44th cases are objected to, because the veins were too small and remote from the heart, and because death was sudden and without convulsions; the former objection we have already seen is unfounded, and in the 44th case the relations of the vein to the tumour were precisely those which it is generally admitted may extend the sphere of danger beyond its ordinary limits, and to veins smaller than usual. As to the second objection, convulsions by no means uniformly occur in animals, and independently of the fact that the rapidity of death varies in different animals, M. Segallas's experiment shows that death may be immediate even in dogs. The same objections are advanced against the 42d, 43d, 47th, 48th, and 53d cases, with the addition that, as regards the 42d case, the sound of air entering a vein in animals is not a *renifle-ment;* that in the 42d and 43d cases we do not know what was the condition of the blood in the right side of the heart; that in the 47th and 53d cases, the air in the heart was not mixed with the blood; and that in the 48th case air was found in numerous vessels, but not in the heart. We have already seen how much the sound of air entering a vein varies in many respects, and when we further consider that its tone must vary with the size and tension of the edges of the aperture through which air passes, we might almost call such an objection mere cavilling. Independently of theoretical considerations, the influence of the condition of the orifice of the vein on the sound is well exemplified by one of Dr. Wattman's cases (26th), in which the sound ceased when the lips of the orifice were sufficiently separated. Though it is digressing a little, it is convenient to notice here M. Blandin's objection, that all the cases in which a "hissing" sound was heard are probably a mistake, as he was once alarmed by this sound, but found it caused by the rapid projection of blood from an artery against the adjacent tissues (Bull. de l'Acad. 1837, p. 903); and also his allegation, that he has repeatedly known a deceptive *lapement* and *gargouillement* to have been heard during an operation, whence he argues that no conclusion can be deduced from the mere occurrence of those

sounds. (Des Accidents qui peuvent survenir pendant les opérations.) But those objections, be they ever so well founded, can only apply to such cases as the 12th, 13th, 14th, 15th, and 30th, in which a sound resembling that of air entering a vein was the solitary symptom observed. But to return to the cases specified above as objected to, because of the condition of the air in the heart or the absence of air from that organ, it has been already seen that the objections are of no weight; the air was sometimes found unmixed with blood, even in the very set of experiments on animals which are arbitrarily assumed by M. Velpeau as a standard of comparison, and Poiseuille has found the heart completely empty of air. We have, we believe, answered all the objections which have been made to any of the cases above adverted to (or indeed to any case worthy of serious examination), with the exception of one, viz. that there is no evidence that a volume of air, sufficiently large to cause either death or the formidable symptoms frequently observed, entered the veins in any of the recorded cases, and that from the rapidity of the supervention of those symptoms or even of death, it is very unlikely that such a volume of air could have entered; and the following computation made by M. Barthélemy has been appealed to in support of this objection. M. Barthélemy found that on an average *four* litres of air quickly (within 7 or 8 seconds) injected into the jugular vein of a middle-sized horse, i. e. weighing about 450 kilogrammes, caused death in a few minutes; and if we suppose (which M. Barthélemy, however, by no means assumes) that the same relation exists between the weight of different animals and the quantity of air necessary to kill them, he computes that it would, on that hypothesis, require the rapid introduction of a little more than $\frac{3}{5}$ of a litre of air into the vein of a middle-sized man to cause death. (Bull. de l'Acad. t. i, p. 370-5). With respect to this hypothetical calculation, it is sufficient to say that its basis utterly fails, for M. Renault found that the sudden injection of a *litre and a half* of air always sufficed to kill a horse. (Bull. de l'Acad. 1841, p. 181.) We might further speculate that the higher organization of man, his upright posture, the shortness of his neck, the greater proximity to the heart of the vessels opened in almost all the recorded cases, than of those operated on in most of the experiments on animals, may favour the deleterious action of air admitted into the human veins, but without dwelling on such inconclusive speculations, we really think it sufficient to read the account of the appearances on dissection in some of the fatal cases, especially the 42d, 43d, 46th, 47th, 48th, and 54th, to see that a very large quantity of air did enter the circulating system in those cases, fully as much, even relatively to the size of the animals, as is described to have been found in the dogs experimented on by the committee of the French Academy of Medicine. This large quantity of air was also admitted with great rapidity, whence its influence was proportionally greater, and if its presence is not admitted to be sufficient to explain death in those cases, we think it impossible to avoid the conclusion that neither can the death of the animals, submitted to experiment by the committee so often referred to, be attributed to the entrance of air into their veins.

If, instead of merely answering objections, we examine the circumstances of some of the foregoing cases, it seems really difficult to understand how

it can be denied that death resulted from entrance of air into the veins, unless it is at the same time contended that all the experiments on animals respecting the spontaneous entrance of air into the veins, are also fallacious and insufficient to establish the reality of death from this cause. In Beauchene's case for example, the external jugular vein is opened within a line of the subclavian vein (almost equivalent to a wound of the latter vein itself) : a whizzing sound is heard, which ceases on applying the finger to the wound, and returns on its removal; the patient exclaims she is dying, *a peculiar rustling sound* is heard *in the thorax*, convulsive motions with insensibility supervene, and death occurs in 15 or 20 minutes; on dissection numerous bubbles of air are found in the vessels of the head, trunk, and lower extremities : the chain of evidence is complete, and yet this case is rejected because of the erroneous assumption that the wound was beyond the limits of danger, and the equally unfounded objection that the heart uniformly contains air after death from admission of air into the veins. The 44th case is as strong, stronger we think it could not be, and what can be more conclusive than the 54th case. But to examine these cases, and some of the others further, would in effect be only repeating in another form what has been already said.

We have left unnoticed many cases in the table, but we willingly admit that we have not sufficient particulars respecting several of them, to form any opinion about them one way or the other. And as our object is merely the general one of endeavouring to establish the fact of death occurring from the spontaneous admission of air into the veins, a particular examination of all the cases, especially of the more doubtful ones, would be quite beside our purpose.

Even those who question that death has ever resulted from admission of air into the veins during an operation, admit that it is prudent in practice to take due precautions for its prevention, and to be prepared to remedy its consequences should it occur; and as some of the remedial measures proposed, are based on the views taken of the action exerted by air when admitted into the vascular system, we must advert to those views.

Brunner, Camerarius, Harderus, Sprœngel, Nysten, Magendie, Dupuytren, Amussat, and others, attributed death to interruption of the circulation from extreme distension of the right cavities of the heart; Harderus, and subsequently Nysten, conceived that the over-distension of the fibres of the heart destroyed their contractility, and Magendie is of opinion that the great distension of that organ is caused by the sudden expansion of the air, from its temperature being elevated on entering the vessels. M. Denot is also of opinion that death arises from the circulation being suspended by the presence of air in the heart, but he does not think that this occurs because the heart is overdistended, but because the tricuspid valves, which permit some reflux even of the blood, easily allow the reflux of an elastic fluid, and the air, therefore, plays to and fro between the right cavities, which thus contain a fluid which they are unable to propel; the access of venous blood to the lungs is, he says, thus interrupted and death results as if the pulmonary artery were tied. (Gaz. Méd. 1837, p. 726.) Mr. Wing also refers death to disturbance of the circulation; the distension of the right cavities of the heart causes the valves to

act imperfectly, and the blood regurgitates at each contraction of the ventricle, this reflux increases the disturbance of the heart's action, which increases till the action of the organ ceases, not gradually, but suddenly. (Boston Med. and Surg. Journ. and Gaz. des Hôpitaux, 1835, p. 108.) Arrest of the circulation is likewise the chief cause of death admitted by M. Mercier, who thinks that there is—1st, venous stasis, consequent on accumulation of blood in the right auricle; and, 2dly, that the action of the right ventricle condenses the air in the pulmonary artery, which causes the blood to be imperfectly propelled through the venous capillaries, the more so as it experiences great difficulty in traversing thém in consequence of being frothy from admixture with air; the blood, therefore, reaches the left heart, and the brain very slowly, and their functions are proportionally disturbed, especially, as whatever blood does reach them is mixed with air. (Gaz. Méd. 1837, p. 481.) M. Poiseuille adopts a portion of M. Mercier's theory; he considers that the *sole* and *only* cause of death is complete cessation of the pulmonary circulation from the frothy blood being unable to traverse the capillaries of the pulmonary artery, the lung is, therefore, obstructed, and little or no blood gains the pulmonary veins. Distension of the right cavities of the heart, whether with air or with blood, or both, is, he thinks, simply the result of obstruction of the pulmonary circulation. (Gaz. Méd. 1837, p. 671.) M. Gerdy's opinion is very similar, but he refers the obstruction of the pulmonary circulation to the mere presence of air in the pulmonary artery, without reference to the frothiness of the blood impeding its passage through the pulmonary capiliaries (Bull. de l'Acad t. ii, p. 287); a view which is also adopted by M. Segallas. (Op. cit. p. 425.) M. Bouillaud adopts a mixed opinion, and considers that the principal causes of death are—enormous distension of the right cavities of the heart, produced by the air which is expanded by the heat of the blood;—the difficulty experienced by the frothy blood in traversing the pulmonary capillaries,—possibly the compression exerted by the air in the pulmonary system, obstructing respiration and producing asphyxia;—and probably pressure on the brain in the cases where the air reaches the vessels of that organ.

M. Piedagnel (Magendie's Journ. 1829, t. ix, p. 6) and M. Leroy (Archiv. gén. de Méd. 1823, t. iii, p. 142) think that dilatation of the air from its sudden elevation of temperature, ruptures the capillaries of the lungs, which thus become generally emphysematous, and the circulation at once stops.

Bohn long since considered that air injected into the veins acted as a powerful and rapid poison. M. Marchal de Calvi has recently received this theory, and maintains that the poison is carbonic acid; for that gas must be evolved when atmospheric air encounters venous blood, and the bright red colour of the venous blood in animals killed by throwing air into the veins proves that the blood is decarbonized. He thinks that the more rapidly fatal effects of air expired from the lungs favour this view; but he acknowledges that Nysten's experiments on injecting carbonic acid gas into the veins discountenances his theory. We should rather say they completely disprove it. (Annales de Chirurg. Franc. et Etrang. Nov. 1842, p. 302.) Bichat's theory that air killed by irritating the brain,

perhaps comes under this category; but its erroneousness is shown, as Nysten observed, by the fact that the air by no means constantly reaches the brain. Nysten, however, thought that when air did penetrate to the vessels of the brain, it acted injuriously by compressing that organ.

The opinion of MM. Piedagnel and Leroy now finds no countenance. No emphysema of the lungs existed in any of the experiments of the commission. Mr. Cormack blew air with much force into the veins, and no emphysema resulted, and he showed that the natural appearance of the rabbit's lung, which much resembles a human emphysematous lung, probably misled M. Piedagnel.

The experiments of M. Barthélemy, coupled with some facts observed by many others, disprove, we think, all the theories which refer death to suspension of the circulation, whether caused by distension of the heart, viscidity of the frothy blood, or any other cause. M. Barthélemy found that the sudden injection of four litres of water, a fluid perfectly miscible with blood, produced the same effects as the injection of the same quantity of air. As to distension of the heart, that organ is by no means always distended, its volume and consistence are often perfectly natural, especially it would seem in the horse, and the air instead of being accumulated in it, is disseminated through the circulating system even when the horse has lived but a few minutes. Sometimes on opening dogs, rapidly killed by the entrance of air into the veins, the heart is found contracting, and M. Barthélemy found the heart contract in horses till the last expiration. M. Barthélemy also performed the following experiment. He cut off the tail of a horse, and the blood jetted from the coccygeal arteries; he then threw four litres of air into the jugular vein, the jet from the arteries then intermitted, but soon became larger and stronger than previous to the intermission; after three or four minutes the flow of blood became feeble, but continued uninterruptedly till the animal expired, so that the circulation and the respiration ceased almost simultaneously. M. Barthélemy performed this experiment on ten horses; in one animal the arteries jetted after respiration ceased, and in another they continued to bleed for one minute after the last expiration. (Bull. de l'Acad. t. i, pp. 377-61.)

We cannot then consider any of the foregoing explanations satisfactory, neither does the following view, proposed by Dr. Wattmann, seem more so. The ordinary condition of fulness of the vascular system allows the heart a moment's rest after each contraction; but the admission of air renders the vascular system full to tension, and then neither the fibres of the heart that contract, nor those that dilate the organ, are allowed the necessary alternation of rest and exertion; thence the activity of the heart and its influence on the circulation diminish every moment, and at the same time the blood reaching the lungs mixed with air and in diminished quantity is imperfectly decarbonised, and its electro-magnetic conducting power correspondingly diminished. Consequently, whatever portion of blood does reach the several organs through the systemic circulation, is incapable of stimulating them properly, whence their functions become suspended.

A variety of precautions have been recommended to prevent the entrance of air into the veins during an operation, in a region where that accident

may be apprehended. Experiments on animals having shown that there is more danger in the vertical position, M. Lisfranc and others recommend that the patient should be placed in the horizontal posture. Amussat and others consider that the most important and effectual precaution is to permanently compress the veins between the part operated on and the heart, but this would often be impracticable. Dupuytren attached particular importance to avoiding putting the parts on the stretch while the dissection was being performed, and, to facilitate this, recommended, in the extirpation of a tumour, that it should be removed piecemeal if necessary. M. Velpeau advises that motions of the neck or arm should be avoided, and that the deep attachments of a tumour situate near a large vein should be firmly grasped or encircled by a ligature before they are divided. Cormack recommends that the patient should be made to inspire before a vein is divided; and M. Gerdy, with the view of limiting the motions of inspiration, considers that the thorax should be encircled with a tight bandage. (Bull. de l'Acad. t. i, p. 909.) Of those recommendations the last only is objectionable; but the others, however useful, cannot always be adopted, and it must, we think, be admitted that no care, no precaution, however great, can afford any certain guarantee against the occurrence of the accident.

When symptoms indicating the admission of air into the veins occur, a variety of means have been proposed to remedy the consequences, and many of those means are founded either on the results of experiments on animals, or the view taken of the mode in which the air acts injuriously.

Nysten concluded, from his experiments on dogs, that sudden compression of the abdomen and thorax was beneficial in remedying the symptoms in question. M. Amussat is of opinion that the most effectual remedy we possess, is to repeatedly compress the thorax during expiration only; the air he thinks may be thus expelled from the heart, and he recommends that the vein should at the same time be immediately closed, provided a small quantity only of air has entered; but if much air has gained admission, and especially if frothy blood issues from the wound, closing the vein he thinks hastens death, while by leaving it open, at least during expiration, and while the thorax and abdomen are compressed, the left cavities of the heart may be emptied of air and life preserved. M. Mayor also recommends repeated pressure on the thorax, which he considers useful, by exciting artificial respiration as well as by expelling the air from the heart, and recommends the orifice in the vein to be always closed. (Bull. de l'Acad. t. iii, p. 938.) MM. Gerdy and Velpeau object to compression of the thorax, that even if it is useful when applied to the yielding thorax of the dog, the human thorax could not be safely compressed so as to act on the heart. M. Blandin also objects that the valves of the veins would prevent the reflux of air; but to this M. Amussat replies that the veins are destitute of valves at the lower part of the neck, as is shown by the flux and reflux of the blood. (Bull. de l'Acad. t. i, pp. 899-912; t. ii, pp. 20-275).

As the presence of air in the circulating system is the cause of the mischief, it seemed rational to endeavour to remove it, more especially as its chief injurious action was considered to consist in distending the heart,

and as in some experiments the animals revived when frothy blood escaped from the wounded vein. The proceedings above adverted to contemplated the expulsion of the air from the heart, but Magendie conceived the idea of removing it directly by aspiring it through a tube passed into the vein towards the heart. Mr. Cormack in his experiments found this proceeding sometimes succeed, and sometimes fail, but thought any good effects derived from it were more attributable to the evacuation of blood than of air. Mr. Cormack, however, it is right to add, notices the danger of air entering the tube, and though MM. Blandin, Rouchoux, Segallas, and some others (Bull. de l'Acad. t. i, pp. 901-3; t. ii, p. 428), theoretically recommend this practice, no one, we believe, has had the hardihood to apply it to the human subject. Even were it safe, there would rarely be time for so delicate a proceeding; in most cases it would be absolutely impracticable, and if practicable it would, to use M. Barthélemy's words, but open the door to the enemy.

Bloodletting has been found useful in experiments on animals by Mr. Cormack, and by MM. Bouley, and Leblanc. More recently it has been strongly recommended by M. Marchal de Calvi. (Annales de Chirurg. Franc. et Etrang. Nov. 1842, p. 304.) This practice certainly deserves further experimental investigation. .

Though bloodletting is recommended on the grounds that it diminishes the contents of the circulating system, and thus relieves the over-distension of the heart and vessels, yet M. Mercier advises a proceeding which, so far as it is efficient, must have a contrary effect; he recommends that the abdominal aorta and axillary arteries shall be compressed, with the view of directing towards the brain whatever blood does reach the arterial system, and states that he found this method very beneficial in experiments on the dog. (Gaz. Méd. 1838, p. 236.)

M. Poiseuille, from his theory as to the mode in which death occurs, recommends that the patient shall be placed in such a position that the air in the right side of the heart shall not correspond to the orifice of the pulmonary artery. (Gaz. Méd. 1837, p. 671.) M. Denot, on the contrary, influenced by his theory, advises that the patient shall be placed in the dextro-dorsal posture, to favour the flow of blood from the right ventricle into the pulmonary artery. (Gaz. Méd. 1837, p. 728.)

When air is supposed to have entered a vein, there is no indication so obvious as to prevent its further access by immediately compressing the wound, and accordingly reference to the Table of cases will show how generally that practice has been adopted. M. Velpeau, it is true, doubts how far closing the orifice of the vein is dangerous or the reverse, for though so doing prevents air entering, it equally prevents its exit. We have also seen the limitations M. Amussat puts to this practice; but both those gentlemen adopted it in their own cases. (Cases 10 and 28.) But, with those partial exceptions, we may fairly say this practice is generally approved of, and has been generally adopted. Now this is precisely the practice which Dr. Wattmann has written a book to recommend as "a certain remedy," and which he inculcates in the two following paragraphs with whatever emphasis is conveyed by printing in capital letters.

"The required therapeutic Indication is to prevent as quickly as possible, and permanently, the Entrance of air into the Veins."

"The indication can only be fulfilled by the Instantaneous and permanent closure of the wound of the vein." (p. 94.)

As we have not the slightest doubt of the propriety of this practice, we shall not occupy space by detailing the reasons adduced by Dr. Wattmann in its support. We need only say that as regards the permanent closure of the orifice in the vein, Dr. Wattmann is satisfied with compression whenever the wound can be completely closed, or when a flap or one of the lips of the wound can be laid over the vein and retained so by sufficient compression. When this cannot be effected, he either includes the vein in a ligature (as in the 20th and 21st cases, in which the axillary vein was tied), or if there is but a small opening in a large trunk, he pinches up the orifice in a forceps and ties a ligature round the portion of the coats of the vessel included in the instrument; in this way he successfully secured a wound of the internal jugular vein in the 27th case. Should the wounded vein lie too deep to thus apply a ligature, as in the case of the subclavian vein in a fat person, Dr. Wattmann proposes that the lips of the wound should be pinched up in a small light spring forceps, which should be left *in situ* as long as might be necessary. This mode of securing a partially wounded vein is by no means so original as Dr. Wattmann seems to imagine; for Mr. Guthrie, in a case of suicidal cut throat, included a small wound in the internal jugular vein in a circular ligature. (On the Diseases and Injuries of Arteries, p. 328.) We do not pretend to deny that the ligature of a large vein may not sometimes be necessary, but we believe it will be rarely so; we have seen the axillary vein wounded during the extirpation of schirrous glands in the axilla, a dossil of lint was laid on the wound, and all went well. In the Table of cases above given, the internal jugular vein was wounded in the 29th, 30th, and 31st cases, and no ligature was requisite, simple compression answered every purpose.

Dr. Wattmann is of course perfectly aware that closing the orifice in the vein has often been practised; but he says it has only been considered of secondary importance, or at most as a method to be employed in conjunction with others, and not as the means on which our sole and only dependence should be placed. He admits, however, that if a sufficient quantity of air has gained admission to produce general symptoms, especially syncope, that the means usually had recourse to under such circumstances should not be neglected, such as the horizontal posture, free access of air, cold aspersion, the application of the vapour of ammonia or of strong vinegar to the nostrils, warmth to the extremities, &c. Artificial respiration he deems useless.

On reviewing the various modes of treatment which have been recommended to remedy the effects of admission of air into the veins, it must, we think, be admitted that our resources are very limited, and perhaps the only really useful measures are closing the aperture in the vein, and the general means alluded to in the last paragraph. We should, however, be inclined also to practise moderate compression of the thorax during expiration, not on the grounds recommended by M. Amussat, but in accordance with M. Mayor's view of exciting artificial respiration. Dr. Warren in one case performed laryngotomy, in order to employ artificial respiration, but this we consider useless and injurious as involving loss of time; injecting a saline solution into the veins, proposed by the same gentleman,

is still more to be deprecated. Bloodletting would appear from some experiments on animals to be useful, but further experiments are required before it can be recommended with any confidence.

We shall not examine Dr. Wattmann's speculations respecting the medico-legal import of entrance of air into the veins; for he makes several assumptions which we have shown to be erroneous. Thus he says, that in every examination after death from entrance of air into the veins, the right cavities of the heart have been found to contain air mixed into a froth with blood, and air has also been found in the vessels of the brain; and he also admits that the presence in the heart of free air unmixed with blood, indicates that the air has been admitted after death. We shall, therefore, here terminate our notice of Dr. Wattmann's work, which, though scarcely perhaps in all respects what we might have anticipated from the high attainments and eminent position of the author, is certainly a work of much research, and taken altogether is, we believe we may safely say, certainly the best extant on the subject of which it treats. As that subject is one respecting which opinions are very unsettled, and which is not very likely to come frequently before us, we have taken the present opportunity to consider it somewhat in detail.

Art. XI.

The Moral Aspects of Medical Life, consisting of the 'Akesios' of Professor K. F. H. Marx. Translated from the German, with Biographical Notices and Illustrative Remarks. By James Mackness, m.d., Member of the Royal College of Physicians, &c.—*London,* 1846. Sm. 8vo, pp. 348.

In the 19th Volume of this Journal we noticed Professor Marx's 'Akesios; a Glance into the Ethical Relations of Medicine.' Dr. Mackness has taken up this thin volume, and by adding a biography of each person to whom Professor Marx addressed his letters, and a running commentary, has swelled it into a goodly octavo. His first intention was to publish a translation only, but as he proceeded to scan the work, line by line, he was "struck with its remarkable condensation of style, and with the germs of noble thought which often lay buried in a single sentence." So Dr. Mackness determined to make these germs germinate, and to view on all sides certain interesting subjects of which Professor Marx only takes a glance; to compare, contrast, illustrate them.

The biographical notices are all selected; some of them are translated from the collection of 'Eloges Historiques,' and others from articles in the 'Dictionnaire Universelle.'

The biographical notice of Stieglitz is derived from a recent memoir of that physician by the author of 'Akesios,' entitled 'Zum Andenken an Johann Stieglitz.' The details of the professional life and character of the twelve distinguished physicians and surgeons to whom Marx addresses his epistles are generally interesting and instructive.

The 'Remarks' which follow each letter are, on the whole, excellent, and creditable alike to the head and the heart of the commentator. Some

little blemishes a critical eye can easily detect in them. They are somewhat too much like sermons; too much in the firstly, secondly, thirdly vein to read well, at least when regarded in a mere literary point of view. The great member of quotations is a further drawback. "'Besides the qualifications,' says Gregory." "'Woe to the physician,' says Simon, 'who,'" &c. "We much admire Percival's view of this subject; he says," &c. "Here we may cite a passage from Dr. Simon. 'If the masters of the science,' says he," &c. Now, all this "he says and she says" gives the book a gossiping air; and the numerous phrases of a peculiar stamp so liberally interspersed through the remarks, make us involuntarily think of an evangelical tea-table. We do not use the phrase in an offensive sense, and merely wish thereby to designate that class of persons who, thoroughly devoted to religion, get their conversation tinctured with biblical phrases, and contract a sermonizing style. We honestly think it to be matter of regret that Dr. Mackness's work should be thus tinctured, for it abounds with just sentiments and pure morals, and the effect of the peculiarity mentioned is to render these valuable qualities less attractive. Many of the quotations are eminently beautiful, and several of the views expressed by the commentator are good and well given: they would have been indeed much better if Dr. Mackness had boldly pushed off and quoted less. For example, Dr. Mackness would have written much better on cheerfulness as a remedy, if he had trusted himself more, and the "late excellent Rev. Robert Anderson of Brighton" less.

We shall now give a few extracts as examples of Dr. Mackness's style, and on some of them we may offer occasional remarks.

"There is one trait in the character of Hallé which, however much we may admire the spirit which prompted it, we cannot approve, and that is his reluctance to receive fees, even from those who could well afford to pay. It is true that his ample fortune might be quite sufficient to supply all the wants of his family, and, therefore, such a line of conduct was attended with no great sacrifice; while to his benevolent mind it doubtless afforded him the purest enjoyment. But did none others suffer from such a line of policy? Was the medical profession so affluent in his day in Paris, that there were no men of energy and talent belonging to it, who, without property, without friends, felt their spirits broken, their hearts sickened, for want of professional success? Such is too often the case in England, and doubtless the same in France." (p. 66.)

This is all very correct; so also is the adoption of Percival's views that the poorer clergy should be attended gratuitously, as well as all the members of the medical profession and their immediate families. But we should have been glad to have seen Dr. Mackness raise the question as to the fees to be demanded from those persons who are "decent people" but not poor; persons with large families and limited incomes, but "respectable." Is the same fee to be taken or demanded from these as from the wealthy? and if not, on what principles, or according to what proportion is the abatement to be made? Is the physician to refuse his advice to the decent person? Or how is it to be given? We believe that a just determination on this point can only be come to, on weighing the merits of individual cases. But we trust the medical profession will never forget their glorious mission as helpers of humanity, and think of their duty first, their interest afterwards. In the class of persons here referred to, we often find the

most liberal disposition in regard to fees, and at the same time a becoming pride in coming under obligations. A wise and good physician will generally find little difficulty in deciding as to the proper conduct. A fee may be once, or occasionally, taken to destroy the unpleasant sense of obligation; and excuses may easily be found for paying friendly visits, more or less frequently, according to the nature of the disease and other circumstances. In some cases of the kind under consideration, unquestionably, fees should be entirely declined; and the heart that is really kind, will readily prompt excuses that will not give offence. In all cases, and under all circumstances, let the physician remember, that liberality carried even to excess is but a venial fault, while the reverse is a professional sin of the deepest dye, degrading to the individual, and dishonoring to the class to which he belongs.

The letter to Albert Thaer affords an opportunity for the discussion of medical scepticism. With reference to the present direction of the current of change in the profession, Dr. Mackness observes:

"An important change appears in the present day to be taking place in the minds of many of the medical body, especially in its higher sections—a change favorable to greater reliance on the powers of Nature and on simple hygienic measures. Whilst we foresee from these changes a better era in the history of medicine, there is also some reason to fear the increase of medical scepticism, for which, in Simon's view, homœopathy itself is only a decent name. Dr. Robert Williams is mentioned, in an oration to the Medico-Chirurgical Society, as having had no faith in medicine. One can sympathize with men who, like Thaer, Haller, and others, declined practice from an over-scrupulousness of mind and dissatisfaction with the state of the science, but it seems hardly consistent with moral honesty to continue to practise with such views of the medical art." (p. 138.)

We fully agree with the opinion here expressed; but we suspect there are very few persons indeed who have no faith in medicine. Even the apparently greatest sceptics are often the most credulous. Thus a strong-minded man will sneeringly refuse the advice and assistance of a hospital surgeon for a wound or an ulcer, and trustingly consult an old soldier returned from the wars. And so also with such men as Thaer and Haller; it is not that they doubt the value of medical art and science, but rather the value of that experience which is so dogmatically thrust before the public by evidently incompetent persons. They have, in short, no certainty of many of the facts, and still more of the theories, and hence they despair of success, and practise without hope. This indicates a want of sound common sense and decision of character. The physician may be allowed to doubt, that he may the better arrive at truth, for, in fact, there can be no true faith where doubts have never entered.

In his remarks on the letter to Boerhaave, Dr. Mackness reverts to doctrines recently promulgated and advocated in this Journal, honoring us, (we are bound to say) by quoting us more than once.

"From nomenclature the letter proceeds to touch on practice, and our author acknowledges that, whilst there has been a departure from simplicity in the former, there has been an approximation to it in the latter; and he instances the greater simplicity in modern practice of prescriptions, of surgical treatment, and of obstetric practice, and proceeds to notice the greater prominence now given to moral and hygienic measures in medicine. Amongst the thinking part of the profession there is, it is well known, a feeling in favour of the adoption of a

rational system of hygiene and regimen in preference to active therapeutics. It is becoming better understood that many diseases run a natural course and terminate in a particular manner; and that the most that art can do is to watch their progress, and place the system in that position in which the malady is likely to terminate favorably." (p. 337.)

We believe there is no topic of medical ethics untouched in this volume; at least no important topic. The various questions of morals and manners which arise out of the biographical sketches, and out of the sentiments expressed in the letters, enable Dr. Mackness to be discursive, but finally comprehensive. We cannot undertake to indicate, even in a bare outline, the principal questions; our readers will, we are sure, be both interested and instructed by the volume, and we, therefore, recommend it to them, and strongly. Dr. Mackness has done a decided service to the profession in compiling this work. We trust he is only the pioneer to open the way to a still greater undertaking of the same kind.

While thus approving of his work generally, and with the most friendly feelings towards the author personally, we cannot quite forego that less agreeable task which forms part of our editorial duty, of noticing the literary demerits of the volume. We regret to say that it contains a good many inelegancies of style, and a considerable number of imperfections of translation. We observe errors even in those few portions of which we gave a translation in our review of the original work, and which we have reason to know gave satisfaction to the author with a solitary exception.*

As Dr. Mackness is a reader of this Journal, our notice could hardly have escaped him, and seeing that our version differed from his, he ought, we think, to have assured himself fully that his was correct. Some of Dr. Mackness's errors really misrepresent his author. Thus in the opening sentence of the letter to Stieglitz, Professor Marx is represented as saying: "Since the 31st of October 1840, when you bade farewell to these earthly scenes, not merely my writing-paper, but every day of my life has worn a mourning edge." But Professor Marx had no such connexion with Stieglitz as would warrant the use of black-edged paper, and, in fact, he did *not* use it, and says so. "Since the 31st, of October, 1840, when you said farewell to the earthly, my days, and not my writing-paper, have had a margin of mourning." We willingly allow that Professor Marx is not an easy writer to translate, but surely where the work was already done to Dr. Mackness's hand he might have appropriated it. Some of the expressions lose all their pith in the translation, and are even made ludicrous. For example, Dr. Mackness translates "Die Gedankenscheu ist so unheilbar wie die Wasserscheu," into "Bashfulness is as incurable as hydrophobia," and on this he hangs a comment, a quotation from Cowper, and an extract from this Journal. Yet Professor Marx never thought that bashfulness is so incurable. The aphorism should be rendered "the dread of thinking is as incurable as (hydrophobia) the dread of drinking." Another mistranslation is "physicians are born honorary members of all *human* societies," whereas it should have been, and as Dr. Mackness might have learnt from our translation, *humane* societies, the point having reference to societies instituted for the preservation of persons

* For "he who has no theory cannot be practical," we ought to have said, "—— is not *therefore* practical.

from death, as our own Royal *Humane* Society. Another example of incorrectness is one which concerns our own *métier*. Dr. Mackness translates an aphorism thus: "Reviewers are like the scourge, as the tails of comets, they frighten the weak, but do the strong no harm." Now the point in this aphorism is in likening reviews (not reviewers) to birch-rods, to which handy instruments of correction the tails of comets are not, in form, at least dissimilar, and not unlike in their operation on weak minds, in being considered scourges. The passage should have been, "reviews are just such rods as the tails of comets; they frighten the silly, but don't alarm the strong." Let some of our thin-skinned friends ponder this aphorism, and wince less when their faults are pointed out.

In addition to these actual errors, (which we have taken at random) we observe that several passages are so done into English as to read obscurely, or at least inelegantly. We subjoin one more example:

"For the dying there is an euthanasia, for the mourner a visit of condolence; but who concerns himself about the suffering physician? And yet he has most frequently to experience, that in bereavements the tears of survivors become like aquafortis to the soul, and that powerlessness to save others curdles, as it were, his own blood."*

There are two points here, the one is the play on *scheiden*, which signifies separation, and figuratively death, and *scheide*wasser, which is aqua-fortis used in etching;—the English verb to etch, being derived from the German aetzen, to corrode by acid or caustic. The other is in the word ohnmacht, or fainting; figuratively the heart bleeds from grief; fainting (the weakened heart) checks a hemorrhage, so the consciousness of *powerlessness*, the *literal* meaning of ohnmacht, may check the feelings of sorrow. The version we would suggest might run thus:

"There is heaven for the dying, and visits of condolence for the mourning survivors; but who troubles himself about the sorrowing physician? And yet the latter has the most frequently to experience and to feel that corroding death, with tears for aquafortis, etches sorrows on the soul; and that 'tis only a conviction of his powerlessness to save, that relieves his bleeding heart, as the powerlessness of a swoon checks the bleeding from a wound."

With such a form of expression, every practitioner will call to mind how deeply, when he has stood by the bed of a dying patient, he has felt his inability to save or even to relieve, and that his only consolation was a thorough confession of his utter helplessness.

This volume is affectionately dedicated by the author to his friend Dr. Archibald Robertson, of Northampton, in terms of the justest eulogy, as all who know him will avouch; a physician, "who, in his intercourse with his brethren and the public, has, during a long professional career, honorably exemplified the principles maintained in the following pages."

* "Für die Sterbenden hat man eine Euthanasia, für den Trauernden Condolenzbesuche; wer kümmert sich um den leidtragenden Artz? Und doch hat dieser am häufigsten zu erfahren und zu empfinden, dass beim Scheiden die Thränen zu Scheidewasser für die Seele werden, und dass die Ohnmacht das Bluten stillt." (Akesios, p. 21.)

ART. XII.

Lectures on the Comparative Anatomy and Physiology of the Vertebrate Animals, delivered at the Royal College of Surgeons of England, in 1844 *and* 1846. By RICHARD OWEN, F.R.S., Hunterian Professor and Conservator of the Museum of the College. Part I—*Fishes.* Illustrated by numerous wood-cuts.—*London,* 1846. 8vo, pp. 308.

THIS volume forms a portion of the promised continuation of the publication of Professor Owen's Hunterian Lectures; the first part of which, embracing the Comparative Anatomy of the Invertebrata, was noticed by us at the time of its publication in 1844. In one respect, however, this second volume has much the advantage over the first; for, whilst that was published from the notes of Mr. W. White Cooper, revised by Professor Owen, the continuation has been entirely undertaken by the Professor himself; and the result has been, that a large quantity of new matter has been added, the result of discoveries made since the delivery of the Course in 1844; whilst the views stated in that course have been substantiated and more fully developed by renewed examination of the data on which they were founded. Many details, moreover, have been added, which were necessarily omitted in the theatre, from the limited period allotted to the Lectures; so that the public are in every way great gainers by the delay, although they are yet in possession of only the first twelve Lectures of the course. The concluding volume will appear, we are led to hope, during the earlier half of the present year.

We need scarcely say that this work is one of the greatest interest and value. Of the vast opportunities at Professor Owen's command, he has most zealously availed himself; and we believe that we do but speak the general sense of competent judges when we say that, for comprehensive knowledge of the comparative anatomy of the vertebrate series, alike exact in its details and philosophical in its generalizations, he is certainly second to none, either living or dead. We should gladly take the opportunity of presenting to our readers a summary of the entire volume, in the hope of exciting their attention to its contents; but our limits compel us on the present occasion to confine ourselves to an exposition of Professor Owen's views on a question which possesses the greatest interest for the philosophical anatomist, and which should not be neglected by the anthropotomist who desires to acquire a real knowledge of the human fabric. We refer to the constitution of the vertebrate skeleton. No one can obtain the merest smattering of comparative anatomy without perceiving that mammals, birds, reptiles, and fishes, are so far constructed upon a common plan, as to agree in possessing an internal osseous skeleton, the essential part of which consists of the jointed bony casing that surrounds the spinal cord, and of the bony envelope that protects the encephalic mass. We find other parts of the skeleton deficient in the several tribes of vertebrata: thus the extremities, with the scapular and pelvic apparatus, are altogether wanting in the serpents, which moreover have no sternum; whilst the frogs and their allies are destitute of ribs, but have a sternum with well-developed anterior and posterior limbs, and scapular and pelvic arches. But we never find the vertebral column and cranial bones altogether want-

ing, except in those lowest fishes which retain the embryonic type so far as the development of the skeleton is concerned; and even in them we can trace the rudiments of the bony envelope to the nervous centres, in the membranous or cartilaginous tube which incloses the cerebro-spinal axis. But as we pursue our studies further, we find that although a cranium and vertebral column are thus constantly present, they exhibit considerable variations amongst different animals, not merely in regard to their form, but as to the number of parts of which they are made up. We find, for example, that the cranium of a cod has a vastly greater number of separate bones than that of man; and that the single vertebra of the human backbone is represented in the shark by a number of pieces held together by ligaments only. The comparative anatomist, who would duly systematize his study, will not be satisfied with bringing together all the diverse forms which he encounters in his progress through the vertebrated sub-kingdom; but he will endeavour, by patient scrutiny and careful comparison, to form an *ideal type* of a vertebra, which shall include all the parts that seem essential to its structure, and of which he may consider the several forms that present themselves to his attention as *variations*, resulting from want of development, from excessive development, or even (in some instances) from multiplication of certain of the elements.

This search after the "typical vertebra" has been made by several distinguished comparative anatomists, especially Carus and Geoffroy St. Hilaire; but the result of Professor Owen's inquiries is not precisely accordant with the conclusions of either of these distinguished physiologists, and seems to us much more satisfactory. There is a vagueness about Carus's conception of a vertebra, which must be apparent to any one who attempts to follow out his interpretation of the bones of the extremities, in which he uses the term "vertebra" in a sense almost equivalent to that of the more general term "bone" or "segment;" whilst, on the other hand, Geoffroy St. Hilaire has founded his idea of the vertebrate structure too exclusively upon that form of it which is presented in the fish, and has consequently included in it some elements which it should not really embrace, whilst he has neglected others which form legitimate parts of the vertebra in other classes.

Before proceeding, however, with the analysis of the vertebrate skeleton, we shall inquire, with Professor Owen, into its relations with the tegumentary envelope or dermo-skeleton, which is the usual characteristic of the invertebrata. The latter, of which we have the most remarkable example in the shell of the crustacean, is formed by the consolidation of the epidermis, and serves for the protection of the entire body; like the epidermis it is non-vascular and is not capable of interstitial growth; so that if it be not formed (as is the case with the shell of the echinus) of separate plates capable of being extended at their edges, or if it be not so shaped that additions made to its free borders give increased dimensions to the interior cavity (as is the case with the shells of mollusca, which form only a partial envelope to the body), it must be periodically thrown off and renewed, in order that it may be adapted to the augmentation of the body during the period of growth. Accordingly we find the "moulting" process almost universal in the articulated series, whose bodies and limbs are enveloped in a tightly-fitting integument. The contrast between the

skeletons of the vertebrated and invertebrated animals is most striking, when their respective relations to the nervous system are considered. In none of the latter does the nervous apparatus attain that predominance which it possesses in the former; we find its centres isolated from each other, and without any constant or determinate position. Thus the number and the relative situation of the ganglia varies in the mollusca with the development of the foot and of different portions of the muscular layer of the mantle, and with the position of the branchiæ; all of which conditions are subject to such variation, that scarcely any two genera are alike in the arrangement of their nervous centres. And in insects, crustacea, and other articulata, although the ganglia are usually disposed on a more uniform plan, their number varies with that of the segments of the body, and their relative size with that of the development of the parts of the muscular apparatus with which they are respectively connected. We nowhere encounter anything like the same fixity of plan as is shown in the disposition of the cerebro-spinal axis of vertebrata, from the very lowest to the highest of the group; nor does the entire mass of the nervous centres present in any case the same bulk relatively to that of the body, as it possesses in all vertebrata, excepting perhaps the very lowest of the series. In the invertebrata, accordingly, we find that the nervous system only receives the same amount of support and protection from the skeleton as the other tissues possess; for although it has been thought that certain internal projections of the dermo-skeleton of the insect and crustacean were specially adapted for this purpose, yet there is really no conformity between these and the number of ganglia, and they are so situated as to inclose the intercommunicating cords rather than the ganglia themselves; so that, as Professor Owen very justly urges, they must be regarded as essentially destined for the attachment of muscles; their relation to the nerve-trunks being accidental. On the other hand, the internal skeleton of the vertebrata, as we have seen, is essentially connected with the nervous system; the support and protection of the nervous centres being obviously its primary purpose; and the number of pieces of which the vertebral column (which constitutes its fundamental portion) is composed, being in constant relation with the number of pairs of nerves to be given off from the nervous axis. There is a similar relation between the functions of the nervous system, and those of the endo- and exo-skeletons respectively. When the powers of discerning hurtful agencies by the organs of sense, and of avoiding them by the use of the motor apparatus, are dull and contracted, the entire animal is protected by a hard insensible dermal armour; but as those powers become expanded and quickened, the body is disencumbered of its coat of mail, the skeleton is put inside and made subservient to muscular activity, and the skin becomes proportionally more susceptible of outward impressions of pleasure and pain. Thus, as Professor Owen justly remarks, "the exo-skeleton is the reflex of the circumambient medium and relations of the animal; the endo-skeleton is the index of its motive energies and its intelligence."

We need not waste our space in exposing the unphilosophical nature of the views of those, who have attempted to identify as "homologous" parts a segment of the thoracic dermo-skeleton of an articulated animal, and a thoracic segment of a vertebrated animal, including (with the vertebra) a

pair of ribs and a segment of the sternum. For although this identification has been advanced by authorities no less weighty than Geoffroy St. Hilaire and Carus, and although it may seem to derive support from a cursory comparison of the two structures, yet it totally fails, when the examination is pushed further, and is found to depend upon a relation of "analogy" merely, which results from an adaptation of each to a set of functions in some degree similar.*

The most convincing proof of the want of real conformity between the endo- and exo-skeletons, is their coexistence in no inconsiderable number of vertebrated animals. Thus among fishes, we find the lepidosteus and the ostracion entirely covered with a connected armour of dense enamelled bony scales; the substance of which, though formed by the calcification of the cutaneous tissues, presents the same organized structure as the bones of the internal skeleton. In the crocodiles, many of the dermal plates present almost the same complete ossification; and the tesselated armour of the armadillos and extinct glyptodons affords even in the mammalian class an example of the coexistence of a well-developed bony envelope, with a complete osseous internal skeleton. And among the highest invertebrata, we find somewhat of the same coexistence; the true homologue of the endo-skeleton presenting itself in the cuttle-fish as a cartilage supporting the cephalic ganglia; whilst the exo-skeleton of the testaceous mollusks is represented by the calcareous dorsal plate.

But there are certain hard parts, both in vertebrated and invertebrated animals, which cannot be referred to either of these divisions,—to the endo- or neuro-skeleton, or to the exo- or dermo-skeleton. Thus we find in the lobster, a calcified framework supporting the gastric teeth and giving attachment to the muscles that work them; and in the bulla there is a pair of calcareous plates imbedded in the walls of the muscular stomach or gizzard, which add considerably to their crushing and triturating power. A corresponding group of parts is to be found in the general skeleton of most vertebrated animals.

"The cartilages or bones of the larynx, trachea, and bronchi of air-breathing vertebrates, the bones and cartilages supporting the branchiæ in fishes and batrachians, the bones in the hearts of certain birds and mammals, are examples of the visceral series of hard parts, or the "splanchno-skeleton" as it has been termed by Carus; and very nearly and naturally connected with this primary division of hard and dry parts are those bones and gristles which form capsules or support the appendages of the special organs of the senses; as for example, the sclerotic osseous cups or plates of the eye, the petrous capsule of the labyrinth,

* We follow Professor Owen in his employment of the terms *homology* and *analogy*, as expressive of relations essentially distinct. Homologous organs are those which correspond with each other in position, connexions, and development, although they may differ in function; thus, the lungs of air-breathing vertebrata and the swimming-bladder of fishes are indubitably homologous, although the latter has a function almost invariably distinct from that of the former. This relation has been before expressed by the terms "essential" or "fundamental analogy;" but there is a great advantage in the use of the simple term "homology." On the other hand, "analogous" organs are those which have similar functions in different animals, or in different parts of the same animal, although they may be formed of elements really dissimilar. Thus, the wings of bats, birds, flying-fish, and pterodactyles are analogous organs, and are so far homologous as being supported by some modification of the bones of the anterior extremity; but though analogous, they are not homologous with the wings of the Draco volans, which are expanded over an extension of the false ribs; and still less are they homologous with the wings of insects, which are supported by prolongations of the dermo-skeleton.

the ossicles and cartilages of the tympanum and external ear, the turbinate bones and gristles of the nose. But some of these "sense-capsules" are connected and intercalated with the true bones of the endo-skeleton, and subservient to similar functions, besides their own special uses; so that they are generally described as ordinary bones of the skull. As in all arrangements of natural objects, where nature is followed in selecting their characters, so in classifying the parts of the general skeleton of vertebrata, the primary groups blend into one another at their extremes, and make it difficult to draw a well-defined boundary line between them. Thus, the hyoids and branchial arches closely resemble one another in fishes. Bones of the dermo-skeleton combine with those of the endo-skeleton to form the opercular and the single median fins. But we must not on that account abandon the advantage of arrangement and classification, in acquiring an intelligible and tenable knowledge of a complex system of organs, when typical characters clearly indicate the general primary groups. Clearly appreciating the existence of such characters in the very numerous and diversified parts of the general skeleton of the vertebrate animals, I therefore adopt the primary division of those parts into endo-skeleton, exo-skeleton, and splanchno-skeleton." (p. 24.)

It must be carefully borne in mind that the presence of *bone* is not requisite to indicate the existence of a constituent part of the skeleton. There are many instances in which parts that are ossified in some animals are cartilaginous or merely fibrous in others. Thus every intelligent anatomist is aware that the costal cartilages of the mammalia are ossified in birds, so as to form sternal ribs; and that white lines across the recti abdominis represent the abdominal ribs of reptiles. In the lower cartilaginous fishes, such as the lamprey and myxine, no part of the endo-skeleton advances beyond the cartilaginous grade; and in the amphioxus or lancelet it is entirely fibrous. In regard to the composition of bone, we may remark, that the recent analyses of Von Bibra and Dr. Stark indicate that the proportions of animal and mineral matter do not vary so widely in the bones of different animals, or in those of young and old individuals, as was formerly supposed; the difference in texture being principally due to the quantity of membranous and adipose matter in the canals and cancelli of bone (forming no part, however, of the true osseous texture) and to the variable proportion of water. It is on this last condition that the relative softness of the bones of the sharks and rays depends; for although they are commonly ranked as cartilaginous fishes, they are not so in reality; their bones, whilst soft and flexible, being but little different in composition from the densest parts of the skeleton of higher animals, except in losing a much larger proportion of their weight by drying, and in containing a greater proportion of the soluble salts of soda.

After describing the general nature, chemical constitution, development, growth, and structure of the osseous system, Professor Owen next proceeds to define *a bone;* the endeavour to do which, he remarks, has not been the least difficult part of his task, with reference to the applicability of the definition to the vertebrate series in general.

"To the human anatomist the question—what is *a bone?*—may appear a very simple, if not a needless one; he will most probably reply that *a bone is any single piece of osseous matter entering into the composition of the adult skeleton;* and, agreeably with this definition, he will enumerate about 260 bones in the human skeleton. Soemmering, who includes the thirty-two teeth in his enumeration, reckons from 259 to 264 bones; but he counts the os spheno-occipitale as a single bone, and also regards, with previous anthropotomists, the os temporis, the os

sacrum, and the os innominatum as individual bones; the sternum, he says, may include two or three bones, &c.: but, in birds, the os occipitale is not only anchylosed to the sphenoid, but these early coalesce with the parietals and frontals; and, in short, the entire cranium consists, according to the above definition, of a single bone. Blumenbach, however, applying the human standard, describes it as composed of the proper bones of the cranium consolidated, as it were, into a single piece. And in the same spirit most modern anthropotomists, influenced by the comparatively late period at which the sphenoid becomes anchylosed to the occipital in man, regard them as two essentially distinct bones. In directing our survey downwards in the mammalian scale, we speedily meet with examples of persistent divisions of bones which are single in man. Thus it is rare to find the basi-occipital confluent with the basi-sphenoid in mammalian quadrupeds; and before we quit that class, we meet with adults in some of the marsupial or monotrematous species, for example, in which the supra-occipital, 'pars occipitalis propriè sic dicta' of Soemmering, is distinct from the condyloid parts, and these from the basilar or cuneiform process of the os occipitis; in short, the single occipital bone in man is four bones in the opossum or echidna; and just as the human cranial bones lose their individuality in the bird, so do those of the marsupial lose their individuality in the ordinary mammalian and human skull. In many mammalia we find the pterygoid processes of anthropotomy permanently distinct bones; even in birds, where the progress of ossific confluence is so general and rapid, the pterygoids and tympanics, which are subordinate processes in man, are always independent bones. In many mammalia, the styloid, the auditory, the petrous, and the mastoid processes remain distinct from the squamous or main part of the temporal throughout life; and some of these claim the more to be regarded as distinct bones, since they obviously belong to different natural groups of bones in the skeleton; as the styloid processes, for example, to the series of bones forming the hyoidean arch. The artificial character of that view of the os sacrum, in which this obviously more or less confluent congeries of modified vertebræ is counted as a single component bone of the skeleton, is sufficiently obvious. The os innominatum is represented throughout life in most reptiles by three distinct bones, answering to the iliac, ischial, and pubic portions in anthropotomy. The sternum in most quadrupeds consists of one more bone than the number of pairs of ribs which join it; thus it includes as many as thirteen distinct bones in the *Bradypus didactylus*." (pp. 36-7.)

From these and numerous similar facts, we are led to see the arbitrary character of any definition of a bone founded upon the composition of the skeleton in any single animal, the complex nature of many of the so-called single bones of man, and the real independence of many of those parts which are only ranked as "processes" in anthropotomy. We further see that it is only by a comparative examination of *all* the forms presented by the vertebrate skeleton, that we can learn what are to be regarded as really individual bones, and what modifications they undergo in the human subject. That a fundamental unity prevails throughout, is obvious not only from the gradation which may be traced between forms apparently the most diverse, but also from the fact that the similarity among all becomes much greater when the comparison is instituted at an early period of development. Thus in the human fœtus the expanded portion of the occipital bone is ossified from four distinct centres, which correspond to the four permanently distinct bones of the marsupials and reptiles; the pterygoid processes have distinct centres of ossification; the styloid and mastoid processes, and the tympanic ring, are separate parts in the fœtus; the constituent vertebræ of the sacrum remain distinct to a later period;

and the ilium, ischium, and pubis are still later in anchylosing to form the os innominatum. So strongly impressed was Cuvier with the importance of attention to the osteogenic process, in determining the true composition of the skeleton, that he goes so far as to assert that, in order to ascertain the true number of bones in each species, we must descend to the primitive osseous centres as they are manifested in the fœtus. According to this rule, we ought to count the humerus as three bones, and the femur as four bones, instead of one; for the ossification of the latter begins at four distinct points,—one for the shaft, one for the head, one for the great trochanter, and one for the distal condyles. But there is no such distinction in any of the lower classes; for in both birds and reptiles the femur is developed from a single centre. The rule laid down by Cuvier would greatly mislead us therefore, if rigidly applied; and it is necessary to distinguish, as Professor Owen justly points out, between those centres of ossification which have *homological* relations, and those that have only *teleological* ones: i. e. between the separate points of ossification of a human bone which typify permanently distinct bones in the lower animals, and the separate points which, without such signification, facilitate ossification, and have for their final cause the well-being of the growing animal. The contrast between these relations is well seen in the following examples.

"The young lamb or foal can stand upon its four legs as soon as it is born; it lifts its body well above the ground, and quickly begins to run and bound. The shock to the limbs themselves is broken and diminished at this tender age by the divisions of the supporting long bones,—by the interposition of the cushions of cartilage between the diaphyses and the epiphyses. And the jar that might affect the pulpy and largely-developed brain of the immature animal, is further diffused and intercepted by the epiphysial articular extremities of the bodies of the vertebræ. We thus readily discern a final purpose in the distinct centres of ossification of the vertebral bodies, long bones, and the limbs of mammals, which would not apply to the condition of the crawling reptiles. The diminutive brain in these low and slow cold-blooded animals does not demand such protection against concussion; neither does the mode of locomotion in the quadruped reptiles render such concussion likely; their limbs sprawl outwards, and push along the body, which commonly trails upon the ground; therefore we find no epiphyses with interposed cartilage at the ends of a distinct shaft in the long bones of saurians and tortoises. But when the reptile moves by leaps, then the principle of ossifying the long bone by distinct centres again prevails, and the extremities of the humeri and femora long remain epiphyses in the frog.

"A final purpose is no doubt also subserved in most of the separate centres of ossification which relate homologically to permanently distinct bones in the general vertebrate series; it has long been recognized in relation to facilitating birth in the human fœtus; but some facts will occur to the human osteogenist, of which no teleological explanation can be given. One sees not, for example, why the process of the scapula which gives attachment to the pectoralis minor, the coracobrachialis, and the short head of the biceps, should not be developed by continuous ossification from the body of the blade-bone, like that which forms the spinous process of the same bone. It is a well-known fact, however, that not only in man, but in all mammalia, the coracoid process is ossified from a separate centre. In the monotremes it is not only distinct, but is as large a bone as in birds and reptiles, in which it continues a distinct bone throughout life. Here, then, we have the homological, without a teleological explanation of the separate centre for the coracoid process in the ossification of the human blade-bone." (pp. 38-9.)

The distinction here first pointed out by Professor Owen is of primary importance, and must be kept in view in all our attempts at a philosophical determination of the essential parts of the vertebrate skeleton; for without such a guide we shall be continually thrown aground—as past philosophical anatomists have been—in the comparison of the bones of animals in which the skeleton seems most complex (owing to the great number of pieces of which it is composed), with those of animals in which (owing to the much smaller amount of distinct pieces) it appears to be most simple. There are certain bones which are *simple*, as being developed from a single centre through the entire series, and the determination of the homologies of these presents no difficulty; but the representatives of the *compound* bones, or of those which are developed from two or more separate centres, cannot be always found in groups of simple bones. For if these bones are only *teleologically compound*,—that is, if their development from separate centres has reference only to the special exigencies of a particular animal or class, as is the case with the cylindrical bones in the mammalia,—we shall find them elsewhere represented by simple bones. Indeed we may even find a group of bones remaining permanently distinct, as that which composes each half of the lower jaw in fishes and icthyoid reptiles, and yet represented elsewhere by a simple bone; the teleological purpose or final cause having required this subdivision in a particular tribe, whilst no traces of it are to be found elsewhere. On the other hand, the *homologically compound* bones are those which, like the occiput, scapula, vertebræ, and sacrum, are developed from separate centres, which represent permanently distinct simple bones in other vertebrata; and thus their relations extend over the whole vertebrate series. The comparison on which we shall presently enter, between the vertebrate skeleton of fishes and that of man, will afford us examples of both these species of relation.

In order to understand the fundamental type of the vertebrate skeleton, we must commence its study,—not in that form of it in which there is the greatest heterogeneousness of character, on account of the variety of purposes to which it has to be rendered subservient,—but in the class in which vegetative uniformity most prevails, and the primitive type is least obscured by teleological adaptations. Such conditions are best displayed in the skeletons of fishes; in which, although they are apparently the most complex (from the number of distinct pieces of which they are usually composed), we find a greater real simplicity, since these pieces are for the most part the *simple* bones, by the union of which, under various forms, the *homologically compound* bones of higher animals are made up. Moreover, it is much more easy to detect in this class the intercalation of osseous pieces from the dermo- and splanchno-skeletons among the parts of the neuro-skeleton; all traces of which are frequently lost, if we confine our attention solely to the skeletons of air-breathing vertebrata. But, as Professor Owen justly observes, fishes form only one branch of the vertebrate stem, which, like other primary branches, ramifies in diverging from the common trunk; and we should miss our aim, and be led astray from the detection of the true general type of the vertebrate skeleton, were we to confine our observations to fishes alone, which have teleological adaptations and other peculiarities of their own.

"A comparison of their skeletons with those of the higher classes teaches that the natural arrangement of the parts of the endo-skeleton in vertebrata, like that of the exo-skeleton in articulata, is in a series of segments succeeding each other in the axis of the body. I do not find these successive segments composed of precisely the same number of bones in all vertebrata; rarely, indeed, in the same animal. Yet certain constituent parts of each segment do preserve such a constancy in their existence, relative position, and offices throughout the body, as to enforce a conviction that they are homologous parts, both in the consecutive series of the same individual skeleton, and throughout the entire series of vertebrate animals. To each of these primary segments of the skeleton I shall, with Geoffroy St. Hilaire, apply the term 'vertebra:' the word may seem to the anthropotomist to be used in a different or more extended sense than it is usually understood; yet he is himself, unconsciously, perhaps, in the habit of including in certain vertebræ of the human body elements which he excludes from the idea in other natural segments of the same kind; influenced by differences of proportion and coalescence, which are the most variable characters of a bone. Thus the rib of a cervical vertebræ is the 'processus transversus perforatus,' or the 'radix anticus processus transversi vertebræ colli;' whilst in the chest, it is 'costa' or 'pars ossea costæ.' But the ulna is not less an ulna in the horse, because it is small and anchylosed to the radius." (pp. 41-2.)

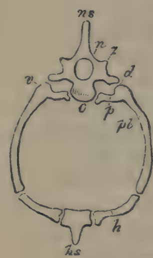

Fig. I. Segment of endo-skeleton, *mammal.*

A vertebra, then, according to Professor Owen's definition, is *one of those segments of the endo-skeleton which constitute the axis of the body, and the protecting canals of the nervous and vascular trunks;* such a segment may also support *diverging appendages.* Now as all hope of detecting the true homologies of the vertebræ in the assemblage of bones which form the cranium, and of identifying the really homologous parts in animals that are formed upon plans greatly dissimilar,—still more, of ascertaining the fundamental character of the extremities, depends upon a true determination of the real elements of a vertebra, this point is one of essential importance. The following is Professor Owen's account of the composition of a complete typical vertebra, excluding the diverging appendages:

c. A body or *centrum.*
n. Two *neurapophyses.*
p. Two *parapophyses.*
pl. Two *pleurapophyses.*
h. Two *hæmapophyses.*
ns. A *neural spine.*
hs. A *hæmal spine.*

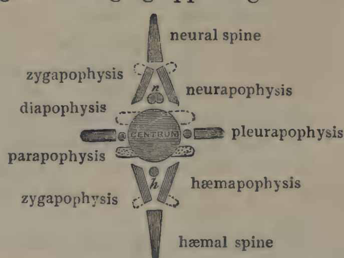

Fig. II. Ideal typical vertebræ.

"These, being usually developed from distinct and independent centres, I have termed 'autogenous' elements. Other parts, more properly called processes, which shoot out as continuations from some of the preceding elements, are termed 'exogenous;' e. g. (*t*) the diapophyses or upper 'transverse processes,' and (*z*) the zygapophyses, or the 'oblique' or 'articular processes' of human anatomy.

"The autogenous processes generally circumscribe holes about the centrum,

which, in the chain of vertebræ, form canals. The most constant and extensive canal is that (Fig. 2, *n*) formed above the centrum, for the lodgment of the trunk of the nervous system (neural axis) by the parts thence termed 'neurapophyses.' The second canal (Fig. 2, *h*) below the centrum, is in its entire extent more irregular and interrupted; it lodges the central organ and large trunks of the vascular system (hæmal axis), and is usually formed by the laminæ, thence termed 'hæmapophyses.' At the sides of the centrum, most commonly in the cervical region, a canal (Fig. 3, *v*) is circumscribed by the pleurapophysis or costal process (Fig. 3, *pl*), and by the diapophysis or upper or transverse process (Fig. 3, *t*), which canal includes a vessel, and often also a nerve." (p. 43.)

The mode in which these elements are arranged in the thoracic vertebræ of mammalia, will be seen by reverting to the first figure, in which are marked the neural spine or spinous process (*ns*), the neurapophyses or neural arches (*n*), the zygapophyses, or oblique processes (*z*), the diapophyses or upper transverse processes (*d*), and the centrum or body (*c*), of which the mammalian thoracic *vertebra* is commonly regarded as consisting; and in addition we have the parapophyses or anterior roots of the the hæmapophyses (*h*) represented by the *costal cartilage;* and the hæmal spine (*hs*) by the *sternum*, which, though usually flat in the mammalia, often presents a projecting keel as in birds. The great enlargement of the hæmal arch is here due to the increased development of the pleurapophyses; transverse processes (*p*), and the pleurapophyses (*pl*), making up the *ribs;* and by the removal of the hæmapophyses from the centrum, and their articulation with the distant ends of the pleurapophyses. Besides the neural and hæmal canals, above and below the centrum, we find the lateral vascular canals, included between the origin of the rib and the included between the transverse process, which in the cervical region are two roots of the transverse process itself, of which the inferior one is obviously the pleurapophysis here anchylosed to the body of the vertebra.—In the cervical vertebra of a bird, we find another arrangement of these elements; the hæmapophyses being anchylosed to the under part of the centrum, and the hæmal canal being only required for the protection of the carotid arteries. In the cranium, we shall find the chief modification to result from the expansion of the neural arch to form the brain case, just as the hæmal arch in the thoracic region is expanded to include the heart and lungs in the thoracic region.

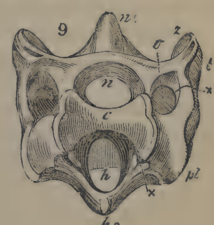

Fig. III. Natural typical vertebra from the neck of a Pelican.

We shall not follow Professor Owen through his description of the vertebral column in fishes; but shall only observe that he clearly demonstrates the fallacy into which Geoffroy St. Hilaire was led, when he adopted the vertebra of the fish as the type of vertebral structure in general; the hæmapophyses being always absent or unossified, the parapophyses being frequently so modified as to inclose the hæmal canal; whilst, on the other hand, certain bones are superadded, which have an apparent claim to be ranked among the elements of the vertebræ, but which really belong to the dermal skeleton. Thus we find the dorsal fins of osseous fishes supported by a row of bones superposed upon the ordinary neural spines, and imagined by Geoffroy St. Hilaire to be formed by the change in the position of the two lateral halves, which were in his view placed one on

the top of the other, instead of meeting on the median line. But it is now generally admitted that these spines in reality belong to the dermo-skeleton, although they are closely related in position and aspect to the real neural spines of the vertebræ.* But there are other projections from the vertebræ of certain fish, which constitute "diverging appendages" of the vertebra itself; such are certain spines, which are to be met with in the salmon and herring, the mackerel tribe and the dolphin, projecting from the ribs near their heads and sometimes diverging from the parapophyses and even from the neurapophyses. The homologue of these diverging appendages may be distinctly recognized in the ribs of birds; each of which, as is well known, is furnished with a process that passes over the ribs next below, and serves to give additional firmness and compactness to the bony case. We shall presently find that the recognition of them in the cranial vertebræ of fishes conducts us, according to Professor Owen, to the true homology of the bones of the extremities.

The fact that the spines which support the dorsal fin, constituting a second row of greater or less extent above the true neural spines, belong to the dermo-skeleton, is extremely well seen in the sturgeons, which have a well-developed osseous endo-skeleton coexisting with a covering of hard enamelled calcareous plates; and to this tribe the philosophical anatomist finds it requisite to make frequent reference, for the determination of the parts that really belong to each division. Here we find the rays upon which the dorsal fin is supported, clearly developed from the dermal plates, which along the middle line of the back shoot upwards and backwards a moderately long spine. From the base of these dermal spines, other spines usually shoot downwards into the intervals of the neural spines; these inverted interneural spines, which are double in the flat-fish, appear to be regarded by Professor Owen as formed by the "vegetative repetition" of the neural spines themselves; but we must take leave to question this determination, for it seems to us much more natural to consider them as portions of the dermo-skeleton passing inwards,—the manner in which they are intercalated among the true neural spines bearing a very strong resemblance to the reception of the fangs of the teeth into the alveolar processes of the jaw. It is not only in the back that we find these additional parts derived from the dermo-skeleton; for just as in the framework of the dorsal fin we find interneural spines and dermoneural spines, so in that of the anal fin we recognize interhæmal spines and dermohæmal spines. The framework of the caudal fin is composed of similar intercalary and dermal spines, superadded to the proper neural and hæmal spines of the proper caudal vertebræ, which have coalesced and been shortened by absorption, in the progress of embryonic development, to form the base of the terminal fin. There is usually an exact correspondence in intimate structure, between these dermal spines and the real bones which support them; this conformity has been urged by Professor Agassiz as an argument against the possibility of drawing a valid distinction in such cases between the parts belonging to the neuro-skeleton and those which appertain to the dermal envelope. But such a conformity exists likewise between the undoubted tegumentary scales of the lepidosteus

* In the herring, moreover, we find a sort of sternum formed by dermal plates, which are articulated to the ends of the ribs.

and its true internal bones; so that the presence of true bony structure must not be regarded as of itself proving that the part which exhibits it belongs to the neuro-skeleton; this being only determinable by its connexions and by the history of its development.

We shall now follow Professor Owen, as briefly as may be consistent with clearness, through his view of the craniology of the osseous fishes; in which group, owing to the permanent distinctness of a large proportion of the elements of which the skull is composed, the vertebral structure may be recognized more clearly than in any other.

"The bones of the skull are primarily divided in anthropotomy into those of the cranium and those of the face; but the proportions which these divisions bear to one another in man are reversed in fishes. According to this binary classification, the facial series in fishes includes an extensive system of bones—the hyoid—of which part only, viz., the 'styloid element,' is admitted into the skull by the anthropotomist, who describes it as a process of the 'temporal bone.' This very 'temporal,' moreover, is originally and essentially an assemblage of bones, which are always distinct in fishes and reptiles; and the squamous part, which enters so largely into the formation of the cranial cavity in man and most mammals, has no share in its formation in the lower vertebrata. The two classes of cranial and facial bones having been originally founded upon the exclusive study of the most peculiarly and extremely modified skull in the whole vertebrate series—that of man,—their characters, as might be expected, are artificial, and applicable to the same bones in only a small proportion of the vertebrata; the unity of the plan pervading the organization of which, it is the business of the anatomist, properly so called, to demonstrate.

"The bones of the skull of fishes are primarily divisible into those of

A. Neuro-skeleton;
B. Splanchno-skeleton;
C. Dermo-skeleton.

"The bones of the neuro- or proper endo-skeleton are arranged here, as in the rest of the body, in a series of horizontally succeeding segments; each segment consisting of an upper (neural) and a lower (hæmal) arch, with a common centre, and with diverging appendages. As the bones respectively entering into the formation of these segments are the same in relative position, and nearly the same in number, as in the typical vertebræ of the trunk—the excess arising from subdivision of the peripheral elements—the same term ought to be extended to those cranial segments which has been usually restricted to their neural arches, in which the typical characters of the vertebra are least departed from." (pp. 86-8.)

Before proceeding with the enumeration of the parts entering into the composition of the cranial vertebræ, we must stop to notice that Professor Owen fixes the number of these vertebræ (which has been a subject of much discussion) at four; and that he justifies this by reference to the encephalon, which consists of four primary divisions, succeeding each other horizontally in a linear series. These are,—1, the medulla oblongata, with the superimposed cerebellum, recently termed by Vogt the *epencephalon;* 2, the third ventricle, with its upper (pineal) and lower (hypophysial) prolongations, and the superimposed optic lobes, or the *mesencephalon;* 3, the cerebrum proper, or *prosencephalon;* and 4, the olfactory ganglionic prolongation of the cerebrum, or *rhinencephalon.* Though subject to various degrees of anchylosis, the cranial vertebræ of fishes always agree in number with these primary divisions of the encephalon, and are named by Professor Owen in accordance with them.

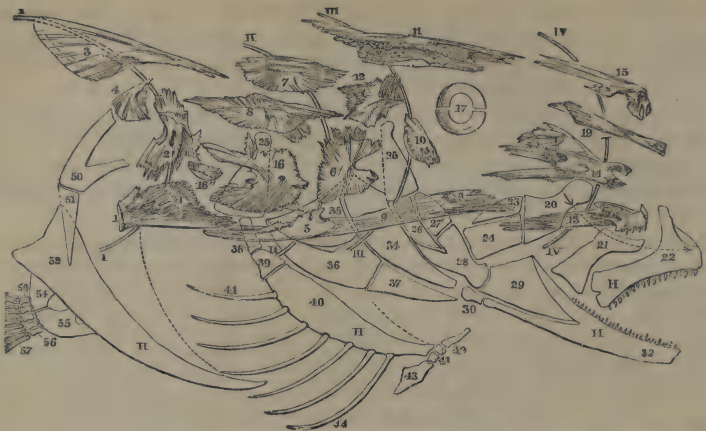

Fig. IV. Disarticulated bones of the cranial vertebræ, and sense-capsules, in Cod-fish; the hæmal arches (H, H) and appendages in diagrammatic outline.

"Each cranial vertebra, or natural segment of the skull, is divided into a *neural arch*, with which the centrum and parapophyses are always more immediately connected; and a *hæmal arch*, with its appendages.

"The neural arches are:
I. Epencephalic arch (Figs. IV and V, 1, 2, 3, 4);
II. Mesencephalic arch (Figs. IV and VI, 5, 6, 7, 8);
III. Prosencephalic arch (Figs. IV and VII, 9-12);
IV. Rhinencephalic arch (Figs. IV and VIII, 13-15).

"The hæmal arches are:
i. Scapular, or scapulo-coracoid (Fig. IV, in outline, 50-52);
ii. Hyoid or stylo-hyoid (Ditto, 25, 38-43);
iii. Mandibular or tympano-mandibular (Ditto, 25-32);
iv. Maxillary, or palato-maxillary (Ditto, 20-22).

"The appendages of the hæmal arches are:
1. The pectoral (Fig. IV, in outline, 54-57);
2. The branchiostegal (Ditto, 44);
3. The opercular (Ditto, 34-37);
4. The pterygoid (Ditto, 23-24).

"The bones of the *splanchno-skeleton* constitute:
The ear-capsule or petrosal, and otolite (Fig. IV, 16, 16′);
The eye-capsule or sclerotic, and pedicle (Ditto, 17);
The nose-capsule or æthmoid, and turbinal (Ditto, 18-19);
The branchial arches (Ditto, 45-49).

"The bones of the *dermo-skeleton* are:
Supra-temporals (Fig. IV, 71);
Supra-orbitals (Ditto, 72);
Sub-orbitals (Ditto, 73, 73′);
Labials (Ditto, 74)." (pp. 88-9.)

We shall now examine each of these divisions separately. In most osseous fishes, the bones encompassing, or in vertebral relation with, the epencephalon, and thus forming the neural arch of the first or occipital vertebra, are six in number, as shown in Fig. V. These are called, in

ichthyology, the basi-occipital (1), the ex-occipitals (2, 2), the supra-occipital (3), and the par-occipitals (4, 4). No one can have any difficulty in recognizing in the basi-occipital the centrum or body of a vertebra, since it articulates posteriorly with the body of the atlas; and bearing in mind the relations of these bones to the nervous cord, it is obvious that the ex-occipitals are the neurapophyses, the par-occipitals the parapophyses, and the supra-occipital (which very frequently possesses a prominent keel or ridge superiorly) the neural spine. Among the variations in form presented by these bones in different members of the class, are some which indicate in a very striking manner their conformity to the vertebral elements of the spinal column. On the other hand, various degrees of anchylosis are met with, which, by uniting two or more pieces, apparently reduce the number of parts of the vertebra; and in the polypterus the elements of the neural arch are all united into one piece, which corresponds to the occipital bone of man. In the early condition of the latter, we find seven ossific centres; one for the basilar portion or body of the vertebra, two for the condyles or neurapophyses, two below the crucial ridge for the parapophyses, and two above for the triangular portion which forms the summit of the bone and represents the neural spine. The supra-occipital, its homologue in fishes, is itself divided by a median suture in the lepidosteus, showing that it is in like manner formed from two centres of ossification.

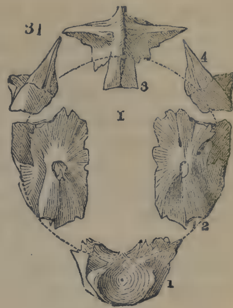

Fig. V. Disarticulated epencephalic arch, viewed from behind, in the Cod.

The neural arch of the second cranial vertebra surrounding the mesencephalon, is composed of seven bones, of which the parietal elements in the human cranium are the largest, hence this vertebra is termed the parietal vertebra. Its body or centrum is formed by the *basi-sphenoid* (Fig. VI, 5); its neurapophyses are the bones termed the *ali-sphenoids* (6); its parapophyses are the *mastoid* bones (8); whilst its spine is formed by the *parietals* (7), which in the fish are comparatively small, in accordance with the small size of the encephalon, whose upper portion they are to protect. The names given to these bones sufficiently indicate the parts of the human cranium with which they are homologous. The basi-sphenoid is always united by continuous ossification with the *pre-sphenoid* (9), which is considered by Professor Owen as the centrum of the third or frontal vertebra; and the fact that the whole basi-pre-sphenoid is developed from a single centre of ossification is shown by him not to afford any sufficient objection to this homology, since many other cases exist in which bones that are elsewhere undoubtedly distinct, are in like manner repre-

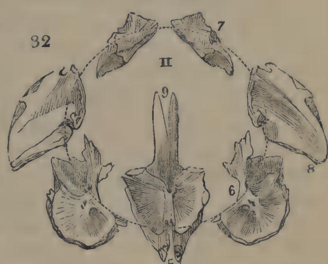

Fig. VI. Disarticulated neural arch of parietal vertebra, viewed from behind; from the Cod.

sented by a single bone developed from one ossifying centre. The elements of the occipital and parietal vertebræ are so formed as to leave a large cavity, or *otocrane*, for the lodgment of the proper acoustic capsule; this cavity, which is analogous to the orbital cavity for the lodgment of the eye, is excavated in the ex-occipital, par-occipital, ali-sphenoid, and mastoid bones, with the addition in some instances of the parietal and supra-occipital. The acoustic capsule is either cartilaginous or osseous; when in the latter state it is known as the *petrosal* bone; and although it coalesces with the elements of the neuro-skeleton in higher animals to form the temporal bone, yet we think that Professor Owen is perfectly justified in regarding it as in itself a portion of the splanchno-skeleton, like the sclerotic capsule of the eye.

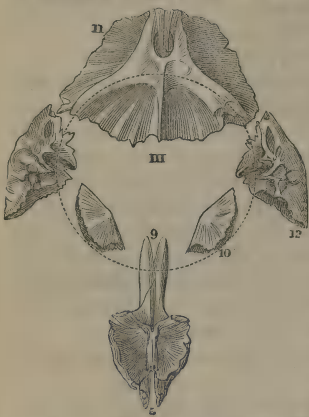

Fig. VII. Disarticulated neural arch of frontal vertebra, viewed from behind; from the Cod.

The neural arch of the third or frontal vertebra, which surrounds the prosencephalon of fishes, has for its centrum (as already stated) the pre-sphenoid (Fig. VII, 9); its neurapophyses are the *orbito-sphenoids* (10), whose essential functions are the protection of the sides of the prosencephalon, and the transmission of the optic nerve; the *post-frontals* (12) form its parapophyses; whilst the *frontal* (11), which is oftener divided by a median suture than a single bone, obviously constitntes its spine. We thus see the exceedingly complex nature of the human sphenoid bone; since, independently of the pteregoid processes, whose representatives have not yet come before us, the upper portion entering into the walls of the cranial cavity is represented in the fish by the basi-pre-sphenoid, the ali-sphenoids, and the orbito-sphenoids, which enter into the composition of two distinct vertebræ. All these are anchylosed into one bone in the polypterus.

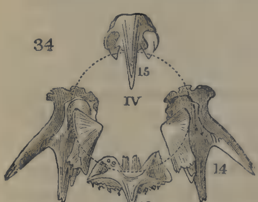

Fig. VIII. Disarticulated neural arch of the nasal vertebra, viewed from behind; from the Cod.

The circle of bones which completes the axis of the skull anteriorly, and protects the olfactory ganglia, may be regarded as the neural arch of the fourth or nasal vertebra. Its body, however strange this may appear to the mere anthropotomist, formed by the *vomer* (Fig. VIII, 13); which, instead of being a narrow plate that occupies scarcely any space on either side of the median plane, is here a broad thick bone, whose aspect presents no difficulty in its recognition as the centrum of a vertebra. The neurapophyses are formed by the *prefrontals* (14), which defend and support the olfactory prolongations of the cerebral axis, and bound the orbits anteriorly; and the spine is formed by the *nasal* bone (15), which is usually

single. The parapophyses are not present as distinct elements in this vertebra. The elements of this nasal vertebra are closely connected with the capsule of the organ of smell, which is represented in man by the æthmoidal and turbinal bones. The former is seldom completely ossified even in the osseous fishes; and it often retains its original cartilaginous condition. By Oken and Bojanus, who regarded the cranium as made up of only three vertebræ, the æthmoid was regarded as the centrum or body of the third or anterior vertebra. This view might appear justifiable, when we look merely to the form and position of this bone in the higher vertebrata; but a more comprehensive examination shows that it forms the anterior wall rather than the floor of the cranium; and that it is related rather to the protection of the olfactory organ, than to the support of the olfactory ganglia, although these sometimes rest upon it as in man. The existence of other sense-capsules as parts of the splanchno-skeleton, affords an obvious reason for regarding the boundary of the olfactory organ in the same light.

We have thus endeavoured to explain the principal features of Professor Owen's determination of the homologies of the neural arches of the cranial vertebræ; and we feel a very strong conviction that it is the most philosophical that has yet been offered. It is founded upon the relation of the cranial envelopes to the nervous centres and nerves proceeding from them; and the slightest acquaintance with the anatomy of the encephalon in fishes enables us to see that the number of its principal segments is four. It is very interesting to perceive that this number corresponds with that of the parts entering into the composition of the cephalic ganglia in the myriapoda; and that the number of Professor Owen's cranial vertebræ is identical with the number of segments detected by Mr. Newport in the cephalic portion of the head of those animals. (See Brit. and For. Med. Rev., Vol. XX, p. 493.) The determination of the complex system of bones forming the remainder of the skull of the fish, and including those which are subservient to the respiratory process, is much more difficult; and it must be entirely guided by that of the bodies and neural arches of the cranial vertebræ. Our space will only admit of our indicating very briefly the results of Professor Owen's inquiries on this point.

The *hæmal* arches, like the neural, are four in number; and are connected with the lower portions of the neural arches. The most anterior appertaining to the nasal vertebra, is the *palato-maxillary* arch; of which the *palatines* (Fig. IV, 20) constitute the pleurapophyses, the *maxillary* (21) the hæmapophyses, and the *intermaxillary* or *premaxillary* (22) the hæmal spine. This arch has a "diverging appendage" consisting of the *pteregoid* (24) and *ento-pteregoid* bones (23); which are, however, by no means constantly present. The ten bones of which the palato-maxillary arch is made up in osseous fishes are very commonly so disposed as to appear like three parallel and independent arches, successively attached behind one another by their keystones to the forepart of the axis of the skull, and with their piers or crura suspended freely downwards and outwards. But this arrangement seems to have reference to the peculiar mobility which the lips of fish, in the absence of other prehensile organs, require to possess; and the existence of only one true arch in this series of palato-maxillary bones seems to be indicated by its simple condition in the lepi-

dosiren and in plagiostomous fishes, as well as by the fact that it is completed or closed inferiorly at one point only, viz., where the premaxillaries meet and coalesce.

The *tympano-mandibular* arch presents its true inverted or hæmal character; its apex or keystone being formed by the symphysial junction of the lower jaw hanging downwards freely below the vertebral axis of the skull. It is usually, however, very complex in its construction; each of the vertebral elements being here repeated or subdivided, so as to be made up by many distinct bones. Thus the pleurapophyses are represented by the *epi-tympanics* (Fig. IV, 25), the *meso-tympanics* (26), the *pre-tympanics* (27), and the *hypo-tympanics* (28); but even within the class of fishes we find a tendency to simplification by the coalescence of the four bones on each side into two; and there seems no difficulty in regarding this complicated tympanic pedicle of the lower jaw as a "serial repetition" of the same element which, as the pedicle of the upper jaw, forms the single palatine bone of either side. The *mandible* or lower jaw consists of two principal portions on each side; of which the one that is articulated to the suspensory pedicle (29) represents the hæmapophysis; whilst the anterior portion (32) in which the teeth are implanted evidently corresponds with the hæmal spine. The hæmapophyses often possess, however, one or two additional pieces, which are found still more developed in reptiles. The extreme mobility given to the lower jaw by this subdivision of the arch, is one of the most remarkable *teleological* modifications in the cranium of fishes; and if viewed in this light, we are spared the necessity of looking out for a separate *homology* for each element, which would certainly not be found in animals that have no occasion for such a peculiar conformation. One of the most interesting, and in our opinion the most successful, of Professor Owen's determinations, is that of the homology of the *opercular* bones which support the gill-cover. This series, made up of the *pre-opercular* (Fig. IV, 34), the *opercular* (35), the *sub-opercular* (36), and the *inter-opercular* (37), is now regarded by him as the "diverging appendage" of the tympano-mandibular arch. Physiologists have long since repudiated the strange doctrine propounded by Geoffroy St. Hilaire of the identity of these large bones with the minute *ossicula auditûs* of higher animals; but their connexion with the proper elements of the cranium has always been doubtful; and the idea suggested long since by Professor Owen himself,—that these bones really belong to the dermo-skeleton—has been accepted by many distinguished anatomists. He has found, however, upon further inquiry, that this is not their true relation; and looking at the size and importance which these diverging appendages from the hæmal arch elsewhere possess in the vertebral column of the fish, we have little doubt that his present view is the correct one.

The third inverted arch of the skull, the *hyoidean*, is the hæmal arch of the parietal vertebra; being suspended (through the medium of the epitympanics) from the mastoid bones or parapophyses. Like the preceding arch, it is usually composed of portions more numerous than the ordinary vertebral elements which they represent; the pleurapophyses being represented by the *stylo-hyals* (Fig. IV, 38), the hæmapophyses by the *epi-hyals* (39) and the *cerato-hyals* (40), whilst the hæmal spine is made up of the *basi-hyals* (41), the *glosso-hyals* (42), and the *uro-hyals* (43). That these

pieces only form a single arch, however, is perfectly obvious from their relative position; and we find in some fishes a considerable simplification of it. The "diverging appendage" of the hyoidean arch retains the form of a series of simple, elongated, slender, slightly-curved rays, which are articulated to depressions in the outer and posterior margins of the hæmapophyses. They are called *branchiostegals,* or gill-cover rays, because they support the membrane which closes the branchial chamber externally. The number and size of these rays vary considerably; sometimes they are absent altogether, whilst in the elops there are thirty on each side, and in the angler they are of enormous length. The most common number is seven, as in the cod (Fig. IV, 44). With the keystone or hæmal spine of the hyoidean arch are connected, more or less closely, a series of bony arches, of which six are usually at first developed and five retained. They are altogether called the *branchial* arches; but only the first four support the gills; the fifth, which is beset with teeth and guards the opening of the gullet, is distinguished as the pharyngeal arch. All these gill-and-tooth-bearing arches appertain to the splanchno-skeleton, or to that category of bones to which the hard jaw-like pieces supporting the teeth of the stomach of the lobster belong. They are sometimes cartilaginous when the true endo-skeleton is ossified, as in the lepidosiren; they are never ossified in the perenni-branchiate batrachia, and are the first to disappear in the larvæ of the caduci-branchiate species; and both their place and mode of attachment to the skull demonstrate that they have no essential homological relation to its vertebrate structure.

The fourth hæmal arch, appertaining to the occipital vertebra, is the *scapular;* forming the framework to which the anterior or thoracic extremities are attached. However absurd such a notion may appear to the mere anthropotomist, it is fully justified by an examination of the condition of this arch in the class of fishes; for, except in the higher cartilaginous fishes, it is as distinctly articulated to the cranium at its hinder part, as the lower jaw is in front. The pleurapophysis is usually represented by two bones; the *supra-scapula* (Fig. IV, 50) and the *scapula* (51); these are always confluent in the siluroids. The hæmapophysis is formed by a bone which is commonly termed the clavicle, but which Professor Owen regards (and we think with good reason) as rather corresponding with the *coracoid* of other oviparous vertebrata. The hæmal spine is wanting; the lower end of the arch being completed by the symphysis of the carocoids, which are usually united by a ligament; but are sometimes joined by a dentated suture. Like the other hæmal arches of the cranial vertebræ, the scapular arch supports a "diverging appendage" on each side; and this diverging appendage is nothing else than the *thoracic extremity.* We can well anticipate the ridicule with which this determination will be received by those who delight in laughing at what they are pleased to call the outrageous absurdities of the philosophical anatomists,—merely because they cannot comprehend them. That the hands and arms of man are nothing else than "diverging appendages" to his occipital bone, will be doubtless in their eyes to stamp the whole system as a tissue of dreamy transcendentalism. But if such persons will go to Nature, and interrogate her by a careful and candid scrutiny of the various forms and combinations which she presents, with the real desire to ascertain whether there be a

guiding plan, a unity of design, throughout the whole, or whether each organism is built up for itself alone without reference to the rest,—we are confident that they will find the former doctrine to be irresistibly forced upon them; and if, having adopted it, they will further inquire into the particular mode in which this plan is worked out, and will follow the guidance of the distinguished Hunterian Professor in the examination of the cranial bones of fishes, we are quite certain that if they do not feel every probability of his general correctness, they will at least be unable to prove him to be in error on any important point. We speak this advisedly, after having been present at a long debate between Professor Owen and the greatest ichthyologist of the present or any other time, Professor Agassiz; in which we perceived that every objection which the latter could urge against the vertebral theory (to which he *had been*, though we doubt whether he *still can be*, a decided opponent) had been met by anticipation in Professor Owen's system, and that he was consequently able to afford a satisfactory solution of it.

The homology of the thoracic extremities as "diverging appendages" of the scapular arch, is manifestly free from objection on the score of their complexity of structure, when we trace them to their simplest form, such as is presented in the lepidosiren, in which there is but a single-jointed ray. We have but to suppose these rays to be multiplied laterally,—according to the law of vegetative repetition, which most affects the parts that are farthest removed from the centre,—in order to understand how the bone which is single in the arm becomes double in the fore-arm, and forms a quintuple or even more numerous series in the hand. And when we look at the size and number occasionally presented by the branchiostegal rays, and the expanded fin-like aspect and movement of the opercular bones, we can scarcely deny that whatever be the relation which *they* bear to the hæmal arches of the two anterior cranial vertebræ, that of the thoracic extremities is the same to the scapular arch, or hæmal arch of the occipital vertebra. Nor does the removal of the scapular arch and its appendages from the back of the skull to the other end of the neck, in the higher vertebrata, afford the least ground for imagining that their homology is thereby changed; for this removal is seen to take place even in the class of fishes; and in the embryonic condition of the higher vertebrata, the scapular arch closely approximates the occiput, almost as in ordinary fishes. Numerous other examples of such displacement might be cited; it will be sufficient to refer to the ventral fins of fishes, which are not less the homologues of the pelvic extremities of the higher vertebrata, because they are sometimes brought forwards into close proximity with the pectoral fins, or even into advance of them.

Besides the cranial vertebræ with their various appendages, and the auditory, ophthalmic, and nasal sense-capsules, the skull even of the osseous fishes contains some bones that are referred by Professor Owen to the dermo-skeleton. The evidence for this homology is chiefly derived from the cranium of the sturgeon, in which the dermo- is much more fully developed than the neuro-skeleton; and also from the fact that these bones are more especially connected with the mucous organs of the skin. They are those denominated the *sub-orbital*, the *supra-orbital*, and the *supra-temporal*. One of the sub-orbitals is folded upon itself in such a manner as

to form a mucous channel, which extends from the orbit to the nasal sac, and is obviously analogous to the lachrymal canal of higher vertebrata; hence it may be inferred that the lachrymal bone, which has the same position and connexions, has the same origin, being the only part of the dermo-skeleton which is ossified in man, unless the turbinate bones are to be regarded in the same light.

We should most gladly quote largely from Professor Owen's admirable remarks on the teleology of the skeleton of fishes, or the modifications it presents in conformity with the special conditions in which these animals are to exist. The whole plan of structure appears at first sight to be so different from that which prevails in higher vertebrata, that if we do not keep the necessity for these modifications steadily in view, we shall be continually baffled in our homological pursuit. We must content ourselves, however, with one extract.

"We must guard ourselves from inferring absolute superiority of structure from apparent complexity. The lower jaw of fishes might at first view seem more complex than that of man, because it consists of a greater number of pieces, each ramus being composed of two or three, and sometimes more, separate bones. But, by parity of reasoning, the dental system of that jaw might be regarded as more complex, because it supports often three times or ten times, perhaps fifty times the number of teeth which are found in the human jaw. We here perceive, however, only an illustration of the law of vegetative repetition as the character of inferior organisms; and we may view in the same light the multiplication of pieces of which the supporting pedicle of the jaw is composed in fishes. But the great size and double glenoid or trochlear articulation of that pedicle, are developments beyond and in advance of the condition of the bones supporting the lower jaw in mammalia, and relate both to the increase of the capacity of the mouth in fishes for the lodgment of the great hyoid and branchial apparatus, and to the support of the opercula or doors which open and close the branchial chambers. The division of the long tympanic pedicle of osseous fishes into several partly-overlapping pieces adds to its strength, and by permitting a slight elastic bending of the whole diminishes the liability to fracture. The enormous size, moreover, of the tympano-mandibular arch, and of its diverging appendages, contributes to ensure that proportion of the head to the trunk which is best adapted for the progressive motion of the fish through the water. But, without the admission and appreciation of these pre-ordained adaptations to special exigencies in the skeleton of fishes, the superior strength and complex development of the tympanic pedicle and its appendages would be inexplicable and unintelligible in this lowest and firstborn class of vertebrate animals." (pp. 151-2.)

We have only left ourselves space to add, that the other organs and systems are treated in the same comprehensive manner with the osseous, although not with the same fullness of detail,—the determination of the homologies of the cranial bones being a question of such deep interest to the palæontologist as well as to the philosophical anatomist, as to require a full investigation of the data on which it is founded. We may especially remark that it gives us very great satisfaction to find that Professor Owen's views on the composition of the nervous centres of fishes are, in all essential particulars, the same with those which have been recently expressed in our own pages; and that his deductions regarding the physiology of the cerebellum, from the comparative development of that organ in different groups, are the very same with those at which we had arrived.

We anticipate with great interest the remaining volume, which is to in-

clude the comparative anatomy of reptiles, birds, and fishes; and we trust that Professor Owen may not be driven, by the numerous demands upon his time and attention, to that system of indefinite postponement, which has manifested itself of late in the delay of the later portions of almost every anatomical work that has been published in detached parts or volumes. Having ventured upon something like a promise as to the time of its appearance, we hope that he will not indulge the belief that literary "promises, like pie-crust, are made to be broken."

ART. XIII.

1. *Animal Chemistry; or Chemistry in its Applications to Physiology and Pathology.* By Baron LIEBIG.—*London,* 1846. 8vo.
2. *Experimental Researches on the Food of Animals and the Fattening of Cattle; with Remarks on the Food of Man.* By ROBERT DUNDAS THOMSON, M.D.—*London,* 1846. 8vo.

THE minute study of the nature and relations of the food of animals and plants has often appeared to us calculated to elucidate, not only the practical points, which are at once obvious even to a superficial observer, but also to throw a light upon several more abstract physiological questions, which have been debated too often with a reference merely to one phase in the history of organized beings. In directing our attention to some of the new views which have been broached upon the nature of food, it may not be out of place briefly to consider the bearing of the subject upon one or two of these disputed points.

A proper distinction between the animal and vegetable world has long been a desideratum with the physiologist. Nor will the circumstance of the question being still undecided appear remarkable, if we reflect that correct notions are scarcely yet formed of the proper constituents of the frames of the two classes of beings, and more particularly that the source of these substances is still disputed. But scientific men have even experienced much difficulty in defining the general characters of vegetables and animals. The former being confined to one spot, and the latter being moveable, some have considered that animals were distinctively locomotive. But it so happens that many of the lower tribes of animals are incapable of locomotion, and hence this definition is untenable. Others, observing that plants are destitute of sensation, have proposed to ascribe this attribute alone to animals, and define them as nervous beings. But in opposition to this view, we find many inferior animals apparently destitute of sensation, and only supplied with a degree of irritability even inferior to that of the sensitive plant, cultivated so frequently in our botanic gardens; and hence this definition also fails us. We believe that the true distinction between animals and vegetables will be detected more readily by discovering the nature of the matter by means of which they increase in bulk, or, in other words, by the nature of their food—the term food being a word applied to express the matter which enables plants and young animals to increase in size, and full-grown animals to preserve their forms unimpaired. According to this view then, it is from chemical physiology

that we are to expect an answer to the questions, "What is an animal?" "What is a vegetable?" To one accustomed to view only the larger kinds of animated beings it might seem an easy task to give a reply to these questions. But when we know that nature is simple in her works; that in her we find no sudden leaps from great to small, but that the whole animated world consists of a chain formed of a series of beings passing down in regular gradation from comparatively the most perfect to the most imperfect state, the lowest plant being closely allied to the lowest form of animal, it will at once be obvious that to say where plants begin and animals end cannot be a problem of easy solution.

To apply, however, the test which we have proposed, let us begin with plants. We find a plant cultivated among the Chinese and introduced among ourselves, termed the air-plant, which, by being merely suspended in the air, increases in bulk and weight without even the application of water. This is one of the most simple forms of vegetable life, as the plant has nothing to feed on save the atmosphere, which, however, contains all the elements necessary for its growth—oxygen, vapour of water, carbonic acid, and ammonia or some form of nitrogen. But all these are gaseous bodies or vapours, while the air-plant is a solid; hence we infer that this plant is capable of reducing gases to the solid form, and of thus increasing in bulk and weight. According to the views of persons best qualified to judge, it appears that all plants are endowed with similar properties, and that they mainly subsist by feeding on the gases which surround them, by converting these gases, by means of the organs with which they are supplied, into the solid forms of the vegetable kingdom, so endless in figure, but yet so lovely, that the greatest familiarity only renders them objects of superior admiration.

When we turn to the animal world, we find that the individuals of which it is composed are incapable of condensing gases; in fact the least educated knows that animals derive no sustenance from air; but that they require to imbibe solid matter similar to that of which they consist. Man lives upon animal food, and those kinds of grain which contain matter nearly allied to it. The question, why has grass, perhaps the most abundant vegetable in nature, never constituted a portion of human food, unless perhaps among the most degraded of the species, may not strike us as being in its answer fraught with important information. And yet the only reason which can be given for the fact, that it has never been an article of human food, is, because it contains such a small portion of matter similar in its nature to the constituents of man's frame, that the quantity required would be too voluminous for the digestive capacity of the stomach and other organs. An animal may therefore be defined, according to this view, to be a being which subsists by appropriating to itself food similar to the matter of which its own body is composed. Hence we see the necessity for its locomotion; while a plant, finding its nourishment in the air which constantly surrounds it, has its food brought to it by the usual laws of inanimate nature. We believe, then, that such will be found the most correct mode of separating animals from plants. It is probable that among the inferior tribes of animals, where an approximation is made to the vegetable kingdom, there may be individuals partaking of a semi-vegetable and animal nature, partly living on air and partly on solids. Although it does

not follow that such an occurrence is necessary, yet, from the simplicity and gradation which we find subsisting throughout nature, we might expect to discover some such union of the two kingdoms, or some equally simple transition from one set of beings to the other. To discover whether any such series of beings exists, it is obvious that we can proceed upon the principles which we have now been discussing. For example, if we find any usually considered plant or animal possessing in its constitution some substance which we know to be peculiar to a plant, and capable of being produced from gases through the instrumentality of the vegetable organism; and further, if we never find this matter present in animals, we seem to be drawing a legitimate conclusion when we infer that these species partake of a vegetable nature, and that we are approaching a point in creation, where the two great organized kingdoms are closely allied, or insensibly passing into each other.

This investigation has been commenced and has been followed by a successful result in one or two instances. It was observed some years ago by Wöhler, that the *frustulia salina* of Ehrenberg, a small zoophyte found in the slimy matter of saline springs at Königsborn, disengages large quantities of pure oxygen, so that when the mud containing a number of these beings is stirred with a stick, a beer-glass of water inverted may be filled with the gas in a very short space of time. This remarkable phenomenon, which never occurs in the clearly established animal race, but is a characteristic of plants, naturally attracted the attention of chemists, and, accordingly, upon examination by Schmidt, it was found that the constitution of this zoophyte shows that it is closely allied to the vegetable kingdom. A substance was extracted from it, which, after treatment with ether and dilute caustic potash, gave, by analysis, the silica being subtracted, the following composition:—carbon 46·19, hydrogen 6·63. Now these are exactly the numbers which have been obtained as expressing the composition of the basis of that inferior though familiar class of plants the lichens. The inference then seems legitimate, that the substance which constitutes the walls of the cells of at least this class of plants is identical with, in composition with, a substance found in the *frustulia salina.* If we compare the constitution of the cells of decided animals with that of vegetables, we find a sufficient distinction in the presence of nitrogen in the animal, and its absence in the vegetable cell. If we distil each of these matters respectively, we obtain an ammoniacal fluid in the one case, and an acetic fluid in the other,—a distinction pointed out between animal and vegetable substances so long ago as 1742 by Beccaria of Bologna. (Thomson's Researches on Food, p. 158.) All such chemical results, it is obvious, therefore, are important in enabling us to arrive at a conclusion in reference to the nature of the food of different beings, and therefore of the position which these beings hold in the scale of creation. So important, therefore, is an acquaintance with the nature of the food on which such beings subsist even to the physiologist.

Again, in the *cynthia mammillaris,* a species of ascidia, there is a thick fleshy sack connected with the gills, liver, &c., which consists of a congeries of large cells similar to the parenchyma of the cacti and many fruits. Upon its inner side numerous vessels expand, which communicate with the gills. When this outer sack is successively treated with water, alcohol,

ether, dilute acids, and alkalies, a colourless membrane remains, which is not altered by nitric, hydrochloric, acetic acids, nor by the most concentrated solution of caustic potash. It is quite destitute of nitrogen, and when analysed, was found to consist of 45·38 per cent. of carbon, and 6·47 of hydrogen, a composition identical with that of the cellular membrane of plants, which has been termed *cellulin* or *cellulose*, and is identical in composition with starch and sugar. In the preparation of the substance as previously described, the solution of potash in a caustic state dissolves up from the cellulin a quantity of azotized matter possessing albuminous properties. So that the cells of the beings examined were quite analogous to those of plants in more than one point of view, as it is an operation of some difficulty to separate the nitrogenous constituents of a plant from the cellular matter in those species on which no doubt exists as to their proper position in the organized scale.

From the observations which have been previously made, it is apparent that before we can point out the proper food destined for animals, we must study carefully their habits and endeavour to discover the food which they prefer when left freely to the enjoyment of their instincts. It is evident that there are certain laws which nature has laid down that serve to guide even the lower animals in the choice of their food. It is a rare occurrence to hear of the suicide or accidental death of a domesticated animal; still less frequent that of a creature which is free to roam amid the wild scenes of nature. We remember one case in which some dried and powdered monkshood (*aconitum napellus*), to the extent of a quarter of an ounce, was licked up by a pony. The animal suffered considerably, as if under an attack of glanders, for a few hours. But these instances are so rare that it might almost be affirmed that man is the only created being which disobeys the laws of nature; and it is merely when domesticated and under circumstances analogous to those in which man himself is placed, that we find the inferior creation imitating by such experiments their more godlike superiors.

It is of essential importance to decide by what method we are to arrive at the nature of the proper food of animals. There must be no *petitio principii.* With all our reverence for Scripture, therefore, we must protest against the conclusions which some well-meaning, but one-sided medical commentators have drawn, as to its being employed as a text-book for all or any of the sciences. Its object was moral, not physical, and the statements that occur in it in reference to science of all kinds, are necessarily such as are consonant with the opinions of those who lived in the earliest ages. If they had been, or could have been otherwise, it is highly probable that these writings would never have reached our times; but have long since been consigned to oblivion as the records of physical impossibilities, and of erroneous speculations in science, or as the dreams of the mystics. Nor are we to expect to derive physical truth from the lucubrations of the often well-meaning, but equally ignorant mere humanity-mongers, who "strain out the gnat and swallow the camel," exhibiting in themselves, as with all ultra-advocates, the most distressing instances of human inconsistency. These are imitators of Pythagoras, not only in their support of a so-called humanity, but also in their advocacy of the principle without consistency. That distinguished philosopher could exclaim against the

abominable wickedness that men should permit bowels to be buried in bowels, that one greedy body should grow fat with another body crammed into it, and that one animal should live by the death of another animal, and characterize the use of animal food, as champing with the teeth nothing but dreadful wounds, and thus reviving the manners of the Cyclops. "Why has the sheep deserved death," he asks, with vehemence, "that harmless animal which carries nectar in its full dugs, and furnishes us with soft clothing and aids us more by its life than its death?" At first sight, it is impossible to avoid sympathizing with the energetic and poetical eloquence of such a humane advocate. But the scene is suddenly changed; our sympathies are drawn back to our own bosoms, and another variety of feelings is engendered when we remember that this remarkable philosopher, as if to demonstrate that ultra views necessarily produce inconsistency, gave the lie to all his aspiring sentimentality, by sacrificing in the most cruel manner, and without any ulterior object to serve, 100 oxen in commemoration of his discovery, that the square on the hypothenuse of a right-angled-triangle is equal to the sum of the two squares on the base and perpendicular. So striking is this imperfection in his character, that his apologists suggest, but without any authority, that the oxen *must have been* composed of wax. But let it not be supposed that such opinions have fled before the light of knowledge, which has been promulgated during the 2344 years that have intervened since his death. For we find the same school of humanity-mongers, at the termination of that long period of time, still inculcating the idea in reference to the use of animal food, that "the man of cultivated moral feelings shrinks from the task of taking the life of the higher grade of animals, and abhors the thought of inflicting pain and shedding blood," just as if this were a correct statement of the question. While we therefore protest against every needless occasion of suffering, to all classes of animated nature, we should characterize such bastard humanity as we have alluded to as beneath consideration, nay in many cases as insufferable, if carried out with any semblance of consistency.

Some of the exclusively vegetable dietists, who base their doctrines upon a kind of one-sided metaphysico-historical research, commence by asking and solving the question as to the original food of man, not by the simple physiological method of observing the nature of the food with which the mother is supplied by nature for the support of the *original* man, but by speculating respecting the food used in the garden of Eden. The primary and original food of man, whatever speculators may say to the contrary, is milk, a fluid of animal origin. If those who are to regulate diet are not guided by scientific knowledge, and are not to exercise their judgment, they might be inclined to draw from this fact the conclusion that the proper nutriment of man is animal food, and this deduction might be defended with great show of reason to the exclusion of a vegetable diet altogether. But observation having proved that animals can subsist upon a vegetable as well as upon an animal regimen, and scientific research having satisfactorily demonstrated that the constituents of the two kinds of nutriment when well selected are identical, the one-sided position which might have previously been assumed and strenuously maintained, must yield to the lights of knowledge.

A careful examination of the constituents of the milk supplied by nature to animals, proves to us, as originally suggested by Dr. Prout, that a nitrogenous substance is required in our food, together with a representative of the oily and saccharine classes of substances. To Liebig we are indebted for the view which enables us to discriminate between the relative values of these species of nutriment; the nitrogenous food according to him, serving to supply the waste of the muscular tissues, while the unazotized materials act as agents in the process of respiration. An additional importance has lately been attributed not only to the necessary existence of such classes of substances in the food, but likewise to the proportion in which they exist in the different kinds of nourishment used by animals under different circumstances. (R. D. Thomson on the Food of Animals. &c. p. 167.) For example, we find the composition of the milk of women, the ass, and the cow, to be in reference to the amount of curd, sugar, and butter, nearly as follows.

	Woman.	Ass.	Cow.
Casein	1·52	1·95	4·16
Butter	3·55	1·29	3·70
Sugar	6·50	6·29	4·35

If we divide the amount of butter and sugar by the amount of casein, we shall obtain the relation of the nitrogenous to the non-nitrogenous principles. The relations are as follows:

In woman's milk the relation of casein to sugar and butter is as . .	1 to $6\frac{1}{2}$
In asses' milk ,, ,, ,, . .	1 to $3\frac{3}{4}$
In cow's milk ,, ,, ,, . .	1 to 2

We observe, therefore, that in the food of growing animals, there is a difference in some degree in the relation between that portion required to supply the waste of the system, and that employed in respiration or rather for the production of animal heat, depending undoubtedly on the nature of the animals. In practice we act upon this principle without perhaps appreciating its force; for when we are required to substitute cow's milk for the support of the human infant instead of its mother's milk, we add sugar and dilute it with water. Both of these precautions are judicious, but the former, in accordance with the view now taken of the subject, is particularly advisable. In confirmation of the existence of a relation between the two series of constituents of the food, it deserves attention, that all the analyses of the different kinds of milk which have hitherto been made prove that their composition is steady, and undergoes comparatively little variation. It is pretty obvious that from this circumstance alone, even if it were not strengthened by an abundant induction from other facts, that we may derive important knowledge with regard to the nature of human food. For when the milk is converted by the stomach into a proper condition for being absorbed, and is presented to the open mouths, it may be of the lacteals in its descent through the intestinal canal, we cannot understand how its constituents should be taken up by these absorbents in a proportion different from that which prevails in the natural condition of the fluid. Indeed we have evidence that in the healthy condition of the digestive organs of an infant, almost the whole of the constituents of the milk are absorbed, from the fact that when the fæces

of a sucking child are examined under the microscope, they are seen to consist mostly of epithelial cells derived from the mucous membrane of the intestines (Liebig.) If this be the fact with regard to milk, we may expect that it will also hold in reference to any other kind of aliment which the stomach is capable of digesting, that is of preparing it in a condition proper for being absorbed by the lacteals. And this conclusion we are compelled to adopt, unless we were to have recourse to the ancient exploded doctrine that the absorbents are endowed with a discriminating power which enables them to select what is proper for making good blood, and rejecting what would be injurious. If this doctrine were true, then poisons when swallowed internally ought to be innoxious, and the causes of diseases operating from within should uniformly be effete. But even the analogy of the vegetable kingdom exhibits the fallacy of such a view, and the beautiful process of variegating wood by causing chemical solutions of various kinds to be absorbed from the roots, and being conveyed by the sap to permeate the hardest portion of the monarch of the woods, is a sufficient answer to such notions. These considerations tend to show that not only must the food contain certain ingredients; but that these proximate elements must be present in the proper proportion.

The contrast of another species of food often administered to children with milk, which all must agree to be the type of the natural food of the infant man, will enable us to form an idea of the importance of the relationship which has now been insisted on. Arrow-root, for example, is almost universally given as infant food, and it belongs to the class of what is designated farinaceous diet. The following is the approximate composition of some of these substances per cent.:

	Arrow-root.	Tapioca.	Sago.
Starch, &c. . . .	84·29	83·87	84·87
Albuminous matter . .	3·21	3·13	3·33
Water . . .	12·50	13·00	11·80

In these articles of diet, the starch occupies the position which the sugar and oil represent in the milk. Hence if we divide the starch by the albuminous matter, we obtain the relation between the nitrogenous matter and heat-producing substance (calorifiant matter of Dr. Thomson) of 1 to 25. If we consider, for the sake of conciseness, these numbers to represent particles of the constituents of the food, we shall find that when a child consumes milk and arrow-root at separate meals, the nature of its fluids must be completely altered at these several times, since in the case of milk we have presented to the mouths of the lacteals in a digested state, 1 particle of nitrogenous matter and 6 particles of calorifiant matter, while with arrow-root 1 part of muscular substance and 25 of heat-producing materials are exhibited to the extremities of the absorbents, and carried into the circulation so far as we are capable of judging from an examination of the excretions. Under such circumstances, it is obviously impossible that the blood can possess a steady composition. The great predominance of calorifiant matter must give rise to the formation of substances which cannot be removed from the system with sufficient rapidity; while the muscular system cannot be sufficiently sustained in consequence of the necessary nutriment being withheld, and its place supplied with

matter which ought ultimately to be excreted in a volatile condition. It is true that the arrow-root class of food is usually administered along with milk; but although this may afford some palliation for the mistake of considering arrow-root as a sufficient substitute for milk, it affords after all but a slight amelioration of the fallacy. For if we represent the arrow-root and milk by a formula, by substituting such a diet for milk alone, we should commit the error of representing milk + arrow-root, as equal to milk + milk. And thus we should be landed in the exact position from which we had endeavoured to escape by the preceding subterfuge; that is, if we persisted in recommending arrow-root as a fit diet for children, we should be compelled to prove that it possessed the same composition as milk, or that its proximate constituents were capable of being transformed into the same substances and in the same proportions as exist in milk.

It does not follow that an error of this nature should be followed by any immediate result of such a description as to excite alarm, but it may act as a prelude to the access of dangerous causes of disease, or in the common language of medical writers, it may afford a predisposing cause for disease. A remarkable confirmation of the view which is here given, was afforded by an experiment made by Mr. Smith of Deanston, the distinguished agriculturalist. At a time when sago was universally recommended as a cheap material for feeding cattle, he purchased a quantity of it, and employed it for fattening a number of calves, substituting the sago for a certain amount of milk. The animals appeared to thrive and grow fat, but in the course of the following year every animal so fed died, some from inflammation, and others from incidental diseases, the accession of which he attributed to the use of the sago, the calves fed on milk alone, exhibiting no symptoms of unusual unhealthiness. This statement was made at a public meeting by Mr. Smith himself, after the previous view of the proper constitution of food had been explained, and is therefore an application due, not to the author of the view, but to one of extensive practical experience, who obtained the result without any reference at the time to the essential nature of the cause. This view, then, is directly opposed to the doctrine of Boussingault, to which reference was made in a former Number of this Review. That chemist states that the equivalent amount of different kinds of food may be determined by ascertaining the quantity of nitrogen which each contains. Every substance by this view, which therefore is possessed of nitrogen, is equally as good as any other, if the equivalent be used. Thus, 100lbs. of hay are equal to 612lbs. of turnips, or 400 of beetroot, or 281 of potatoes in reference to the feeding of cattle. But such an idea is at once refuted by the most common experience. Because, according to the same principle, 281lbs. of potatoes should answer as well for feeding horses as 54lbs. of oats, But every one knows that the greatest possible quantity of potatoes could never replace any quantity of oats in the nourishment of horses. The view which has been previously given affords a satisfactory reason why no such substitution is adequate. The following table supplies us with a view of the relation between the constituents of different articles of food, (R. D. Thomson, on the Food of Animals, p. 167):—

	Relation of nutritive to calorifiant food.
Milk—food for a growing animal	1 to 2 / 1 to 6½
Beans	1 to 2½
Oatmeal	1 to 5
Semolina / Barley	1 to 7
Wheat flower—food for an animal at rest	1 to 8
Potatoes	1 to 9
Rice	1 to 10
Turnips	1 to 11
Arrow-root, Tapioca, Sago	1 to 26
Starch	1 to 40

This table (which, however, can only be considered as approximate, since the proportion between the proximate constituents of food is constantly undergoing slight modifications) enables us to perceive that we can never expect such a substance as potatoes to replace such species of food as beans or oats without producing a decided prejudicial influence upon the animal so treated, more especially if the animal were exposed to any degree of muscular exertion. Suppose, for example, we represent the amount of muscle removed from the body of a horse to be 2lbs. per day, while the amount of food consumed in the production of heat was 12lbs., it is at once obvious that, to make up for this loss, we should never think of giving to the animal, food containing 2lbs. of albuminous or muscular matter and 52lbs. of calorifiant matter, that is, sago; neither should we give a diet containing 2lbs. of albumen and 22lbs. of calorifiant matter, that is, turnips. But we should endeavour to administer nourishment which contained as nearly as possible the ingredients which the animal's consumption required. This object would be nearly attained by the use of oats, which would give, for every 2lbs. of albumen, 10lbs. of calorifiant matter, or by barley, 2 to 14. A mixture, then, of the two grains would supply the deficiency required by the animal; or the same result would follow by the employment of beans and hay. In short, we can understand at once from this insight which we attain of the composition of food the whole rationale of the usual practical mode of feeding animals of all descriptions. "The animal system is thus in an analogous condition to a field from which different crops extract different quantities of matter from the soil, which must be determined by experiment." (On the Food of Animals, p. 168.)

We have already seen how nature provides for the different circumstances in which the varied species of animals are placed, by modifying in some degree the constitution of the milk. Judging from this fluid, we should be led to infer that the calf and young ass undergo more muscular exertion than the human infant, without any acquaintance with the habits of the animals; and that the calf wastes more muscular matter than the ass.

There are some animals which apparently make no use of calorifiant matter. How, then, it may be inquired, is their animal heat kept up? The American savage, for example, consumes animal food alone, and beasts of prey are nourished after a similar fashion, without having an opportunity of partaking of the fruits of the field; for, as they do not sow, it is impossible for them to reap. Yet their animal heat is sustained; not,

however, it must be observed, without great exertion, and a restless and wandering existence impressed on them in a great degree by the nature of the food upon which they subsist. All the food which they eat, with the exception of fat, which is, however, always present to a certain extent in muscular tissue (Liebig), enters into the solid constitution of their frames; and it is only when the muscular substance of the body is passing into gelatinous and other soluble states that it is capable of evolving heat. This degradation of muscular tissue must proceed even in an animal in a perfect state of rest; but it is accelerated by motion in proportion to the degree and violence of the exercise. To keep up the animal heat, more particularly in winter, when it is tending to radiate and to be conducted away by the colder surrounding media, the savage is compelled to cover himself with warm skins and to take a great amount of exercise. These expedients serve likewise as antagonists to the action of the atmosphere, and assist in forming and retaining heat, which is but tardily extricated. The function, then, which the calorifiant food fulfils in the animal economy, is at once obvious from this view. It is to save the waste of the muscular tissue and to admit of the formation of a sufficient amount of heat by a direct process of combustion. It is not a little remarkable that this physiological condition should constitute an essential element in civilization, by enabling the body to fulfil its functions without violent exertion, and to admit of unrestrained exercise of the mental powers.

There is an important subject of inquiry, upon which much light requires to be thrown before precise notions can be entertained of the true office of the constituents of the food. There seems little doubt that the fibrinous portion of the food is either freely divided or dissolved by the digestive action of the stomach, and is in this state absorbed by the lacteals. But what becomes of the starch is a question which has occasioned much labour of late among chemists. If we feed a hungry pig upon porridge, and in three hours, having killed the animal, filter the contents of its stomach, we find that the filtered liquid does not afford a blue precipitate with tincture of iodine, which it would do if a solution of starch were present; but we obtain a purple colour, indicative of the presence of dextrin or soluble starch. In addition to this fact, we observe that the fluid has a strong acid reaction, resembling that which can be detected in the process of starch making, when wheat flour is allowed to stand in contact with water. This acid possesses the properties of lactic acid. (Thomson on Food of Animals, p. 20; and Philosophical Mag., April and May, 1845.) Besides the production of the dextrin and lactic acid at the expense of the starch, we can demonstrate the presence of sugar in the stomach, having all the characters of that procurable by artificial means from starch. It is obvious that the lactic acid is not absorbed in its free state by the lacteals, because the fluids contained in these vessels are alkaline. There is no reason to suppose that dextrin enters these vessels. The most probable idea is, that the greater portion of the starch is converted into sugar, and that the acid is neutralized by the soda of the bile. That sugar enters by the lacteals appears highly probable from the experiments of Prout, who found a trace in the chyle of a dog fed on vegetable food; and a similar result has been obtained with reference to a trial for sugar in the blood by Dr. R. D. Thomson. (Phil. Mag., 1845, ut supra.) Still more lately Magendie states that he found sugar in the blood of a dog fed on potatoes (Journ.

de Pharmacie, Janvier, 1847), and also a substance resembling dextrin. It must also be stated, however, that Liebig endeavoured to detect sugar in the blood unsuccessfully. In such experiments, much must depend upon the method of investigation, and, as all those hitherto employed are liable to objection, it does not appear that we can give a distinct explanation of the destination of the sugar after it has left the intestines, although there is considerable evidence and probability in favour of the idea that a portion of the sugar derived from the starch of the food may circulate to a certain extent in the sanguineous system. But the circumstance which we observe, even out of the body, that starch and sugar rapidly change their forms when in contact with moist albuminous matter, would negative the notion that any considerable amount of sugar can exist in the normal blood. The rapidity, too, with which sugar can be converted into carbonic acid, renders the prospect still less likely. When we compare the formulæ of sugar and carbonic acid as represented by Liebig, we see that the only difference between these bodies is the replacement of hydrogen in the sugar by the oxygen of the atmosphere. (Liebig's Animal Chemistry, 3d edition, p. 108.)

Twelve atoms carbonic acid.	One atom grape sugar.
$C_{12}\,\begin{matrix}O_{12}\\O_{12}\end{matrix}$	$C_{12}\,\begin{matrix}H_{12}\\O_{12}\end{matrix}$

It is not necessary that the volatile form should be assumed at once by the sugar. On the contrary, it is probable that various intermediate stages are produced, such as, first, alcohol; then acetic acid, or lactic, butyric acids, &c., all tending to ultimate termination in carbonic acid, as represented by Liebig's formulæ.

Alcohol.	Acetic acid.	Formic acid.	Carbonic acid.
$C_8\,\begin{matrix}H_{12}\\O_4\end{matrix}$	$C_8\,\begin{matrix}H_8\\O_8\end{matrix}$	$C_8\,\begin{matrix}H_4\\O_{12}\end{matrix}$	$C_8\,\begin{matrix}O_4\\O_{12}\end{matrix}$

But when such formulæ are exhibited to represent our notions respecting our present views of any chemico-physiological process, it should be borne in mind that the authors of such formulæ merely give them as indicating the direction in which such changes may occur, not as being absolute expressions of fact; and this must be considered as a great step in the progress of physiology. Within but a few years we had not the most distant idea that starch could be converted into the principal constituent of butter; but we now understand how this can be effected, and thus are able to appreciate the connexion between farinaceous and oily food, two classes of bodies which were formerly placed at the greatest possible distance from each other in our chemical arrangements, but which can now be made to pass rapidly into each other. Such inquiries as that to which we have alluded, together with many others which have of late occupied the attention of chemists, have originated in theoretical views, which, although not accurate in all their details, have been proved by subsequent researches to have been made in the right direction. Numerous voyages have been made towards the Arctic and Antarctic Poles: those who have commanded them, it is true, have not succeeded hitherto in reaching the desired goal; but they have all conducted their expeditions with proper precautions, and each voyager has added to the knowledge acquired by his predecessor.

ART. XIV.

Diagnostische und Pathogenetische Untersuchungen in der Klinik des Herrn Geh. Raths Dr. Schönlein, auf dessen Veranlassung angestellt, und mit Benutzung anderweiliger Beobachtungen, veröffentlicht von Dr. R. REMAK. Mit einer Kupfertafel.—*Berlin,* 1845.

Diagnostic and Pathogenetic Investigations in Dr. Schönlein's Clinical Works. By Dr. R. REMAK. With a Copper-plate.—*Berlin,* 1845. 8vo, pp. 250.

THE contents of this volume of observations are arranged chronologically, and are the results of his labours in Schönlein's *clinique* from the commencement of the winter session, 1843, to the end of the winter session, 1844. The three sessions occurring in this interval are represented by three separate departments : the first session yielding materials for eight articles, the second (the summer session of 1844) for three, and the third for ten. In consequence of this arrangement, we find observations on the same disease scattered through several parts of the book ; thus we have three articles on abdominal typhus, and an equal number on pneumonia ; and two on Bright's disease, glanders in the human subject, and on the signification and importance of the buffy coat of the blood. In addition to them, we have single articles on dysentery, scarlatina, spermatorrhœa, tubercle, the metamorphosis of thrombus, pus and mucus, porrigo lupinosa, fungi occurring in the mouth and intestinal canal, and certain forms of parasitic acari. From this ample store we shall endeavour to select the most important and practical matter.

Abdominal typhus. Remak has minutely described the microscopic characters of the excrements in this disease. After noticing the circumstance that the occurrence of crystals of ammoniaco-magnesian phosphate is not diagnostic of typhus, as Schönlein originally (Müller's Archiv, 1836) believed, he states that he failed in detecting any fixed relations between their quantity and the stage of the disease. They were generally the most frequent in the most liquid stools, and were rarest when numerous infusoria were present.

Granular cells resembling (but more delicate than) pus-corpuscles are frequently observable in the liquid evacuations. The whitish flocculi which we commonly notice in typhus evacuations, are merely fragments of undigested vegetable food. It is singular that cylinder-epithelium is never to be detected ; in all probability the detached portions are altogether broken up within the intestinal canal. A small number of blood-corpuscles, more or less altered in form, may be often detected by the microscope ; and fat-globules are by no means uncommon, even in cases in which no fatty matters have been administered. In one instance, on examining the diseased portion of the intestine, the epithelium investing it had a whitish appearance, arising from its cells being filled with minute dark (fat ?) corpuscles.

The cryptogamic plants and the infusoria occurring in these cases are of comparatively little importance, and must be merely regarded as signs of the fermentation and putrefaction going on within the intestinal canal, in consequence of the deranged condition of the digestive functions.

Dysentery. Similar as are the ordinary appearances of the fæcal evacuations in dysentery and typhus, yet, examined under the microscope, they occasionally present striking differences. In the former, crystals of ammoniaco-magnesian phosphate are very rare, and the blood-corpuscles are not so much decoloured, nor so modified in form: moreover, long strings of coagulated fibrin are almost always intermingled with the corpuscles, indicating that the blood is derived from larger vessels and deeper structures. The granular cells already mentioned are, in this case, mixed with numerous flattened, spherical, and cylindrical epithelial cells, and the whole are imbedded in the structureless stroma (grundlage) of the mucus. Vibriones are scarcely ever present, but confervæ, and sometimes fermentation-fungi, occur in great excess, and apparently in a direct ratio with the degree of acidity presented by the evacuation.

Bright's disease. Remak confirms the view maintained by Henle, that the cylinders occurring in the urine are not modified urinary canals, but simply coagula of fibrin moulded in those canals. He conceives that the granular appearance of the cylinders is due to the presence of urate of ammonia. He regards their presence as of great importance in enabling us to establish an early diagnosis of the disease, and states that by this means he has, on more than one occasion, determined the nature of the disease several weeks before the first appearance of albumen in the urine.

Scarlatina. In cases of scarlatina the urine often continues to deposit a white flocculent sediment for a considerable time after the process of external desquamation has ceased. This deposit consists, for the most part, of epithelial scales from the surface of the bladder; and as long as it continues to occur, the patient must be carefully watched, even though in all other respects his health be completely re-established.

Pneumonia. Remak has, as we have already mentioned, contributed three articles on this subject, extending collectively over the space of thirty-four pages. After noticing how few important facts have yet been elicited in the examination of the expectoration in inflammatory affections of the air-passages, he proceeds to remark that he is fully convinced that there exists a microscopic distinction between true pus-corpuscles and the mucus-corpuscles which occur in the expectoration in cases of pulmonary inflammation. The latter differ from the former, not merely in possessing the property of absorbing water, and consequently, of becoming swollen, but likewise in the circumstance that the substance in which the nucleus is imbedded, is a fine granular, almost pulverulent matter, in which, after saturation, molecular motions may be observed. It is only after imbibition that the burst and compressed mucus-corpuscles bear any resemblance to pus-corpuscles; and even in the purulent (?) expectoration occurring in the last stages of pulmonary expectoration, Remak has never detected cells, which, in the character of their granular contents, altogether correspond with pus-corpuscles from abscesses. Moreover, the granular cells occurring in puriform sputa are always deposited in a tenacious stroma peculiar to mucus, and never present in genuine pus.

In pneumonic expectoration he not unfrequently met with dark granular, roundish bodies, considerably larger than mucus-corpuscles, and probably identical with Gluge's inflammatory globule. As he has also noticed them in the tough mucus, loosened from the fauces on clearing

the throat, and likewise in the pulmonary vesicles of perfectly healthy cattle, it is difficult to decide their exact pathological importance. By far the most important of his observations in relation to the sputa in pneumonia is, that they invariably contain *ramifying bronchial coagula.* This peculiar form of expectoration may occur at different periods of the disease and in different quantities. It is not always easy of detection. Remak advises that the whole of the expectoration should be poured into a dark-coloured flat vessel filled with water, the colour enabling us to distinguish the white coagula from the mucous and puriform matter with which they are associated; or that the individual clots in which the presence of these coagula may be suspected, should be examined on a dark glass plate.

The bronchial coagula form ramifying cylinders pursuing a nearly rectilinear course, presenting a dichotomic arrangement, and gradually diminishing in length and thickness. The main stem is, however, usually thinner than the contiguous branches, and tapers off in a thread-like form at its free extremity. At the points of bifurcation we not unfrequently observe a slight dilatation, depending probably on a similar condition of the bronchial ramification. These cylindrical coagula are sometimes partially flattened, and are sometimes swollen at various points, the latter phenomenon being caused by inclosed air-bubbles.

The annexed engraving copied from Remak's treatise represents the natural size of different forms of these coagula.

" Ramifying bronchial coagula, spread on a piece of glass, lying on a black ground. Natural size."

(From Plate, Fig. 4.)

These bronchial coagula are formed of extremely delicate threads running parallel to, and closely connected with, one another, and in most cases, either inclose or are covered with granular cells, closely resembling pus-corpuscles. Their strength and mode of arrangement are suggestive of areolar tissue, but the difference becomes marked on the addition of acetic acid; for although the fibres become perfectly transparent and a number of nuclei remain, yet they are evidently the nuclei of dissolved granular cells, and altogether different from the elongated variety occurring in areolar tissue. Dr. Heintz has shown by a series of chemical experiments that these coagula are formed of a protein-compound,

but whether that protein-compound is fibrin or whether it contains oxy-protein is uncertain.

The bronchial coagula appear, in the majority of cases, between the third and seventh day of the disease, being rarely absent on the fourth and fifth. This observation applies, however, only to those cases in which the proper remedies have been applied from the commencement; for in a man in whom the disease was allowed to go on unchecked, and there were signs of hepatization before any treatment was adopted, they were not apparent till the fourteenth day.

The characters of the expectoration in the three stages of pneumonia are well known: in the first stage the sputa consists of gray, stringy, viscid mucus, usually tinged with blood; in the second stage it consists of whitish, firm, clotted masses, whose white delicate fibres extend to the bottom of the vessel; and lastly, it forms soft roundish masses of a white or yellow (puriform) colour, without dependent mucus-fibrils. The bronchial coagula occur in the first and second but not in the third form. When they are found during the first stage they are very thin, not thicker than fine threads, and give off few branches; when present in the second stage they are stronger and ramify to a greater extent.

In making these examinations, the difference between the dependent white mucus-fibrils and the bronchial coagula may be easily discerned, even without the use of the microscope, by spreading the expectoration on a glass plate, when the white mucus-fibrils can be readily drawn out with a needle in the form of a delicate membrane, while the bronchial coagula, from their firmness, resist such an attempt, and are further rendered sufficiently conspicuous by their constant ramifications.

Remak next considers the connexion between the expectoration of these coagula, and the phenomena of auscultation and percussion.

The appearance of the delicate coagula imbedded in viscid mucus is generally simultaneous with that condition of the lungs in which the crepitation is the most marked while at the same time the sound evolved on percussion indicates that the substance of the lung is partially impervious to air; the firmer coagula usually occur at the period when the crepitation has given place to bronchial respiration, and the dullness on percussion indicates the existence of hepatization.

One important practical result deduced from Remak's observations, is that the earlier the expectoration of the coagula commences, and the more abundant and continuous it is, so much the more certain and speedy will be the cure. In the ordinary course of pneumonia in vigorous persons, the delicate coagula appear in the first viscid mucus that is expectorated, and continue increasing in quantity and size till the fifth day of the disease; decreasing gradually from that period, until white, easily compressible masses of a cylindrical but non-ramifying form appear in their place, exhibiting, under the microscope, indications of the fibrous structure of coagulated fibrin, and a multitude of granular mucus-corpuscles.

In the fifty cases of pneumonia observed by Remak, there were not more than four or five in whom even a partial diminution of the symptoms occurred previously to the appearance of the coagula in the expectoration, while in the great majority an amendment was first observed after their occurrence. In the few cases in which the proper remedies had not been

applied as early as they ought, the appearance of the firm, ramifying, bronchial coagula, was preceded by that of soft, cylindrical, white fibrous masses, which likewise assume the form of the bronchi, but usually follow the discharge of the firm coagula, and appear to indicate their softening. Schönlein is of opinion that in these cases there is a want of sufficient energy in the lungs to eject the bronchial coagula at the due time, and that consequently, they soften in the bronchi.

Although these coagula were detected by Remak in every case of pneumonia, yet in four cases of genuine bronchitis that fell under his observation, he could not discover the slightest trace of them, but simply granular cells in a mass of ropy mucus.

Schönlein regards the presence of the bronchial coagula in the expectoration as a certain evidence that exudation is going on, in which case, unless any violent reaction should call for its adoption, general blood-letting is replaced by cupping, mercurial and iodine frictions, calomel, and diuretic and cooling medicines, such as infusion of digitalis with nitrate of potash.

Glanders. Remak describes two cases of acute glanders occurring in the human subject, and states that Froriep usually sees several such cases annually. After giving a description of the microscopic appearances of the lardaceous, purulent masses met with in the lungs, muscles, &c., he mentions that in relation to the diagnosis of typhus fever, it is always of the greatest importance to ascertain whether the patient has exposed himself to the possible infection of glanders, and quotes the following case:

"During the past session a waggoner aged thirty years entered the *Clinique*, having for the six previous days experienced shiverings, great depression, and pain in the muscles of the calf, thigh, and arm, on those parts being touched. The fever was moderate (pulse 90), the tongue moist and not coated; no appetite. Neither the urine nor the skin gave indication that the disease was of a rheumatic nature. From that day the pain disappeared from the affected muscles, but was felt in the left calf, in which a slightly red tumour, painful on pressure, began to form. The patient confessed that he had been engaged with diseased horses. On the third day there was pain in the muscles of the back, and in the region of the kidney, on pressure; blood-corpuscles and albumen were found in the urine, and continued to present themselves for several days. On the supposition that the disease was connected with the infection of glanders, Schönlein adopted Andral's plan of treatment with iodide of potassium, giving a scruple daily. The muscular pains, hæmaturia, and tumour then gradually disappeared. In the course of fourteen days he was altogether free from fever, but the affected muscles continued weak for a considerable time." (pp. 191-2.)

The disease in the two acute cases proved fatal on the second and fourth days respectively.

The buffy coat of the blood. A memoir of nearly forty pages on the signification of the buffy coat, and on the formation and production of the blood-corpuscles, terminates with the following practical remarks:

1. In order to arrive at a certain appreciation of the diagnostic and prognostic value of the buffy coat, it is requisite that the blood in all cases of venesection, should be collected in narrow and high vessels. It frequently happens that when broad vessels are used, and the coagulation proceeds irregularly, no buffy coat is formed, when if collected in another

manner it would have undoubtedly appeared—a fact which, although generally known, is very little attended to in a practical point of view. Since attention has been paid in Schönlein's *Clinique* to the mode of collecting the blood, and its coagulation has been carefully watched, a buffy coat, often of considerable extent, has been observed in every case. Although the mode of bleeding pursued by Schönlein in inflammatory cases, can only be adopted when the inflammation is of a high degree, yet in some cases of pneumo-typhus and in a case of glanders, there was an extensive buffy coat containing an extraordinary number of colourless blood-cells. As it is practically impossible to make an accurate quantitative analysis of the blood in every case of venesection, we should not neglect so simple a means of arriving at an approximate determination of the amount of fibrin.

2. The microscopic examination of the buffy coat in relation to the quantity of colourless blood-cells, may be made highly useful in determining its importance as indicative of the stage of inflammation. The absence of many colourless blood-cells in the buffy coat, affords a much more certain indication of an unusually large amount of fibrin arising from inflammation, than does the presence of a great number; the latter condition being generally dependent on the regeneration of blood after repeated venesection, and probably connected with an imperfect metamorphosis of the cellular elements of the blood in discrasic diseases, such as typhus, glanders, scurvy, and cancer.

Spermatorrhœa. From a long and interesting memoir on the subject drawn up from the observation of forty-five cases, we select the two following passages:

"Without the confession of the patient, the occurrence of a cartilaginous hardness of the corpora cavernosa affords diagnostic evidence of long-practised habits of onanism." (p. 153, note.)

"Gonorrhœa undoubtedly predisposes to spermatorrhœa, as was shown by Lallemand. But whether it be the gonorrhœa itself, or the balsam of copaiva taken to cure it, that causes this predisposition, it is difficult to say. I regard the latter as probable, since it has been noticed by Schönlein to induce baneful effects on many persons who have taken it for gonorrhœa in full doses, and for a long time, giving rise to Bright's disease." (pp. 168-9.)

Porrigo lupinosa. Remak carefully examined the scabs in a large number of cases of porrigo lupinosa, in order to ascertain whether the fungous structures contained in them were always identical. His observations on this point coincide with those instituted by Schönlein at Zürich, Fuch's and Langenbeck at Göttingen, Gruby at Paris, and Bennett at Edinburgh, and thus tend to show that geographical position exerts no influence on the species of the vegetable parasite.

A hair is usually found in the centre of each scab, and around the hair there are concentric furrows, dividing the scab into a number of rings averaging the fourth of a line in breadth. The scab increases by the augmentation of the external ring.

On cutting through one of the older scabs, after its removal from the body, we observe two distinct strata separated by a boundary line; the one next to the skin being thin, whitish and brittle, while the other is thick and yellow, forming the free surface of the scab. In the white

layer is the thallus, while in the yellow the filaments (Sporidienträger) and sporidia predominate. After a copious description of the microscopic characters of these minute fungi, he proceeds to consider the place they ought to hold amongst the cryptogamia. From the examination of specimens sent by him to G. R. Link and Dr. Klotzsch, those botanists are of opinion that the favus-fungus is a distant species, and not to be classed under either torula or oidium. The latter rather inclines to regard it as a species of the genus sporotrichum. Remak proposes for it the name of achorion Schönleini, derived from achor, the old name for favus; and gives the following botanical characters of the plant:

"Achorion Schönleini nobis, orbiculare, flavum, coriaceum, cuti humanæ præsertim capitis insidens; rhizopodion molle, pellucidum, floccosum, floccis tenuissimis, vix articulatis, ramosissimis, anastomoticis (?); mycelium floccis crassioribus, subramosis, distincte articulatis, articulis inæqualibus irregularibus in sporidia abeuntibus; sporidia rotunda, ovalia vel irregularia, in uno vel pluribus lateribus germinantia."

He then proceeds to relate a series of experiments on the distribution and growth of these fungi, and gives a circumstantial account of a successful attempt at inoculating his own arm. The mode of formation of the scab, and the chemical character of fluids conducing to the development of the fungi are then considered.

We shall terminate our notice of this section with a quotation containing a sound practical truth—one that should never be lost sight of in the treatment of this class of diseases:

"Eruptions on the heads of children appear to take the place of other morbid processes in more important organs; they are found to alternate with chronic inflammation and mucous discharges of the conjunctiva and cornea, and of the external auditory meatus, with enlargement and suppuration of the cervical glands, with enlargement and atrophy of the mesenteric glands, and probably also with tubercles of the lungs, bones, and intestinal canal. . . . It is very possible that the irritation and suppuration of the skin, produced by the growth of the fungous scabs, may in certain cases exercise a favorable influence on the condition of the organism." (p. 213.)

Fungi in the mouth and intestinal canal. In his observations on the microscopic characters of the aphthæ of children, and of adults (during typhus), Remak found that a single aphtha often contained several species of fungi, and that the species of fungi occurring in different aphthæ on the same individual exhibited no constant relationship; also that while one aphtha contained numerous fungi, others on the same individual contained none at all. Hence it appears that the loosening (auflockerung) and ulceration of the mucous membrane is the primary phenomenon, and that the fungi are only produced under favorable chemical conditions. It has been long known that the presence of aphthæ is usually associated with an excess of acidity in the primæ viæ, and it is very probable that a putrid condition of the surface of the mucous membrane would accelerate the conversion of sugar and starch into lactic acid, and thus afford a favorable soil for the growth of these fungi.

Even in health there are produced during sleep a quantity of confervæ in the cavity of the mouth, which adhere to the mucous membrane and the teeth. They are probably occasioned by the decomposition of particles of food, or of mucus. In a young man suffering from hoarseness, Remak

found that the mucus which was secreted in great quantity by the soft palate, contained a large number of ramifying thallus-filaments. In diseases of the air-passages in which the epithelium of the mouth and throat is being frequently abraded and renewed, traces of cryptogamic plants may be generally detected in the expectoration. In the bronchial coagula of pneumonia patients (vide supra p. 505), Remak constantly found a peculiar variety of bifurcating, ramifying thallus-filaments, whose length and degree of development indicated that they could not have formed subsequently to the expectoration of the coagula.

There is a remarkable difference between the pathogenetic relations of the achorion and the fungi growing in the cavity of the mouth, and probably also in aphthæ. The achorion takes deep root in the sound uninjured cutis, which indeed seems to be the only place where it attains its full development. The fungi of the mouth, throat, and air-passages are, on the other hand, merely secondary products, formed from the decomposition of the mucous membrane and of foreign substances (fragments of food, &c.) in contact with it, and are analogous to fungi which, under proper chemical conditions, become developed out of the body. The achorion may be just as correctly termed a parasitic plant as the *acarus scabiei* or *a. folliculorum* may be termed parasitic animals; but these fungi can no more be regarded as true parasites any more than the infusoria which are abundantly present in the fluid putrid evacuations from the bowels in typhus.

A microscopic examination of the brown, chocolate-like masses observed in the sour vomited matters in cases of cancer of the stomach, has led to the interesting discovery that they consist for the most part of yeast-plants. In one case the fermentation was accompanied by a well-marked development of gas, and there was an obvious separation of the yeast into an upper and lower stratum.

We have endeavoured in the preceding pages to lay before our readers an abstract of the most important facts and observations contained in Remak's volume, and it only remains for us to express our earnest hope that this is only the commencement of a long series of our author's "diagnostic and pathogenetic investigations."

Art. XV.

Practical Remarks on Near Sight, Aged Sight, and Impaired Vision; with Observations upon the Use of Glasses and on Artificial Light. By William White Cooper.—*London*, 1847. 8vo, pp. 216.

We cannot agree with Mr. Cooper, that there is a "paucity of works of authority in the English language, upon Near and Aged Sight, the use of Glasses, and Impaired Vision." Beer's Pflege have long been familiar to the English reader, under the spurious title of 'The Art of Preserving the Sight, by an Experienced Oculist.' Adams's 'Essay on Vision,' Kitchener's 'Economy of the Eyes,' Ross 'on the Use and Abuse of Spectacles,' Walker's 'Philosophy of the Eye,' Franz 'on the Eye,' Hunter 'on the Influence of Artificial Light,' and many other works might be mentioned, which expressly embrace the subjects which Mr. Cooper proposes "to

elucidate." We should still be glad, however, to hail a new and more complete work on the preservation of sight, than any that has hitherto appeared; and that whether it was intended for the popular reader's instruction, or for that of the medical practitioner.

Mr. Cooper's work is divided into six chapters, which treat of Light, Myopia, Presbyopia, Spectacles, Inflammation and other diseases of the Eye and their treatment, and Artificial Light. Upon these various topics there is much information put down in Mr. Cooper's pages; but as he has evidently had in his eye, as worthy of imitation, our old familiar friend, who aimed to kill two dogs with one stone, what he says is half addressed to the popular reader, half to the medical, and we fear will be found satisfactory to neither. For example, he gives in his first chapter, an account of the anatomy of the organ of vision. Now, neither of the above parties will thank him for so scanty a statement respecting the nerves of the iris, or the lenticular ganglion as that contained (page 29) in this single short sentence—"The nerves which supply the iris are derived from the lenticular ganglion." "So we have often been told," will the medical reader be disposed to remark, "and we know very well how the lenticular ganglion is made up of a sensitive root and a motive root, and how its branches, the ciliary nerves, sink into the annulus albidus of the choroid; but since Mr. Cooper was in an "elucidating" mood when he wrote his book, we wish he had thrown some light on the final distribution of these nerves, which, if they go at all to the iris, send but a very small proportion of their filaments to that membrane." "Lenticular ganglion!" exclaims the non-professional reader. "What is that? The nerves of the iris are derived from it? I am just as wise as when I began." This may serve to show the futility of works on anatomy recommended for popular use, but written in the sketchy manner of Mr. Cooper.

We are not satisfied that the scientific parts of Mr. Cooper's work will be found everywhere correct, nor that their faults will escape detection even by the merest tyro in medicine. His frontispiece, for instance, is, he says, a vertical section of the eye, but this must be a mistake, as the optic nerve is represented entering the eye much below the axis. At page 15, he tells us, that the crystalline lens is "the most important of the refracting media of the eye;" whereas every well-informed medical student knows, that the crystalline does not refract the light entering the eye nearly so much as the cornea, and that, although the crystalline may be removed, and yet the person continue to see, we cannot take the same liberty with the cornea or the vitreous humour, without destroying vision. Even evacuation of the aqueous humour will, till that fluid is regenerated, affect sight much more than removal of the crystalline. At page 35, Mr. Cooper says, "If, before a hole made in the window-shutter of a darkened room, a double convex lens be placed, an inverted picture of the scene without will be portrayed by the lens upon a piece of white paper." Now, the merest tyro knows that the inverted picture is portrayed before the lens is applied to the hole; in other words, that the hole forms the picture, which the lens merely renders brighter and smaller. At page 37, he tells us, that "for *single vision* it is essential that our eyes be so adjusted, that the image formed in each shall fall on corresponding points of the retina." Physiologists have given the name of *corresponding points* to those parts of

the two retinæ which lie at equal distances from the vertex, and in the same direction; that is, both to the right, or both to the left, both upwards, or both downwards. Had Mr. Cooper studied Professor Wheatstone, to whom he immediately afterwards refers, he would have learned that the observations of that gentleman afford ample proof, that objects, the images of which do not fall on corresponding points of the two retinæ, may still appear single. If Mr. Cooper will just hold out a pencil-case, or any similar body, directly forwards from the point of his nose, he will see it single, by means of two images, falling the one on the temporal side of the right retina and the other on the temporal side of the left, and therefore on non-corresponding points. What is still more curious, Professor Wheatstone has shown, that similar images falling on corresponding points of the two retinæ may appear double and in different places; all which is familiar to every one who has amused himself with the professor's instrument called the *stereoscope*.

Chapter V is a strange medley, consisting of cases of muscæ volitantes, and amaurosis; Milton's Letter to Leonard Phalaras the Athenian, (who, residing at Paris as envoy from the Duke of Parma, had got acquainted with Thevenot, the oculist), and receipts for curing ophthalmia tarsi. These last can be of no use, as here set down, to medical men, and may be the source of much evil to patients should they take it in their heads to *try* to cure themselves by means of the six formulæ, here clapt so unceremoniously cheek-by-jole with the immortal poet's learned epistle.

At page 51, we have a case narrated of incomplete congenital cataract, for many years mistaken for aggravated myopia. Mr. Cooper dilated the pupil by atropine, and the patient saw past the cataractous portion of the lenses. A very trifling operation, he adds, rendered the newly acquired powers of vision permanent. Mr. Cooper should have stated what the operation was, which, whether it was prolapsing a bit of iris through the cornea or dividing the cataract, he might have done almost in as few words as he has used to raise the reader's wonder by calling it "a very trifling operation."

We have no great opinion of the application of collyria to the surface of the conjunctiva. By removing the mucous secretion of that membrane, they generally irritate the eye rather than relieve it, except where the secretion is morbidly changed in quantity and quality. Mr. Cooper thinks differently, and therefore advises the use of an eye-cup, adding that if one "cannot be procured, the eye should be bathed with soft rag, dipped in the collyrium." We should prefer the rag, but an egg-cup will form no bad substitute for an eye-cup, if such a contrivance is thought essential.

At page 157, the reader will find a sufficient specimen of what we deem the most objectionable parts of Mr. Cooper's book, in the directions laid down for the treatment of congestion of the retina. These embrace nothing but what is perfectly familiar to the practitioner, but they would be extremely dangerous for any patient to attempt to follow, without medical superintendence. Even the dietetical advice is not at all to our mind, for assuredly dyspeptic subjects, troubled with flatulence and acidity, will be far better without such stimulants as India pale ale, porter or stout, while weak mixtures of spirit and water, such as soda or seltzer water, with a little brandy or genuine sherry, recommended by Mr. Cooper,

from running so readily into acid, are the very things to be especially avoided.

On the whole, notwithstanding these objections, we have no hesitation in saying, that Mr. Cooper's work contains many just and valuable hints on the preservation of sight. The instructions he delivers on the necessity of caution in the use of glasses are highly important, and the book is written in a pleasant readable style.

Art. XVI.

On the Pathology and Treatment of Scrofula; being the Forthergillian Prize Essay for 1846. By Robert Mortimer Glover, m.d., &c. &c. —*London*, 1846. 8vo, pp. 315.

Having so recently reviewed the pathology and therapeutics of scrofula, in our notice of Mr. B. Phillips's most valuable publication, it is scarcely to be expected that we should go over the subject again. There are some points, however, in Dr. Glover's publication which merit notice. For several years, much attention has been given by the author (as we learn from the preface) to the subject of scrofula. In the year 1842, the Harveian Society of Edinburgh awarded him their medal for an essay on the physiological and medicinal properties of bromine, and on the analogies of the chlorine, bromine, and iodine group. In the spring of 1844, a dispensary was established in Newcastle, by Mr. Potter and him, with the assistance of some friends, for the treatment of scrofula, phthisis, and cutaneous affections and diseases of the joints, with the view of making an extensive trial of the therapeutical properties of some of the new remedies which have been proposed of late years in these affections, and especially with the view of trying the efficacy of the compounds of iodine. When the Medical Society of London announced their intention of awarding the Fothergillian gold medal for 1846, for the best essay on the pathology and treatment of scrofula, the author was induced to compete, and encouraged by the result, to publish. We have therefore a liber emeritus. Further, the work contains original investigations into the pathology of the disease, which were commenced in a right spirit, and under the guidance of sound principles. The microscope and chemical analysis have been used freely by him, the use of which he triumphantly defends:

"Many have drawn conclusions from the errors committed in the premature application of the resources and modes of research which we have alluded to; but it should be remembered that everything great and important must be bought at a price proportionate to its value. The failures which have taken place occurred in the infancy of chemical science, and before the existence of microscopic analysis as an art. Dr. Graves of Dublin has recently objected to the paucity of real results obtained by chemical analysis in the investigation of disease; but medicine has been two thousand years, according to the system of mere observation, in arriving at its present imperfect state, and the resources of chemistry have just begun to be applied, while every application made of them points at a vital part of our science, and tends to the solution of problems of the first magnitude. Henceforth morbid anatomy must not be confined to the department of the picture maker; but chemical and microscopical analysis should form the more important elements of this branch of pathology.

"The great difficulty to be contended with in medicine is the obscurity in which the connexion of observed facts is veiled. Hence the great value of anything approaching to experiment. The function of experiment must be distinguished from that of mere observation; experiment has in view the nature of the connexion between ascertained or observed facts, in order to test the constancy and essentiality of the relation; in other words, it is the bringing out of what Bacon terms prerogative facts. For instance, iodine is a remedy supposed capable of producing the absorption of scrofulous tumours. But a mere case in which a scrofulous person recovers under the use of iodine is of most moderate value: for patients have got rid of scrofula while submitted to every form of treatment, as proposed by generation after generation of so-called *practical men*, and remedy after remedy, thus used with apparent success, has fallen into oblivion. Suppose we were able to show that the use of iodine promotes not merely the flow of the urine, but also an increase of the solid contents in this fluid, and especially of the amount of urea? In other words, iodine excites the secondary digestion of the tissues; and as urea is the product of the albuminous tissues thus converted, and tubercle is composed chiefly of albumen, we learn that the connexion between the giving of the remedy and the absorption of a scrofulous tumour is not accidental, and thus we may derive a confidence which the blind empiricism so absurdly denominated *practical*, could never properly give. The sequel will prove that there is no wish here to undervalue observation of any kind; it is only intended to put prominently forward the great value of rationalism in medicine; and the reflections just written have been dictated by a review of the numerous failures in generalization presented by the history of the medical literature of scrofula;—failures which have chiefly arisen from too hastily grasping at the sequence and relationship of facts, without a sufficient consideration whether the connexion observed was essential and constant, or merely accidental." (pp. 5-8.)

After glancing at the different opinions regarding scrofula which has been brought forward, or have been current, Dr. Glover proceeds to discuss the pathology and treatment of scrofula, and first gives a description of scrofulous matter or tubercle. By this term, he wishes to designate a peculiar morbid formation, the product, as he considers, of a particular modification of the inflammatory process. This product is to be distinguished, firstly, from structures produced by ordinary (or what may with some liberty of speech be termed normal) inflammation; secondly, from various morbid growths inclining more or less to a parasitic character, and also from malignant structures; and thirdly, from various heterogeneous bodies of a totally different character, but which have sometimes been regarded as partaking more or less of the scrofulous deposit. Dr. Glover does not deny, that there may be cases in which scrofulous formations pass gradually into structures having characters different from those by which true scrofulous deposits are to be here described; this is only what we everywhere find in nature where absolute limits are unknown. As an example, may be noted those compound new formations, in which lymph of the usual structure, organized, is found to be intermingled with scrofulous matter presenting the characters of this formation when examined by the microscope.

"Tubercle, in the sense in which the term is here used, includes all scrofulous formations, whether in the lungs or in the lymphatic or lacteal glands, in the heart, the liver, the kidneys, the spleen, the brain, or spinal cord, the free surfaces of the mucous or serous membranes, the interstitial cellular membranes, the cellular tissue under the skin, the bones and periosteum, and, in short, in every conceivable tissue or organ; and we distinguish the scrofulous matter by its microscopic characters, with a certain not very definite chemical constitution, and a physical

structure apparent to the eye, and which is well known. These characters, taken altogether, after the manner of the method of natural families in classifications of natural history, appear sufficient, as they can be described, to separate as accurately as can be done in any natural study, the scrofulous formations from all others." (p. 25.)

Dr. Glover next proceeds to notice the literary history of the external forms of tubercle, and then discusses the question as to its vascularity. He decides that the vascularity is not essential.

"The obliteration of the vessels of the tissue into which the scrofulous or tubercular matter is effused, may be carried to a greater or less extent; the inflammatory action produced by the presence of the tubercle on the surrounding tissues may, in some instances, cause obstruction of the vessels, or, in an earlier stage, increased vascularity; the envelopes which are formed around the tubercular mass may be more or less vascular, or totally devoid of vessels; but tubercle matter itself is *beyond the normal influence of the circulation;* and this fact is confirmed by the results of microscopic examination." (p. 37.)

The microscopic structure of tubercle, and the degree of organization it possesses, have given rise to much difference of opinion. Dr. Glover very succinctly reviews the opinions of the most recent investigators, including Canstatt, Vogel, Scharlau, Gluge, Kuhn, Gruby, Bennett of Edinburgh, Gulliver, and others. Dr. Glover's own investigations present results closely resembling those attained by Lebert and Dr. Bennett.

"Our own observations have been made with powers of four hundred, and six hundred and ten diameters, and on tubercles from the lungs, heart, spleen, renal capsules, kidneys and bladder, and on tuberculated mesenteric, bronchial, and cervical glands. The ordinary element of tubercle present in all the forms which we have examined, and scarcely different in one situation from what it is in another, is the granular corpuscle described by several writers. Many tubercular masses are composed almost wholly of this matter, which varies in size from about the bulk of a blood-globule to about, perhaps, the one 10,000th of an inch in diameter. These corpuscles are generally of a somewhat yellowish colour, and when magnified by the highest power which we have used (610 diameters) show occasionally spots in their substance which may possibly in some cases be nuclei. Mixed with these, which we believe to be in some instances altered cells, in other cases new formations, we have the following elements. 1st. Epithelial scales, variously altered, observed in lung tubercle, and which are shown in one of the drawings taken from a specimen of miliary tubercle. 2d. Fat-globules. 3d. Crystals of salts, of which some specimens are particularly exhibited in fig. 8 of the microscopic drawings. 4th. Portions of the destroyed tissues which sometimes assume singular shapes. 5th. Cells which also appear to belong to the old tissues; of these there is a specimen taken from tubercles of the heart in fig. 7, and the fig. 12 of Scherer's plate probably represents similar cells. 6th. Large granular and corpuscular masses of the most irregular forms." (pp. 49, 50.)

It thus appears, that there is no essential difference discernible in tubercles from various parts as to their microscopic constitution; while the products of normal inflammation and parasitic growths are readily distinguishable from tubercular deposits. In the latter, the formative power is deficient, and the cells which are formed either remain abortive, or granular corpuscles are developed instead of cells. In expressing this opinion, Dr. Glover only corroborates a similar doctrine enunciated by various pathologists.

The chemical examination of scrofulous products. Dr. Glover takes occasion to notice in detail all the more recent and trustworthy analyses of

tubercular deposits, and then recounts a series of original analyses, twelve altogether, both of scrofulous deposit and pus. We subjoin the results of an analysis of 200 grains of crude mesenteric tubercle, after being freed from everything which had the appearance of ordinary tissue:

"The substance dried, furnished	37·5 grs.
It was therefore composed of—Water	812·5
Solids	187·5
	1000·0

"The fats extracted by alcohol and ether, amounted to 7·2 grs., which burnt, gave 0·45 of saline residue; after the exhaustion by spirit of wine and water, there remained a residuum of 22·30 grs., and the half of the extractive matter removed in this manner, burnt, gave 0·25 of salts, 5 grs. of the protein residue, burnt for salts, gave 0·125 or 2·5 per cent. of ash.* No traces of casein or pyin were discovered. The proximate composition would then be:

Fats		6·75
Extractive matter sol. in spirits of wine and water, and loss		7·50
Salts	Chlorides	0·45
	Earthy salts (phosphates)	0·58
	Alkaline salts	0·50
	Proteine residue	21·72
		37·50." (p. 73.)

A mass of crude tubercle (Analysis 4) which weighed 500 grains, taken from a woman dead of phthisis, was selected on account of the completeness of the tuberculization. It was found to contain 786·4 parts water per thousand, and the proximate analysis was:

Fats and extractive substances by alcohol and ether, with salts	18·25
Spirituous extract, and loss	4·00
Watery extract and salts	6·55
Proteine residue and salts	78·00
	106·80

A mass of tubercle from the bronchial glands contained 25·8 per cent. of solid constituents, the composition of which was:

Fat acids	2·70
Fatty body, non saponifiable—cholesterin (?)	1·00
Extractive matter, sol. in alcohol	1·30
Extractive matter, by spirit of wine and hot water	4·10
Insoluble proteine, residue, and loss	15·00
Salts	1·70
	25·80

A tuberculated cervical gland of an ox, which contained cretaceous fragments, weighed 25 grains when dried, of which 3·85 grains were phosphate of lime. A portion of deposit from the upper part of a tuberculous lung, "exhibiting what appeared to be a partial change to the cretaceous substance," was found to consist of:

Animal matter	24·80
Salts	9·00
	33·80
Chlorides, with a trace of alkaline salts	1 22
Salts soluble in water, as above described	1·90
Phosphate of lime, a trace, and loss	0·48
	9·00

* Three grs. were tested for casein with acids; it dissolved readily by the aid of heat in acetic muriatic, and strong sulphuric acids.

With regard to the results of the analyses of pus, Dr. Glover observes that scrofulous pus appears to differ from ordinary pus, chiefly in the fluid part being thinner and mixed with albuminous granules, proceeding from a decomposition of scrofulous or tuberculous matter. The pus-globules appear also, as stated by Mr. Gulliver, to be fewer and less distinct than those of healthy pus. He found them also more irregular in their form.

State of the blood in scrofula. Dr. Glover here again reviews the facts and opinions promulgated by his predecessors, and then details a series of original analyses after the method of Andral and Gavarret, of the blood of scrofulous persons, namely 11 males and 17 females. The means resulting from the analysis of the blood of males was found to be—solids 208·05, fibrin 3·132, solids of serum 87·60, globules 117·32. We subjoin Dr. Glover's reflections and deductions:

"On the whole, therefore, it may be stated, as the result of these analyses, that in scrofula we have an increase in the solids of the serum, and a diminution of blood-globules, which is very nearly the alteration that has been long suspected to exist. As far as the analyses go, the fats are not deficient in the blood, and however opposed this may be to certain hypothetical notions, with regard to the supposed *modus operandi* of cod-liver oil, it is altogether in harmony with the results of the analysis of tubercle which we have made. The experiments of Ascherson have been supposed to prove the capability of fats introduced into the blood, to work up mechanically the excess of serous matter into blood-globules, and so counteract the tendency to the expulsion of this serous matter, and its excretion into the tissues under the form of tubercle. But we find a large portion of fats in tubercles; in one analysis, conducted on a sufficient scale, almost one fourth of the tubercular matter was found composed of fats. And this result is not much in discordance with other analyses, and with analyses by other persons. If then the diseased matter be expelled by a supposed effort of the vis medicatrix, it would appear as if the sanative materials were also expelled. But this theory depending upon the tendency of an albuminous liquid to form spherical globules in contact with oil, is altogether too mechanical, and vitiates itself by giving a too easy explanation of great difficulties." (p. 115.)

The condition of the bile, lymph, and chyle, in scrofula, are not elucidated by any original researches instituted by Dr. Glover; there is, however, as usual, a succinct summary of the current facts and opinions, but which are meagre and unsatisfactory. The condition of the urine in scrofula was investigated in nine cases. Case 1.—A boy, aged 15, with swelling and suppuration of the cervical glands. The patient took syrup of iodide of iron (drachm dose thrice daily) and used frictions of hydriodate of potass ointment on the tumours. After using them six weeks, iodine was found present in the urine in large quantity, and also traces of iron; and in fact in all the other analyses of the urine of patients using iodine or its preparations, that substance abounded in the urine. Otherwise, the analyses do not show any particular change in the composition of the urine of scrofulous patients.

The scrofulous diathesis and its external signs occupy a chapter of Dr. Glover's work. On this head we do not find much novelty. In 126 cases which Dr. Glover examined, 86 had light hair and complexion, and the remaining 40 were dark. But the inmates of three workhouses situate in the districts which furnished the cases, bore nearly the same proportion of light and dark complexions, there being 97 of the former and 47

of the latter amongst 144 individuals. So that in point of fact the light-haired scrofulous cases were in the greater proportion, because the light-haired race was in the greater proportion amongst the general population.

Scrofulous diseases exist to a considerable extent amongst brutes, and this circumstance has led Dr. Glover to devote a chapter of his work to the comparative pathology of scrofula. It is wholly, however, a summary of facts and opinions already published.

The question as to the identity of scrofulous and tubercular diseases is maintained in the affirmative by Dr. Glover, and we think in the most satisfactory manner. As we have already gone over this question in our review of Mr. Phillips's work, and as we think by far the larger proportion of practitioners treat the question as already settled in accordance with the views there expressed, we think it unnecessary to revert to it. Those, however, who wish for a concise statement of the argument, will find it well made by Dr. Glover.

As to the essential nature of scrofula—that is to say, the actual diseased process, Dr. Glover defines it to consist in "a peculiar modification of inflammation, whereby the usual, or, as they may be termed, the normal products of this process are not evolved, but instead of them other materials incapable of passing into the regular cell-forms, and which constitute the substance already described under the name of scrofulous or tubercular matter. The peculiarity of this formation, and the continuance of the scrofulous diathesis, are the causes of the characters assumed by the various after-processes which result from the existence of tubercles." The word "peculiar" might well be left out, but we have not space to go over the lucid theoretical discussion Dr. Glover enters upon in support of his views: we subjoin the following, however, as an illustration of his method, and the record of an interesting fact:

"Among the arguments usually advanced in favour of the production of tubercles by inflammation, is the formation of them by artificial means. We have produced them in this way in the rabbit and dog, chiefly in order to examine them by the microscope. The mode of experimenting adopted was by incising the trachea, and injecting a quantity of mercury downward into the lungs. The appearance of the bodies which resulted, the animal being killed at a period of from one to two months after the operation, was not externally unlike that of tubercle; little round whitish masses, more or less agglomerated; and each nodule with a globule of mercury in its centre, around which the exuded matter was formed. Pus existed in some parts around the artificial tubercles; but on examining the broken-up bodies by the microscope, although the structure of portions did not appear very different from that of the irregular granules and corpuscles of tubercle, yet the exudation-corpuscles were tolerably numerous, and those towards the edges of the exudation fully formed. Numerous nucleated cells also were found mixed with the mass; in short the formation more nearly resembled an ordinary inflammatory product, such as we find in pulmonary hepatization." (p. 195.)

In this discussion of the etiology of Scrofula, we again find our views corroborated by Dr. Glover. He states truly and with great candour, that Mr. Phillips's work contains by far the best account of the etiology of scrofula ever yet given to the world. We are, however, bound to say, that highly as we have estimated Mr. Phillips's work, we think Dr. Glover has discussed certain points in the etiology with even more philosopical pre-

cision than Mr. Phillips: we refer more particularly to the hereditary transmission of scrofula. "Surely," he observes, "the *fact* that in many instances scrofula is hereditarily transmitted does not now depend upon the researches or opinions of any one individual!" The following is perfectly sound, and is followed by illustrative cases:

"That family peculiarities may pass over one generation and appear in another, is a fact too well established to be denied. Now, as the transmission of the actual disease is admitted to be a very rare event; and as in the various questions connected with this part of the subject, the *real question* is about the diathesis or predisposition, we cannot limit the investigation merely to the parents and offspring. The existence of tuberculous affections also in several of the children, where no adequate occasional causes can be discovered, is, even where the parents are free, a suspicious circumstance. To limit the investigation also to external tubercle, would be as unphilosophical on the view, advocated here, of the ultimate nature of the pathological connexion between the various forms of tubercle. How, upon this principle, could we pronounce that the disease was not hereditary, where we found that the parent of a scrofulous child had died of phthisis?" (pp. 203-4.)

The treatment of scrofula is discussed by Dr. Glover under the two heads *hygienic* and *medicinal.* As to the first, we have nothing new; good nursing in infancy, a good physical education in youth, care in the selection of a matrimonial partner at manhood,—these are the principal views of the hygiene of scrofula. With regard to the medicinal, we subjoin the following words:

"Where the tongue is furred, or spotted with reddish maculæ, the pulse quick and irritable, and the bowels constipated, a state very common at the commencement of scrofulous tuberculization, benefit will be often derived from the use of hydrarg. c. creta, in a dose of three or four grains twice a day, along with seven or eight grains of magnesia, combined sometimes with rhubarb. In some cases we have seen enlargements of the glands disappear under this treatment, without the use of other remedies." (p. 240.)

We agree with Dr. Glover in his statement, that the total proscription of mercury is as unwarrantable as the exclusive use of iodine, or cod-liver oil.

"In truth considerable analogy exists between the action of mercury and that of iodine: the compounds of both substances are powerfully irritant in large doses, tonic and alterative in small doses, and given so as to act constitutionally, appear to have the power of greatly accelerating and increasing the destructive digestion of the tissues; and both, when pushed to excess, give rise to dangerous constitutional effects connected with a peculiar erethism. Mercury is undoubtedly the more powerful remedy of the two. But in our practice we are not deterred by chimeras from the use of mercury in scrofula. Indeed, the most potent remedy which we possess for effecting the removal of a scrofulous tumour, is in our opinion the sub-iodide of mercury, a substance whose powers of salivation have been strangely doubted; but the properties of compounds follow the rule of the base, rather than that of the electro-negative element; and the sub-iodide is a true mercurial compound." (p. 241.)

With regard to the compounds of iodine, bromine, and chlorine, it is the opinion of Dr. Glover, founded on experiments, that they are active in proportion to their solubility and facility of decomposition. Tonics are admissible in every stage; Dr. Glover combines calumba with the saccharine carbonate of iron. The action of cod-liver oil is that of a tonic, due he thinks to the resinous principle it contains, and to its power in stimu-

lating the development of animal heat, (?) and to the fact that it occasionally acts as an aperient and diuretic.

Particular remedies for scrofula. Dr. Glover discusses these under seven heads, digitalis and walnut leaves heading the list, and electricity completing it. We shall only make some excerpta. Of the three non-metallic elementary bodies, which have a certain analogy of therapeutic action, as well as of chemical relation, Dr. Glover observes, that it is probable chlorine is the most potent, bromine the next, and iodine the next, judging from comparative experiments on animals, and in accordance with the great law which makes strict relation subsist between the chemical, and the physiological and medicinal properties of bodies. Bromine, in the proportion of eight or twelve minims to a pint or half a pint of water, makes an elegant lotion. Dr. Glover prefers in ordinary cases to give iodine in the form of the compound tincture of the London Pharmacopœia, beginning with 25 drops thrice a day, and increasing the dose gradually to 30 or 40 drops. Its effect is to act as a general tonic, improve the appetite and increase the quantity of solids and urea in the urine, as well as the quantity of urine itself.

With regard to the compounds of these elementary bodies, Dr. Glover is of opinion, that the chloride of potassium might be advantageously substituted for the iodide, and in a certain class of cases might be also substituted. The bromide excites less nausea than the iodide, and in a certain class of cases might be also substituted. The bromide and iodide of barium have the same physiological properties as the chloride; Dr. Glover details a case confirmatory of a statement in a recent number of this Journal, that the iodide of barium is apt to act too energetically on the uterine system. We have observed, that the iodide of potassium has this effect occasionally, and that menstruation has apparently been induced by it after a long interval of inaction.

Dr. Glover thinks the value of sea-air and sea-water as remedies in scrofula, may be in some degree dependent upon the chlorine in the former and the chlorides in the latter. To ascertain the presence of chlorine in the atmosphere, Dr. Glover forced a quantity of air through a vessel containing a solution of nitrate of silver for several minutes, the result, was (the atmosphere being very calm, and the slight wind not directly from the sea, at about thirty paces from the sea-shore) a distinct milkiness of the water, although there was no precipitate. In England, the mineral waters of Shap and Shotley may be useful in scrofula on account of the large quantity of chlorides they contain. Cod-liver oil is recommended in cachectic cases. Dr. Glover says, "we have weighed (by 'we' meaning *we* suppose 'I') phthisical patients and others, taking it, from time to time, and have found them sometimes grow stouter, even when the disease was unchecked."

An appendix of cases closes the work.

We feel we ought to congratulate Dr. Glover on the able manner in which he has handled his subject. We cannot give his book higher praise than to say that it may fairly bear comparison with that of Mr. Phillips, although totally different in its method. The two volumes are complementary of each other, and both claim the attentive study of the profession.

ART. XVII.

1. *The Susruta, or System of Medicine taught by Dhanwantari, and composed by his Disciple, Susruta (in Sanscrit).* Edited by SRI MADUSUDANA GUPTA, Teacher of Medicine in the Sanscrit College; and printed by order of the Education Committee, at the Education Press.—*Calcutta,* English era 1825, Saka 1757. 8vo, pp. 378.
2. *Súsrutas. Ayurvédas, id est Medicinæ Systema, a venerabili D'hanvantare demonstratum, a Súsruta Discipulo compositum.* Nunc primum ex Sanskrita in Latinum sermonem vertit, introductionem, annotationes, et rerum indicem adjecit Dr. FRANCISCUS HESSLER.—*Erlangæ,* 1844. First Part, 8vo, pp. 206.
3. *Commentary on the Hindu System of Medicine.* By T. A. WISE, M.D., Bengal Medical Service.—*Calcutta,* 1845. 8vo, pp. 431.

DISTINGUISHED as is the present age for discoveries in science, as well as for their application to practical purposes, it is not less so for investigations respecting the manners and customs, the language and history of the civilised nations of antiquity. Among these may be instanced, the knowledge which we have attained of the hieroglyphical writings of the Egyptians, and consequently of their chronology and history, in consequence of the discoveries first made by a medical philosopher, Dr. Young, and since continued by Champollion and Leipsius, as recorded in the works of Sir G. Wilkinson, of the Chevalier Bunsen, and of others. These researches have necessarily elucidated many subjects referred to in the Bible, while its geography and natural history have in many instances been clearly made out by the remarkable resemblance, which still exists between the present Arabic names of places and of things, to those which are made use of in the ancient, but cognate Hebrew. Of this numerous instances are given in the Travels of Dr. Robinson, and in Dr. Kitto's 'Cyclopædia of Biblical Literature.' The discoveries and works of Sir Charles Fellows have clearly shown, that many of the arts in which the Greeks attained eminence, were known to and introduced into Lycia, by a still more ancient people from some eastern country. So the recent excavations in the neighbourhood of Mosul and of Bagdad, into what appeared to be only mounds of earth and heaps of stone, have revealed numerous chambers and extensive halls of palaces, having their walls of marble covered with numerous and long inscriptions, and with sculpture, which is described as original in design and finished in execution, and displaying a knowledge of the anatomy of the human frame. Some of these are stated to afford a complete history of the military art amongst the Assyrians, and prove their intimate knowledge of many of the machines of war, the invention of which has been attributed to the Greeks and Romans, such as the battering-ram, the tower moving on wheels, the catapult, &c. (Athenæum, 1846, p. 1047.) So the cuneiform inscriptions of Persia, which so long resisted the corroding effects of time and all the efforts of the learned, have at length been translated by Major Rawlinson (Journ. Asiatic Soc., Sept. 1846), and found to be chiefly of the age of Cyrus the Great, of Darius, and of his son Xerxes, and in a language which bears a strong affinity to the Sanscrit of the Vedas, "but

is at the same time distinguished from it by that uniform permutation, both of alphabetical powers and of grammatical inflexions, which points to a very remote period for their common separation from the parent stock." These discoveries Major Rawlinson believes are but a prelude to others of far greater moment, alluding probably to the Median and Babylonian inscriptions. If we look still further east, we find that, long as Sanscrit has been studied, it is only within the last fifteen years that the inscriptions cut into the solid rock or engraved on metallic pillars have been deciphered by the late lamented James Prinsep. These are in the ancient Pali language, of which the old Sanscrit is the type. History is much interested in this discovery, for the deciphering of innumerable coins of ancient Bactria, commencing with the third of the Seleucidæ and his known successors, and continuing for many centuries, has revealed a series of unrecorded Bactrian princes, and given some dates to the loose and inaccurate chronology of India. But the history of medicine is also much interested in this question, as one of the earliest of the inscriptions, and which bears indubitable proofs of the third century before the Christian era, is a *medical edict*, promulgated through the provinces of north-western India.

The history of medicine is therefore not unconnected with these investigations. That medicine was early practised by the Egyptians, Assyrians, and Persians is well known, and will be readily admitted when people become acquainted with the high perfection which many of the arts of life had attained among these early civilized nations. We know also that the nations of the East had early established for themselves so high a character for knowledge and wisdom, as to have been visited for the purposes of instruction, and during a series of ages, by the sages of Greece, as by Democritus, Pythagoras, and others. These on their return home established schools, from which new systems of philosophy and discoveries in science were promulgated. Information on the subject of medicine is so extremely scanty, as to give rise to doubts whether it could have made any greater progress than may be observed among some rude and uncultivated nations. Medicine is, however, considered by some to have originated in Egypt, and at all events that the Greeks obtained their first knowledge of it from that country; and, as Herodotus mentions that certain diseases were treated by particular persons, the fact would indicate that some division, and necessarily some progress, had already been made in the profession. They were said also to have possessed a kind of Egyptian encyclopædia of medicine in the books of Hermes, as these are stated to have treated—1, of anatomy; 2, diseases; 3, instruments; 4, remedies; 5, diseases of the eye; and 6, of the diseases of women. Whether this be authentic or not, there can be no doubt that Egypt was considered fertile in drugs in the time of Homer; and we have continued to talk even to our own times of Thebaic preparations, and of one of copper as "mel Ægyptiacum." So the accounts given of the Assyrian practice of physic mention it as consisting chiefly of incantations or of the prescription of precious stones. But it must be remembered, that this is an account given by those who are unacquainted with a subject, and who very generally are apt to take their own ignorance as the measure, by which they attempt to gauge the knowledge of others. Taking, however, material substances, which are

confined to particular soils and climates, we may observe, as in the case of the Egyptians, that many of those produced in Persia were early employed by the Greeks; as, for instance, the several fetid gum-resins which still hold their place in the Materia Medica of the day; also some others, as cuminum, *kumoon;* cannabis, *kinnub;* capparis, *kibbur;* crocus, *koorkum;* carthamus, *koortum;* harmala, *hoormul;* moly, *molee;* susinum, *sosun;* narcissus, *nurgus.* Some of these, again, have travelled southwards, and become known to the Hindoos: assafœtida, for instance, is mentioned in some early Sanscrit works and dictionaries. It is curious in these early accounts of medicine to find so little, if any, notice of the natives of India. This we can account for only by the Hindoos being so far removed, and from their having, on one side the Egyptians, and on the other the Persians, placed between them and the Greeks, from whom we receive our principal accounts. Dr. Whewell, however, in his 'History of the Inductive Sciences,' though inclined to consider that the early efforts of the Greeks in physical speculations, and their philosophy on such subjects, owed nothing to the supposed lore of Egypt and the East, yet makes an exception, "perhaps of the Indians, as the only one of the African or Asiatic nations who ever felt the importunate curiosity with regard to the definite application of the idea of cause and effect to visible phenomena." (p. 32.) Dr. Renouard, the latest author on the history of medicine, and whose work was reviewed in the last Number of this Journal, though admitting the antiquity of the Egyptians, as well as of that of the Chinese, depreciates that of the Hindoos, and referring to some system of medicine, which he calls Vagadasastir, says: "Nulle pensée philosophique n'a présidé, à la distribution de cette encyclopédie méthodique" (p. 45); though at p. 116 he talks of the "antique civilisation Egypto-Indienne," and at p. 132, that "les Asclépiades avaient conservé jusqu'à cette époque les traditions de l'école Egypto-Indienne." But the Asclepiades themselves did not begin to argue, write, or in any way make public their information, until Pythagoras, after his return from the East and his disciples, had set them the example. M. Renouard, however, is in error in ascribing the inferiority of the Hindoos to their belonging to the Mongolian race of mankind: "il paraît que la race Mongole, à laquelle appartiennent les naturels de l'Egypte, des Indes orientales et de la Chine." Now, the Hindoos belong to the Caucasian race as much as any of the nations of Europe, and their language is ranked with the Indo-European, or, as sometimes called, Indo-Germanic languages. The great resemblance between the Greek, Latin, and Sanscrit may be seen in the appendix of an easily accessible work, that is, Dr. Pritchard's valuable 'Physical Researches.'

Though Indian medicine would not appear, as far as our present investigations have proceeded, to have been much known to foreigners, there is little doubt of its practice having been held in high estimation within the limits of their own country. Of this we have indubitable proof in the rock-engraven inscription which has been already alluded to, and which is remarkable, as not only proving the esteem in which medicine was held, but also as showing the high civilization of the governing power. For instead, as is usual with laboured proclamations employed in recording the virtues of a conqueror, or the extent of his conquests, this inscription is a medical edict intended for the benefit of the subjects of

Raja Piyadasi, who has been identified with King Asoka. It directs the establishment throughout his territories of depots of medicines, and the affording of medical aid both to men and animals; and at a period which could not have been long subsequent to the times of Alexander the Great. For it mentions the generals of one of his immediate successors as at that time ruling in some of these provinces, and of which the date is therefore inferred to be 220 years B. C.:

"Everywhere within the conquered provinces of Raja Piyadasi, the beloved of the gods, as well as in the parts occupied by the faithful, such as Chola, Pida, Satiyaputra, and Ketalaputra, even as far as Tambapanni (Ceylon),—and moreover within the dominions of Antiochus the Greek, (of which Antiochus' generals are the rulers)—everywhere the heaven-beloved Raja Piyadasi's double system of medical aid is established;—both medical aid for men, and medical aid for animals; together with medicaments of all sorts, which are suitable for men, and suitable for animals. And wherever there is no (such provision)—in all such places they are to be prepared, and to be planted: both root-drugs and herbs, wheresoever there is not (a provision of them), in all such places they shall be deposited and planted. And in the public highways, wells are to be dug, and trees to be planted, for the accommodation of men and animals."

The records of Indian medicine are not, however, reduced to such scanty notices; indeed, even in the accounts of the ancient philosophy, sciences, and arts of the Hindoos, we find numerous notices of their medical writings. That some of these are still in existence, we have proofs in the original translation and commentary mentioned at the head of this article.

Dr. Wise, in some preliminary remarks, referring to the origin of medicine, and to its subsequent improvement by the Greeks, remarks, that—

"The Grecian philosophers were assisted by the Egyptian sages, who appear to have obtained much of their knowledge from some mysterious nation of the East. Egypt, after having had her institutions destroyed by the sword of the conqueror, became the seat of Grecian learning, which was afterwards transferred to the East, where, under the fostering care of the caliphs of Bagdad, medicine was cultivated with diligence and success. It received still further additions from the East, and thus improved it was conveyed by the Mahomedan conquerors into Spain.

"Among the sacred records of the Hindus there is a system of medicine, prepared at a very early period, that appears to form no part of the medical science, and is not supposed to have enlightened the other nations of the East: a system for which the Hindus claim an antiquity far beyond the period to which the history of the heroic age is supposed to extend." (p. 1.)

Dr. Wise states that he had been induced, from an early period of his residence in Bengal, to examine the Hindoo medical Shastras, and says:

"I translated and compared what I considered the most valuable parts of different manuscripts,—it then occurred to me that the following commentary might be worthy of being published separately, as containing interesting information which had not hitherto been placed before the public. An accomplished scholar* had indeed given an interesting account of Hindu opinions regarding certain diseases: a persevering traveller† had afforded a sketch of certain opinions contained in the Hindu medical Shastras, as translated into the Tibetan language; an anti-

* Professor Wilson, Trans. Med. and Phys. Society of Calcutta, vol. i, and Orient. Magazine, February and March, 1823.

† Csoma de Koros, Journ. Asiatic Soc., Calcutta, January, 1835.

quarian and a distinguished physician* had given some of their peculiar opinions, as found in the medical works of the south of India; and an able lecturer† had combined all this information with important additions of his own; but a comprehensive view of their system of medicine, which it is the intention of the present work to supply, is still wanting to complete our information on the subject."

There can be no doubt of the value and importance of a work like that of Dr. Wise, but the above is hardly a sufficiently full account of the state of our previous knowledge, as Professor Wilson had, in the 'Oriental Magazine,' given us a general idea of the nature of the work of Susruta, and we had some knowledge of their Materia Medica from the investigations of Dr. W. Hunter, of Colebrook, also of Drs. Fleming, Roxburgh, and Ainslie. But it is certainly remarkable that, with the attention which was paid to early Hindoo opinions in philosophy, especially in metaphysics, as well as in science, so little inquiry should have been made, even by medical men, respecting their medicine; so that Dr. Wise be enabled to quote Sir W. Jones as asserting, "that there is no evidence that in any language of Asia there exists one original treatise on medicine considered as science."

In order to show the probability of the Hindoos having made some advance in medicine, Dr. Wise takes a short preliminary notice of their history, chronology, literature, science, and philosophy, as these "are all so combined with each other and interwoven with their theology, as to require elucidation, before the originality of their medical system can be proved, some of their theoretical notions understood, and the probable age determined in which the medical writings were prepared."

The importance of health and the difficulties of the subject no doubt induced the Hindoo sages to accumulate observations, and for the purpose probably of inducing greater reliance to be placed upon them and the advice inculcated, they ascribed both to the assistance derived from their deities. Several of these, indeed, are supposed to have possessed a knowledge of medicine, and who, from taking compassion on the sufferings of the weak and erring creatures on earth, are supposed to have communicated to a few favored mortals the means of preventing and of curing diseases. Hence the assigned origin of some of the medical writings.

1. The *Ayur Veda* is the most ancient system of medicine, and is of the highest antiquity; but fragments only of the MS. are now procurable.

2. The second work, called *Ayugranta,* is said to have been written by Siva, in the *Treta Yuga.*

3. The nature of medicines and diseases is treated of in some of the Puranas, particularly in the Ugni Purán.

4. The names of the following authors, who are said to have flourished under Yudhistira, in the beginning of the *Kali Yug,* are found in the Mahabharatta:

* Dr. Heyne, Tracts on India; and Ainslie, Materia Medica Indica.

† Royle, Essay on the Antiquity of Hindoo Medicine, London, 1837, where the above references are given more in detail, at p. 47.

Author's name.	Found.	Supposed to be irrecoverable.
Atrya	Atri Sangita	
Ugni Besa, Charaka	Charaka	
Bhila		Bhila Tantra.
Jatukarna		Jatukarna Tantra.
Parasara		Parasara Sangita.
Harita	Harita Sangita	
Karpari		Karpari Tantra.
Dhanwantari, Sushruta	Sushruta	

These works are supposed to have been prepared by different munis or sages, on the plan, in a great measure, of the original Ayur Veda. Dr. Wise gives a list of several other works now found in Hindoostan on surgery, medicine, materia medica, &c., and which he has arranged in the probable order in which they were prepared: Systems of surgery—Aupadhanaha and Aurabhra. Systems of medicine—Bhila Tantra, Jatukarna Sangita, Parasara, Harita, Bhagavata, Bhava-prakasa, Todrananda, Chakradatta, Pracharanantabali, Sarangadhara. Systems of Materia Medica—Rajanirghanta, Chakradatta, and Drabyaguna, which is a commentary on the last work. On nosology—Madhaba-Nidana. On pharmacy—Bangaja Ratnabali. On metallic preparations—Rasa Ratnakar, Rasendrachintamani, and Rasendrakalpadruma.

It is evident that a great portion of the interest and value of these Hindoo medical writings must depend upon the period at which they were written. To determine this with certainty we believe to be impossible, from the inattention of the Hindoos to correctness in their chronology. It is fortunate, however, that we can prove the existence of some of these works at a period well known both in the history of the world and in that of medicine; and that is at the establishment of the Arabian school of medicine about A. D. 773, under the patronage of the munificent caliphs of Bagdad. The late Professor Dietz, of the University of Konigsberg, seems to have been the first to prove that the Arabs were acquainted with the Hindoo works on medicine. This is recorded in his 'Analecta Medica,' Leipsiæ, 1833, a work which we have not seen, v. 'Journ. of Education,' vol. viii, p. 176, where it is stated that "Dietz proves that the later Greek physicians were acquainted with the medical works of the Hindoos, and availed themselves of their medicaments; but he more particularly shows that the Arabians were familiar with them, and extolled the healing art as practised by the Indians quite as much as that in use among the Greeks." This information Dietz seems to have derived from the chapter on Indian physicians in the Biography of Ibn Abu Osaibiâh, to which we shall immediately refer. Dr. Royle, in his essay on the 'Antiquity of Hindoo Medicine,' 1837, came to the same conclusion by an independent course of investigation, that is, while employed in identifying the Materia Medica of the East. He found in the Latin translations of Rhases and of Serapion the names of Sarak, Scarak, and Xarek in connexion with the descriptions of drugs the produce of India, and inferred that this could only be the famous Indian writer on medicine, CHARAK or CHARAKA. He further found that the chapter on Leeches by Avicenna commenced with acknowledging his obligation to Indian authorities by saying, "Indi dixerunt," and that the description was word for word that by

Susruta on the same subject, and which had been previously translated by Professor Wilson, and may be seen in Dr. Wise's Commentary at p. 177. Gildmeister, of Bonn, commenced in 1838 a work entitled 'Scriptorum Arabum de Rebus Indicis loci et opuscula inedita.' But he also seems to have derived his information from the above chapter of Ibn Abu Osaibiâh, who lived at the beginning of the 13th century, and died A.D. 1269. This has fortunately been translated by the Rev. W. Cureton, "from a MS. in the "Rich collection" in the British Museum," and is accompanied by some "Remarks on the names which occur in the preceding notices," by Professor H. H. Wilson, in the 'Journal of the Asiatic Soc.' vi, p. 105, 1841. Of the principal names mentioned, Kankah, Shanak, Mankah and Salih, Professor Wilson remarks, that "though they have all the appearance of being Indian, yet that they are names unknown in India. This he attributes to their having a local celebrity only, and, though in great practice and high repute at Bagdad, they may not have been known at Benares or Palibothra; and this was most probably the case, as their knowledge of Arabic and Persian would indicate a long extra-Indian residence. But among the books mentioned are those of Sirak the Indian, and of Sasard, in which are the symptoms of diseases, the manner of treatment, and the medicines to be used for them, and also the book called 'Yedan,' which describes the symptoms of diseases, without the modes of treatment. The first, there can be no doubt, is intended for *Charak*, the name of the oldest Sanscrit physician, and also of his book. The Kitab *Sasard* or *Sasrad* must be intended for the celebrated Indian work, the Susrut, while *Yedan* is, no doubt, intended for *Nidan*, or diagnosis which indicates one division of Indian medicine, on which not only distinct chapters, but separate treatises are written." From all which Professor Wilson concludes that "it is clear that the Charaka, the Susruta, the treatises called Nidan or Diagnosis, and others on poisons, diseases of women, and therapeutics, all familiar to Hindoo medicine, were translated and studied by the Arabs in the days of Harun and Mansur, either from the originals or translations made at a still earlier period into the language of Persia." He concludes that the astronomy and medicine of the Hindoos were cultivated by the Arabs of the eighth century, previous to their studying the works of the Greeks.

Though the Arabian school of medicine, no doubt, did good service in preserving and diffusing a knowledge of medicine for a series of ages, when Europe was so buried in ignorance, that students flocked for instruction to the schools of the Arabs in Spain; it is yet generally acknowledged that they added very little upon the whole to what was previously known. Rhases is generally considered to have been the first author who described smallpox; while Albucasis is acknowledged to have made some improvements in surgery. But the Arabs usually obtain credit for having made considerable additions to our Pharmacy and Materia Medica, as well vegetable as mineral, and to have originated Chemistry. Space will not permit us to enter fully into the question of these additions, or the sources whence they were derived, but we may mention that, in running over the list of drugs described by Rhases and by Avicenna, we perceive no less than 60 or 70 of them which are derived from India, or are mentioned in Sanscrit works; in addition to those which were known to

the Greeks. Almost all the above are enumerated in Dr. Hessler's translation of Susruta, or in the Commentary of Dr. Wise. As instances, we may mention—Ambra, Succinum. Anacardium, Semecarpus Anacardium. Bellyrici, Terminalia Bellerica. Baurach kebuli is Birunj kabulee, Embelia Ribes. Kafoor, Camphora officinalis. Cubabeh, Piper Cubeba. Cambil, Rottlera tinctoria, an anthelminthic, like cowhage. Diudar, Pinus Deodara. Deude or Dund, Croton Tiglium. Emblicus, Phyllanthus Emblica. Granum Nil, Ipomœa cærulea, an excellent purgative. Kirm-daneh is seed lac, and Look is common lac. Myrobolani, Terminalia Chebula. Meisee, described as a kind of pulse, is probably *Mash* of the Hindoos, Phaseolus Mungo. Besa or bish, Aconitum ferox. Nux moschata, Nutmeg. "Nux Indica est neregel," that is, naryul, or Cocus nucifera. Nux methel, Datura methel. Sandalus, Santalum album. Seituragi, Plumbago zeylanica. "Sheel est medicina inda sim. zingiberi," Hedychii species. "Gariophyllus, fructus arboris in insula Indiæ," Caryophyllus aromaticus. Khiar-shumbur, Cassia fistula. Tumr-hindee, Tamarindus indica. Tembul, Piper Betle. Turbit, Ipomœa Turpethum. Kholinjan, Alpinia Galanga. Zedoaria, Curcuma Zedoaria. Zurumbet, Curcuma Zerumbet.

For all these drugs, mentioned often by their Indian names, the Arabs were, no doubt, indebted to the translations of the Indian works, to which we have seen they had access. But some of them may have been previously made known to them by the commerce up the Red Sea, which brought the treasures of the East to the city of Bagdad. There is also reason to believe that translations from Hindoo works were made at periods anterior to the times of Harun-al-Rashid and of Al Mansor, the great patrons of the school of Bagdad. For Rhases, acknowledged to be one of the earliest of the Arabian writers, quotes several others who describe Indian medicines, as Badegora, Mersemai, Persianus, Sindishar, Sindya and Tabri, especially called "Indus." But there is no doubt that there was an earlier school of medicine at Jondisabour, in Persia, and that this will account for the great number of the so called Arabian physicians having been natives of the eastern part of Persia.

That it was the practice to make translations from the Sanscrit into Persian, we learn from the Baron de Sacy, who, in his account of the now well-known Sanscrit origin of the Fables of Pilpay, states that they were first translated in the sixth century, by the physician Barzouyeh, who had made two journeys to India for the purpose of learning Sanscrit, and procuring Indian books as well as medicaments and herbs. As there was constant intercourse between India and Persia, and likewise between Persia and the Greeks, it is possible that the later Greek authors might thus get acquainted with many Indian products; and hence we may account for the appearance in their works of some Indian drugs. Thus Paulus Ægineta, who lived at the end of the sixth and the beginning of the seventh century, and whose work seems to have been translated by Honain at Bagdad, has several compounds which are named Indian, and one of them by a Sanscrit word, 'Trypherum,' used by Susruta and others. Aëtius, who probably lived about the end of the fifth century, mentions "Nuces indicæ," cocoa-nuts; "zador," zedoary; and "galanga" galangal; "santalum" sandal-wood; also the fruit of Semecarpus Anacardium, and an antidote of two kinds of pepper, all of which indicate an acquaintance with, and employment of, Indian products.

Dioscorides, it is well known, describes a considerable number of Indian products, which occur also in the works of his successors down to the time of the Arabs, and which we have every reason to believe to be the same substances for which these authors in many instances give the Indian names, as spikenard, Calamus aromaticus, cinnamon, cassia, malabathrum leaf, or tamala putra, Piper nigrum, and P. longum, and the root of this plant, zingiber, *sans.* shringavera, cardamoms, turmeric, costus, agura or agila wood, converted into aloe wood, bdellium, olibanum. All of these are the produce of India or of neighbouring countries, and most of them we find constantly mentioned in Dr. Wise's Commentary. So *ονυξ*, usually translated Unguis odoratus, seems to be the same substance as the Sanscrit *nakhi,* literally nail, and which is celebrated as a perfume in the Sanscrit Amera Cosha, and is the operculum of one or more species of univalve shells, described by the Greeks as obtained from the *nard-bearing lakes.* Some of the above drugs are mentioned in prescriptions even by Hippocrates, as the two kinds of pepper, cardamoms, ginger, cinnamon, cassia, spikenard, and Calamus aromaticus.

Seeing that so many medicines, the produce of India, and of which the properties must have been investigated by the natives of the country, had at such early periods became sufficiently famous, to have been conveyed to and employed by distant nations, it becomes an interesting question to determine what were the opinions of the Hindoos on medical subjects, and whether their practice was entitled to the esteem, in which it seems to have been held from the rock-engraven inscription.

That the physicians educated according to the then Hindoo system of medicine were esteemed even by strangers, we have the evidence in the proofs quoted by Dr. Wise. "Arrian informs us that, in the expedition of Alexander to India, the Grecian physicians found no remedy against the bites of snakes; but the Indians cured those who happened to fall under that misfortune."

"For this reason, Nearchus tells us, Alexander, having all the most skilful Indians about his person, caused proclamation to be made throughout the camp, that whoever might be bitten by one of these snakes should forthwith repair to the royal pavilion to be cured. These physicians are also said to have made other cures; but as the inhabitants have a very temperate climate, they are not subject to many varieties of disease. However, if any among them feel themselves much indisposed, they apply to their sophists (Brahmans), who, by wonderful and even more than human means, cured whatever will admit of it."

This we conceive is ample testimony to the skill of the Hindoo physicians, according to what was considered such by their Greek invaders 300 years before the Christian era. We are therefore justified in considering that medicine was studied by the Hindoos, and perhaps that some of their works were written at that early but flourishing period of their history. We have seen that the Arabs had access to, and had quoted their two principal works, and that the earliest of the Greeks were acquainted with several of their medicines; hence it has been argued that "this proves the still earlier investigation of their properties, and therefore the cultivation of medicine in the countries where alone these substances are found to grow." Professor Wilson had long since inferred "that, from the Charaka and Susruta being mentioned in the Puranas, the ninth or tenth century is the most

modern limit of our conjecture; while the style of the authors, as well as their having become the heroes of fable, indicates a long anterior date." The name of Dhanwantari is found mentioned with those of Charaka and Susruta in poems written in the time of Nala Raja, and in Charaka the names of the Munis or ancient Hindoo sages are alone enumerated, and without the mythology which is so conspicuous a feature of later works. The 'Ayur Veda' is supposed to have been written about the period of Menu's code, or about 900 years B.C., and the Vedas about the 14th century B.C. But these dates will probably become corrected, as progress is made in the present investigations into Eastern antiquities.

From these works we are taught respecting the origin of medicine. First, that the Ayur Veda, or the most ancient medical writings, are considered to form a portion of the fourth or Atharva Veda, conceived to be the work of Brahma, by whom it was communicated to Daksha. By him the two Aswins, or sons of Surya (the sun) were instructed in it; these then became the medical attendants of the gods. A genealogy, as observed by Professor Wilson, that cannot fail recalling to us the two sons of Esculapius, and their descent from Apollo. The Aswins, according to some authorities, instructed Indra, and Indra was the preceptor of DHANWANTARI, but others make CHARAKA prior to him. The pupil of Dhanwantari was SUSRUTA. The works of Charaka and Susruta are the two most ancient which now remain, with parts of the Ayur Veda in the works of commentators; but in Menu we have several notices of subjects connected with medicine.

The Ayur Veda treated of the whole science of medicine under eight different heads. 1. *Salya,* the art of extracting extraneous substances. 2. *Salakya,* the treatment of external organic affections. These two divisions are included in the surgical diseases of modern times. 3. *Kaya Chikitsa* forms what is now included under the head of medicine, as *chikitsa* means the application of the methods of cure to *kaya,* the body in general, and under it are included fevers, dysentery, diabetes, mania, leprosy, &c. 4. *Bhutavidya,* treats of the means of restoring the deranged faculties of the mind, when induced by demoniacal possession. This long continued a subject of attention in Europe, as we may see in the numerous quotations, in the chapter on the Causes of Melancholy in Burton's remarkable work. 5. *Kaumara bhritya,* included the treatment of infants from their birth, and of their diseases, with that of nurses as connected with lactation. 6. *Ajuda tantra,* treated of the administration of antidotes against poisons, whether mineral, vegetable, or animal. 7. *Rasayana tantra,* chemistry, or rather alchemy, as the chief object of the chemical combinations, which are chiefly metallurgic, was to discover the universal medicine, that was to render health permanent and life perpetual. 8, or last part. *Vajiharana tantra* professed to make known the best means of increasing the human race. We cannot avoid noticing the correspondence between this system and that of the Egyptian Encyclopædia of Medicine mentioned at p. 2. At a later period, Charaka relates that the sacred sages or Munis, being grieved at the weakness and sufferings of mankind, assembled in the Himalaya Mountains, resolved on sending one of their number to Indra in heaven, to acquire a knowledge of medicine. Bharadwaja is stated to have returned with a knowledge of *Ayur Veda.*

This was imparted by the sages to numerous pupils for the good of mankind. Several works were written; that of 'Agnibesa' was declared to be the best practical work, but after it was corrected by Charaka it received his name. He therefore became the instructor of practitioners upon earth. This forms the most ancient and the most celebrated Hindoo medical work: it, however, displays less anatomical knowledge, is obscure in the arrangement, though accurate in the description of disease, and is distinguished from more modern works by simplicity in prescription.

The work of Susruta treats—1st, of *Sutra-stana*, or surgery, including, however, observations on climate and different kinds of food as influencing health, with a description of the diseases of the humours, of prognosis, and a notice of different kinds of medicines, in 46 chapters. 2. *Nidana-sthana*, or the description and diagnosis of diseases produced by vitiated humours. The Latin translation of Dr. Hessler only includes these two parts. 3. *Chikitsa-sthana* (translated therapeutics), is treated of in 40 chapters. 5. *Kalpa-sthana*, or toxicology. 6. A supplementary section, including various local diseases. The whole was printed at Calcutta by the Government Educational Committee, in the original Sanscrit.

Dr. Wise has compiled a very useful and interesting commentary, on the knowledge of medicine displayed in the works now procurable in India; and he acknowledges that,

"For arriving at the true meaning of words and expressions, I have had the assistance of able Pundits; of these, I must particularly mention the assistance I have derived from Abhaycharan Tarkapanchánan, now superintendent of the Bengali department of the College of Mohammed Mohsem at Hoogly, and of Madhusudan Gupta, lecturer of anatomy to the Medical College, Calcutta, whose accurate knowledge of the medical Shástras is combined with an extensive knowledge of the sciences of Europe."

Dr. Wise has unfortunately not sufficiently distinguished what he has extracted from ancient Sanscrit medical writings, from that which occurs only in the works of comparatively modern Bengalee commentators. He indeed makes an apology for the opinions of the moderns having in one or two places been stated with those of the ancients. The instances, however, we fear, are more numerous than a reader would be led to expect, from the above form of expression. This we are unfortunately unable to verify, from unacquaintance with the original Sanscrit, as well as with the Bengalee commentaries; but observing several instances in which diseases and remedies are differently named from what we had been accustomed to consider Sanscrit nomenclature, we begged for an explanation of the difficulty from a celebrated Sanscrit scholar. He has been kind enough to point out several instances in which the Bengalee methods of spelling have been substituted for that of the correct and classical Sanscrit, especially in the substitution of *b* for *v*, and *o* for *a*, as is usual in Bengalee works; as, for instance, the name of an author *Bhavaprakasa* is spelt *Baboprakasa*, *Dravya*, substance, is named *Drabya*, elements; *Chyavana* is *Chyabana; Daksha* the Prajapati is called *Daka*; and the Rishi *Obree* must be *Atree*. *Amassia* and *Puckasia*, receptacles for undigested and digested food, should be written Am-asaya and *Pák-asaya*. The bile, p. 44, is described as being of a blue colour, the word no doubt is *nila*, signifying black.

Dr. Wise also regrets "to find many errors in the orthography of names."

These errors, he states, "would not have occurred had I not been obliged on account of ill health to leave India, and had I not, on my return to this country (that is, India), been stationed at a distance from Calcutta as the work passed through the press." This must form some apology for the numerous typographical errors which occur, many of which, however, we think, might have been avoided if any one of Dr. Wise's medical brethren had read the proof-sheets. We should not have noticed these errors so prominently, if they did not seriously interfere with the value of the work.

For if Hindoo youths, necessarily but imperfectly acquainted with English, are to study the work, they will inevitably learn incorrect orthography, and be unable to attain further information, from dictionaries, &c. respecting such strangely metamorphosed words; while the European reader cannot avoid losing confidence in the parts with which he is unacquainted, when he finds so much incorrectness in ordinary terms. Taking some of the more remarkable, as, for instance, in the table of bones, at p. 53, we have *Annominata* for Innominata, *Palet* for Palate, *Enciformed* for Ensiform, *Umbiliacus* for Umbilicus, throughout the work. *Ascetics* (p. 184,) for Ascites, which is described as a disease at p. 358, by the name of Asitis, *Amenagogue* (p. 123 &c.) for Emmenagogue, *Raisin* for Resin, *Lucoreah* for Leucorrhœa, *Gravel* for gravid uterus, p. 366, and at p. 381, *seamen* discharged with the menses. All indicating that some of Dr. Wise's native friends have had the chief superintendence of the printing.

While alluding to the orthography of English words, we may at the same time state, that the scientific names are not more correctly given or always accurately applied. The author, moreover, does not sufficiently attend to the period at which Indian products became known to other nations, or to the period when foreign products were introduced into India. Thus he talks of wine being made by Noah, the first year after the debarkation from the ark, which one might suppose was mentioned in some Hindoo work. Sugar-cane is stated to be the sweet cane of Scripture brought from a far country. Now the sweetness here referred to indicates *sweetness* of smell and not of *taste*, and is generally supposed to refer to the Calamus aromaticus of the ancients. Oranges, lemons, citrons, &c., are stated, at p. 102, to have been well known to the Greeks and Romans. There is no proof that any of them were known except the citron. At pp. 416 and 417 we have the pine apple and custard apple mentioned, and at p. 274, potatoes recommended as an article of diet. All these are well known to be American products, and of comparative modern introduction. On the contrary, we have European products mentioned as if occurring in these Hindoo works, and all with Sanscrit names applied to them, but without any proof of their having been known at early times. Thus various names, as tikta, caturohini, ushira, katuka, are translated hellebore, which is itself spelt in various ways. So *Vacha* or *Bacha* is in some places translated orris root, and, at p. 428, *Iris germanica*, but at p. 309, correctly *Acorus calamus;* it is known everywhere in India by the name of *buch*. But we cannot conclude these observations without adverting to Dr. Wise's plan of giving the Sanscrit name along with the English or scientific appellation, as a very excellent one, and worthy of adoption in all translations of such works.

From these blemishes in Dr. Wise's work, we proceed to notice the

great mass of curious information which has been incorporated in his Commentary, and which would be much more valuable for tracing the history of medicine, if he had in all cases, as he has done in many, given the name of the original Sanscrit or Hindoo author, whence he derived his information. As in the early ages of Grecian medicine, so in that of the Hindoos, medicine was studied by philosophers who treated of the origin of all things, as well as of the body in particular, its diseases and treatment. Thus—

"At an early period the Hindoo philosophers reduced the material world to five elementary principles and primary qualities, by the agency of which they explained the appearance, composition, and condition of the world, and the structure and functions of the body." (Wise, p. 30.)

The five elementary principles they considered to be earth, water, air, fire and ether, which, as stated by Dr. Wise (p. 31), "was supposed to be separated from the others, and to possess the property of sound and form;" to be "altogether undistinguishable by our senses, and is only made known to us through the evidence of our understanding." But a high Sanscrit authority informs us that the fifth element is properly considered to be equivalent to space, and to be the element that keeps the molecules of bodies from cohesion. We proceed with a few more extracts from this part of the work:

"These elements are all nourishing to the body, and are contained in different proportions in every part of food, so that after digestion each element, by an inherent property, joins with that which already forms a part of the fabric of the body........There being both an active or warm, and a passive or cold principle, which are increased and strengthened by the rays of the sun and moon."

Each of the fluids is also supposed to be influenced by one of the seven planets, which regulates its condition.

All living bodies, among which vegetables are included, are supposed to be composed of the above elements, "with the element-producing action or life superadded. Living bodies are produced from vapour, vegetation, incubation, and parturition, as insects, plants, fishes, reptiles, birds, and animals."

The essential or elementary parts when mixed, form vital bodies, which are divided into two classes. One stationary, the other moveable. This class is divided into four groups: 1, such as are produced from the womb; 2, from eggs; 3, from the warmth of the earth; and 4, from what is written "udjbidgo, such as break their habitation," but we are informed the word is derived from *udbhad*, sprouting, and *ja*, born from, and should be *udbhijja.* Plants are likewise divided into four groups: 1. Trees with fruit but without flowers. 2. Trees with both flowers and fruit. 3. Creepers. 4. Annuals.

"The same elements and quantities, by their combination and action, constitute the human body, which is governed by an independent principle, or soul, which acts through the medium of the members, and is an emanation from the great soul of the world, into which, after certain purifications, it is again absorbed. As long as the soul remains in connexion with the body, the diseases with which it is afflicted may be removed, and it is proper that, during all this time, remedies should be employed for the purpose." (Wise, p. 32.)

From the above few extracts, we perceive that the Hindoos were careful

and attentive observers, and that in their general views, however incorrect, may be perceived the germs of many theories which have prevailed at different times. Without adverting to their mathematical knowledge or their systems of philosophy, we may judge from their observations of nature that they were quite competent to make advances in medicine, which requires careful observation, correct induction, and well-considered generalization. We shall proceed to give specimens of their information and opinions respecting different departments of medicine, and notice how far they were acquainted with the accessory sciences, such as chemistry. In comparing these opinions with those of other ancient nations, we must not forget that even among the early Greeks there was no knowledge of anatomy, except that of the bones; that their physiology was fanciful, but that they had much valuable information on the subjects of climate, of hygiene, and of diet. The same we find to be the case with the early Hindoos. We shall first, however, give their notions on the character and duties of a physician.

The Hindoo physicians now form the caste of Vaidhyas (or those who understand the Vidya or Ayur Veda). It is stated that "Brahmans learn the medical Shastras for their advantage, Khetryas (or the military class), for the benefit of their health, and Vaidhyas for their subsistence." A good teacher is like rain falling upon the germinating seed, and should possess the following qualifications: a perfect knowledge of all Shastras, joined to extensive practical knowledge and skill, &c. The medical student should be the son of a respectable and ancient family, who is either the son of a practitioner, or of one who respects the medical profession," &c.

"There are four circumstances required in the cure of a disease:—a physician; a disease that is known; a reasonable patient; and medicines, instruments, and attendants. The acquirements of a good physician are described as consisting, first, in a knowledge of books, without which he will be confused, like a soldier afraid in the time of action; on the other hand, a want of practical knowledge will impede his advancement, and his senses will be bewildered when called on to treat acute diseases." (p. 16.)

"When such a Vaidya is spoken to by a patient in a peevish and hasty manner, he will remain calm, mild, and courageous, and cherish a cheerful hope of being able to save the sufferer's life." (p. 18.)

Pretenders to a knowledge of medicine were known in those days as at the present time.

"They are sometimes allowed to practise by the neglect of the Rajah, and they may be known by their vanity and ill-will towards the good physician. Such persons avoid the society of learned persons as they would a jungle." (p. 19.)

In visiting a patient, the physician is directed "to observe the state of the planets, the time of day, and the good and bad omens." He should then proceed to "ask questions of the attendant regarding the disease, what things he has eaten, and what he has done to produce or to influence the disease." He "should then mark the signs of longevity in his patient, and proceed next to mark the nature of the disease, and which of the humours is diseased, and how they can be cured. He should examine the symptoms of the disease with his eyes, and consider the probable result of the disease by his judgment. The symptoms enu-

merated in the Shastras should be observed, more especially the state of pulse, of the tongue, as to moisture and dryness, the condition of the bowels, urine, and sleep; his general feeling, more especially the state of the nose, head, hands, feet, and abdomen. By the touch is distinguished the feverish heat or coldness of the surface, the dryness, moisture, &c. By the *hearing*, the passage of air in deep-seated abscesses, wounds, and in the intestines by coughing, &c." By speech, the practitioner learns the time of invasion and progress of the disease, &c. (p. 24.)

"As long as life remains in the root of the throat, and the senses remain perfect the physician may give medicines, as the persons, under such circumstances, may be cured." The physician is, however, to be careful of his own reputation, as well as of the credit of his profession.

"It is proper, however, that much caution be used in the employment of medicine in fatal diseases, as a physician may alleviate pain but cannot give life; and by administering medicine in such cases, without previously stating the danger the patient is in to his relatives, he will bring discredit on himself and on his profession. Taking such precautions, the practitioner may give medicines even when the patient is senseless, without any pulse and any breathing."

Some directions are then given about the collection and preparation of medicines; and the chapter concludes with the recompence of the physician.

"The messenger should always offer a present to the physician. Before the patient takes the medicine, the god of physic is to be worshipped in the person of his deputy, the physician, who must be well paid for his services.

"When a physician has cured a disease he is entitled to the usual gifts for the performance of a good action. These will vary with the rank and condition of the patient. Money will be the recompence bestowed by the rich; friendship, reputation, increase of virtue, prayers, and gratitude will be that of the poor. When a Guru, a Brahman, or a Dandi (fakir), a relative, a humble and good friend, or one without relatives, consults a physician, he must not accept of any pecuniary recompence. His reward in such cases will be an increase of knowledge, and the gratification of his desires in having an opportunity of performing a good action." (p. 29.)

Chemistry is usually supposed to have originated with the Arabs, but without referring to alchemy, there can be no doubt that this science originated with, or was known to the Hindoos previous to the time of the Arabs; and we will take Le Clerc's distinction, "qu'il faut bien distinguer entre la chimie qui enseigne la mélioration ou la transmutation des métaux, &c., et celle qui n'a pour but que la préparation des médicamens, et dont l'objet est la santé."

Even in this view we find many chemical preparations were prepared; thus, to make caustics (p. 181), the ashes of different plants, which we know must have yielded carbonate of potash, are directed to be dissolved in water, and "some shell-lime is then to be mixed with them." The process is the same as that for making solution of potash, according to the Pharmacopœia. Common salt, carbonate of soda, borax, nitre, and sal-ammoniac were well known; to the last, lime-water is directed to be added, in a bag (probably a leathern water-bag), to prepare, what must have been, a solution of ammonia. Iron and tin are stated (Wise, p. 117) to be the only metals which were prescribed internally by the more ancient Hindoo physicians; but in the translation of Susruta, p. 95, we find stannum, plumbum, cuprum, argentum, lapis magnes, aurum, et rubigo ferri. "At

a later period, mercury, gold, silver, copper, lead, and zinc were introduced and several preparations made from them." Metals are, however, usually prescribed only in the form of preparations; the sulphuret of antimony, as well as that of arsenic and arsenious acid seem to have been used at very early periods; as in the translation of Susruta, pp. 85 and 95. We have also the three kinds of vitriol, or the sulphates of iron, of copper, and of zinc, prescribed in different diseases; and directions are given for the making of various preparations, especially of mercury. But here we have again to notice carelessness in nomenclature, with errors in the substances named. Thus cinnabar, or red sulphuret of mercury, is directed to be made with sulphur and *blacklead*. The sulphuret is sometimes called sulphate, but most frequently sulphurate of antimony. Kusees is translated sulphate of iron at p .166, and sulphate of zinc at p. 302. Accurate translations of some of the earliest formulæ for making these chemical preparations, would be extremely interesting as enabling us better to trace the history of chemistry.

The Hindoo Pharmacy may now be noticed, and appears to be sufficiently complete, as weights and measures are attended to, and medicines are apportioned according to age, and prescribed in the form of powders, pills, fresh juices, pastes, decoctions, and infusions; extracts, roasted medicines, electuaries, oils, and spirituous mixtures. The menstrua in which medicines are to be given are water, honey, sugar, &c., or such substances as speedily act on the body.

"The time for administering medicine is important, some requiring to be given before, others during, and a third after eating. The general opinion is, that medicines should be taken on an empty stomach, as it is then soon digested, and like a drop of oil let fall upon water, is taken into the system and diffused quickly over it.

"Medicines given in too small doses will be like throwing a little water upon a large fire, that rather increases than diminishes it. In like manner, too large doses of medicine will increase the disease, and will be liable to produce other diseases."

Having seen that not only the Arabs but also the Greeks had been indebted for many of their drugs to India, we should naturally expect to find these noticed in their medical works. In this we shall not be disappointed. Indeed we find nearly all those which we have already noticed as having been known to the Greeks and Arabs. There are also many others which still continue to be used by native practitioners in India, and which are possessed of useful and active properties. Among all these the only exotic drug which we observe is assafœtida. Dr. Wise states, that Charaka arranges simple medicines under forty-five heads. Many of the groups are intended only to relieve particular symptoms; others as sedatives, stimulants, tonics, emetics, purgatives, &c., are such as we still continue to employ. Susruta divides medicines into two classes: those which evacuate bad humours from the body, as purgatives and emetics; and those which diminish the exalted action of the humours, and restore them to the healthy state. But he also arranges them under thirty-seven different heads, which are given in the translation of Dr. Hessler. Many of these divisions, as those of Charaka, are intended for the treatment of particular diseases or of symptoms only. Subsequently, (chap. 43,) he treats of emetics, and among the principal of these enumerates Vangueria

spinosa, which is interesting as belonging to the same natural family as ipecacuanha; Asclepias geminata, azadirachta, the Indian species of mustard, bitter cucurbitaceæ, fossil salt, and others. In the following chapter, (xliv.) he treats of purgatives. Among these we find danti (not translated by Dr. Hessler) or Croton polyandrum, a substitute for Croton Tiglium, Ricinus communis, Cassia fistula, purgative cucurbitaceæ, the root of Convolvulus Turpethum, and the seeds of Ipomœa cærulea, both excellent purgatives, myrobolans, the juice of Euphorbia antiquorum, others less known, with some milder purgatives, as sugar, fruits, &c. These are usually combined with some warm aromatics, such as pepper, ginger, and the Costus so well known to the ancients. This Dr. Wise translates Costus speciosus, an inert substance, and Dr. Hessler Costus arabicus, but which being still in use in India, and known by the name of *koot* and *koosta*, was proved by Dr. Royle to be this costus, and traced by Dr. Falconer into Cashmere, where he found it growing on the mountains, and which, as belonging to a new genus, he named Aucklandia Costus verus. Errhines were much employed, as exciting a discharge, by which the head was supposed to be cleared from the presence of bad humours. Dr. Wise says, "few narcotics seem to have been known, except the species of datura and the resin of hemp, or Cannabis indica." But they employed Aconitum ferox, bish; Kakola is Cocculus indicus, and Kaephul may have been Strychnos Nux Vomica. Among the anthelminthics we observe viranga or biranga frequently mentioned, which Dr. Wise does not translate, but which Dr. Hessler correctly renders Embelia Ribes. Of this the small berries, something resembling those of black pepper, are still employed as anthelminthics in different parts of India. We also find in Susruta, roots and seeds grouped together in fours and fives, e. g. the five smaller and the five great roots, as continued to be so long the case in Europe with many roots and seeds. Since we cannot devote more space to this subject, we may conclude with the observations which terminate the chapter on drugs. "When administered by an ignorant person, medicine is compared to poison, is like the knife, fire, or lightning; but when administered with the necessary knowlege, medicine is like amrita, or the water of immortality." (Wise, p. 157.)

As none of the nations of antiquity were well acquainted with anatomy, it is not to be expected that the Hindoos had acquired any great knowledge of the structure of the human body. They did not, however, entirely neglect it, and their mode of dissection is certainly curious and original, if not the most satisfactory. Charaka states that a practitioner should know all the parts of the body, both external and internal, and their relative positions with regard to each other. Susruta states, in addition, that it is by combining a knowledge of books, with practical dissection, that he will attain a knowledge of his profession, and possess an intimate acquaintance with the diseases to which the body is liable, and perform surgical operations, so as to avoid the vital parts.

"The body which is to be examined by dissection, should be that of a person who had neither been destroyed by poison, nor had died of a long disease, should not have been very old, and all the members perfect. When a proper body for the purpose has been selected, the dejections are to be removed, the body washed and placed in a framework of wood, properly secured by means of grass, hemp, or the like. The body is then

to be placed in still water, in a situation in which it will not be destroyed by birds, fishes, or animals. It is to remain for some days in the water, when it will have become putrid. It is then to be removed to a convenient situation, and with a brush made of reeds, hair, or bamboo-work, the body is to be rubbed, so as by degrees to exhibit the skin, flesh, &c. which are each in their turn to be observed, before being removed." (Wise, p. 69.)

The body is said to consist of humours, that is, of air, bile, and phlegm, (which are described as the three pillars or supports of the system,) and of the seven essential parts, consisting of the hard and soft parts and fluids of the body, as the chyle, blood, flesh, fat, bone, marrow, and semen. But osteology, as with the most ancient Greek physicians, is the only part that seems to have been correctly ascertained. The number of bones are stated by Charaka, including some cartilages, to be 306, while Susruta states them to be 300 in number. The different forms of bones and the several kinds of joints are enumerated. Ligaments are described as binding together the bones, as strips of ratan bind together the pieces of a boat. "Muscles cover, strengthen, and retain in their place vessels, tendons, bones, and joints." Much absurdity is related respecting the vessels, which are said all to originate from the navel. They "are like decayed leaves, in which the interstices have been removed" but "nourish the body as a garden is irrigated by a small brook." Besides these, canals, fascia, organs, receptacles, and the orifices of the body are noticed. The vital parts are named and described by Susruta, and the necessity of avoiding them in operations pointed out. Thus, if the vital parts in the palm of the hand are wounded, the arm is to be amputated, to save the individual's life. So if the urinary bladder be wounded, the person will soon die, except after the extraction of the stone. If the bone of the head or breast be broken, it is to be raised by the assistance of instruments. Some wounds in vital parts prove fatal on the withdrawal of the instrument inflicting them, others, if the fatal termination is at first prevented, will prove fatal after some days, with much suffering and weakness. Some of these consequences must no doubt have been observed from the effects of sword and spear wounds; but the whole of the information under this head indicates much greater attention to the subject of anatomy, than could have been supposed possible, by those best accquainted with the Hindoos.

Surgery, which is named Sutra sthana in the Ayur Veda (Wise, p. 8) and Shala at p. 157 (and it forms the first chapter of Susruta), seems to have been first practised by the Hindoos as it was also by the Greeks. Dhanwantari, having asked his pupils on what he should first lecture, they replied on surgery, because there were no diseases among the gods; wounds being the first injuries which required treatment. Besides, "the practice of surgery is more respected as affording immediate relief, and is connected with the practice of medicine; although the latter has no connexion with surgery," (p. 8); but at p. 158 Dhanwantari declares "that surgery cannot be practised with success unless the practitioner is familiar with the practice of medicine, of which it is only a branch." In the book of Susruta, in 46 chapters, many things are included which we should now place in other departments, as the general duties of teachers and practitioners, the diseases of the humours, of things useful or hurtful to the different humours, the difference of climates, of winds,

the several classes of medicines, and the different articles of diet. But in this book are also included the different kinds of bandages and the selection of instruments, description of some surgical diseases, the removal of extraneous substances, the restoration of defective ears and noses, the different stages of inflammation, the treatment of wounds and ulcers. Inflammation is described as of two kinds: the one produced by external injuries, and the other by derangements of the air, bile, phlegm, and blood, or their combinations. The different methods of performing venesection are described, and the parts of the body whence blood should be taken, according to different diseases, with a notice of the variety of modes in which the operation may be improperly performed. The actual and potential cautery are fully described, and seem to have been frequently employed. It is observed that, "as cutting, fire, &c. give pain, rajahs, rich people, children, or old people, and fearful and weak people, when they require to lose blood, may have leeches in preference to venesection." The several kinds of leeches are then described, and the mode of cupping with a horn out of which the air is sucked out, or with a hollow gourd, of which the air is exhausted by burning something in it.

In the 9th chapter of Susruta, the student taught science by books, is next to be instructed in the practical use of instruments, &c., for "without practical skill theoretical knowledge is of no use," or, as Dr. Hessler translates it, "Perquam edoctus enim, deficiente exercitatione, in operationibus inexercitus est." (p. 18.) The different surgical operations are to be shown to the student upon wax spread out on a board, on gourds and other soft fruits; tapping and puncturing on a leather bag of water or of soft mud; scarifications and bleeding may be practised upon the fresh hides of animals, from which the hair has been removed, or upon dead bodies, and by puncturing or lancing the hollow stalks of water lilies, or the vessels of dead animals. The removal of substances from cavities by removing the large seeds of the jack (Artocarpus integrifolia) or bel (Ægle marmelos) fruit. The extraction of teeth is to be practised upon dead bodies and animals. The mode of making noses, ears, &c., must be practised upon dead animals.

These practices are mentioned and adduced as instances of how different is not only modern from the ancient surgery of India, but as proving that the prejudices and modes of thinking respecting the dead bodies of men or animals must have been very different from anything we now know of the Hindoos. Surgical operations are described as being of 8 kinds.—1, incisions; 2, opening of abscesses; 3, scarifying parts; 4, puncturing, as in hydrocele and dropsy; 5, probing to ascertain presence of foreign substances; 6, operations of extraction, as of the stone, the fœtus ex utero, the teeth, &c.; 7, removing fluids, as pus, blood, &c.; 8, sewing parts together (Wise p. 170). But several operations which the ancient Hindoos seem to have performed are not enumerated among these. Hemorrhage is to be arrested by the use of astringents, cold, ice, caustics, and by the actual cautery. When an extremity is separated, immediately pour boiling oil on the surface, then apply a cap-formed bandage and remedies to heal the wound, (Wise, p. 188). This book concludes with the treatment of fractures and dislocations. The whole shows that at an early period the Hindoos practised many operations, and had paid attention to several of the objects of surgical science.

The first two books only of Susruta having as yet been translated by Dr. Hessler, we are confined almost entirely to Dr. Wise's Commentary for the Hindoo practice of physic. The second book, however, of Susruta, entitled Nidan sthana, usually considered to treat only of diagnosis, is entitled Pathologia, by Dr. Hessler; and we have in the sixteen chapters composing this book the opinions of Susruta on the pathology of the humours and of some diseases. Dr. Wise commences his chapter with the Hindoo opinions on disease, which are those of the humoral pathology, and says (p. 193):

"Nature, which exhibits the highest degree of order in her operations, is liable to occasional irregularities from the impurities and the imperfect manner in which the elements and qualities are mixed together. In like manner, the harmony of the humours of the body is liable to derangement. At one time the disease is owing to an increase of one of the principal humours, at another to its diminution, with regard to the other humours. They thus explained the occurrence and varieties of disease. The soul (jivita) of the body, like the great soul of the world, tended to retard these derangements, or restore such irregularities. When disorder has been introduced, the soul (vis medicatrix naturæ) tends to reduce the humours that are increased, and to augment those which are diminished. In like manner, certain medicines have peculiar effects in producing these changes, and thus assist the soul in her salutary influences.

"Disease is therefore the pain (dukkha) of the soul, caused by the derangement of the humours." (p. 194.)

Diseases were, however, unfortunately arranged, it is said, "according to their prominent symptoms, and not according to the peculiarity of the symptoms and their combinations." A modification of the classification of the Ayur Veda is said to be usually followed in Hindoo medical works.

Charaka divides diseases into three classes, mental, bodily, and accidental, which he supposed were situated in the semen, chyle, or blood. The general causes of disease are also three, proceeding from matter, or objects of sense; the second from improper exercise; and the third from the seasons. "There are also three sorts of medicines: one sort that cleanses the body, when taken internally, as emetics, purgatives, &c.; another sort purifies the external body, when applied externally, as oils, bathing, diaphoretics; and the third kind is the use of knives or instruments, fire and escharotics."

Charaka states also, that there are three objects of inquiry in the world: the first and chief inquiry being the means of preserving life; the second, the means of acquiring wealth; and, lastly, the means of obtaining beatitude in the next world.

The descriptions of disease, Dr. Wise states, are generally distinct and satisfactory; but sometimes it is difficult to distinguish the disease intended, because an accidental combination of a few symptoms seems to be sometimes mistaken for a distinct disease, and in other cases these are placed as varieties of other diseases.

"The description of a disease is usually commenced with an enumeration of the supposed causes, situation, and humours deranged, as indicated by the symptoms and the varieties produced by the humours affected" (p. 198.)

Before proceeding to a description of particular diseases, detailed indications are given of the morbid changes of the humours which enter into all diseases. (p 199.)

Under the head of diagnosis, it is stated that the nature of a disease is to be ascertained by the five senses and asking questions, as 1st, by hearing he will distinguish the state of the lungs, by the peculiar noise of the breathing, &c.

"Nothing is said in Charaka and Susruta respecting the pulse." (Wise, p. 203.)

Prognosis was a department of medicine early paid great attention to, as we see in the works of Hippocrates, and so it appears to have been by the ancient Hindoo physicians:

"As a flower prognosticates the future fruit, smoke the severity of fire, and clouds the near approach and the severity of the coming shower, so certain symptoms prognosticate the favorable or fatal result of a disease. These signs are, however, but slightly apparent to the general eye, and can only be detected by the eye of the experienced physician." (p. 205.)

Several pages are devoted to this subject, but we cannot spare space for extracts. Either Dr. Wise or his Hindoo friend concludes the chapter with observing, that some of these remarks are just, and some are too much neglected by European physicians: "and hence the error which they sometimes fall into." (p. 212.)

With regard to the general treatment of disease, it is stated, that the indications are—"to promote the just balance of the elements and humours, by a judicious choice of aliment, and by such means as assist the vital principle in the completion of the assimilation," attention being first paid to diet: the object in view is to be effected by emetics, purgatives, or bloodletting.

"But these humours are not to be dislodged indiscriminately, but at certain seasons and diurnal periods of the disease. Health was thus supposed to be promoted by the exhibition of an emetic once a fortnight, a drastic purgative once a month, and bloodletting twice a year, at the change of the seasons. The vital principle was supposed to give warning when the corrupted humours were ripe for being evacuated, and the physician was directed to observe carefully, so as to be able to assist and not disturb the spontaneous efforts of Nature. The seasons at which she exhibited these beneficial influences were supposed to be determined by the known cause of the disease, which led to the belief of the definite course and the mystical powers of numbers, by which Nature may be invariably observed to arrive at certain determinate results, which were supposed to be regulated by an arithmetical progression. This led to the belief of the maturation of the diseased humours, and of the existence of a period in which the perfect state of mixture takes place. These were called *critical* days....... These days were recognized by the Egyptian priests, as related by Pythagoras and others. Among the Hindoos the humoral pathology appears to have originated without any assistance from other nations, and became as generally believed, and arrived to the same consequences as it was in Europe." (Wise, p. 212.)

"Another equally plausible opinion was, that all diseases divide themselves into two great classes, of sthenic or (and) asthenic disease. The one being an increase and the other a diminution of excitement, between the extremities of which health was supposed to be placed. This appears to have been an early opinion among the Hindoos, is now generally believed over all the Asiatic nations, and has led them to the division of remedies into stimulating and cooling, which were employed according to the nature of the disease." (p. 213.)

After an account of the diseases of the humours, Dr. Wise proceeds to give the opinions of the Hindoos on fever, and it is said:

"Fevers are first considered, because it is said that man is born and dies in fever; because it affects the whole body, the organs of the senses, and the mind; and is so severe that only men and gods can survive, and by which various other diseases are produced." (p. 219.)

The causes, symptoms, and the varieties of fever are then described, with detailed instructions of the treatment, and with much of what must have been necessarily the result of careful observation, we have incorrect theory and undue attention to numbers. With regard to critical days it is said:

"The 7th, 10th, 12th days, are always the days on which the fever is severe, or from these periods the symptoms diminish in severity.

"In other books it is stated that the critical days are the 7th, 9th, 11th, 14th, 18th, 22d, from which period it is diminished or increased. Those who live to the 22d day generally recover."

Smallpox, of which, however, measles and waterpox are made varieties, is adduced by Dr. Wise as a disease existing for a long period in one country without penetrating into another. It seems to have been long known to the Chinese and Hindoos: he concludes that it was probably conveyed westward by the Persian conquerors of Hindoostan. Rhases is supposed to have been the first to describe it; but, though Dr. Wise does not adduce his authorities, it is probable that accounts of the smallpox are contained in the works to which the Arabs had access.

As we might expect, the subject of leprosy is rather fully treated of, but under this head some other cutaneous diseases are described. "Though it commences first in the skin, it gradually extends deeper and deeper, like the small shoots of the banian trees (Ficus indica), which at first confined to the surface, advance deeper and deeper, until they extend over the whole body."

Among the diseases affecting the mind, we have a chapter on Devil madness. Among those of the head; it is said of hemiplegia, that it "is very obstinate, and most distressing, so that a thousand physicians cannot cure it." The diseases of the ear are said to be twenty-eight in number, and as cutting off ears was a frequent method of punishment, Susruta recommends the surgeon to prepare a new ear, by "removing the skin from the neighbouring part, leaving a connexion to keep up the vitality." Diseases of the eye seem to have attracted much attention, and the operation for cataract is described, but it is not stated from what work this is taken. (Wise, p. 303.)

In the treatment of asthma, we observe no notice of datura-smoking, which is now so generally practised in different parts of India.

Consumption, defined as a rupture or ulcer of the respiratory organs, is entitled the prince of diseases, from its frequent and fatal nature; but we suspect that other diseases, as land scurvy, &c., are sometimes confounded with consumption, which is rather a complaint of the northern, than of the southern parts of India. Urinary diseases seem also to have obtained considerable attention; and we may observe that diabetes mellitus appears to have been known to the Hindoos long before it had been noticed by others. Of urinary calculi it is said, the disease is dangerous like poison or thunder. "When recent, it may be cured by medicine, but when of long standing, an operation is required." The operation is then described,

but the authors in which the earliest accounts occur are not mentioned. Among the diseases of the organs of generation, syphilis is treated of by Dr. Wise; but he states that it is not mentioned in old Sanscrit works, but was introduced into Hindoostan by way of Persia, and also by the Portuguese into the southern parts of India, and in comparatively recent times.

Towards the conclusion of the work there is a chapter on "Poisons and their Antidotes." The poisons are usually arranged into two classes: the first, consisting of vegetable and mineral poisons, are named *Stábarch,* and the second, animal poisons, *Jargamah.* The first class is, however, afterwards divided into mineral and into vegetable poisons; but we have considerable doubts respecting the Sanscrit names being correctly translated. Among the animal poisons, serpents, poisonous rats, the bites of mad dogs, and the poisonous bites of insects are enumerated. We are sorry to observe here the same inattention to correctness in translating the names of antidotes as we observed in the case of medicines: thus *Kusta,* which is elsewhere translated *Costus* (p. 403), is called Strychnos Nux Vomica, and mentioned as an antidote to be given internally, and also applied externally, after the parts poisoned have been scarified and burned.

Having taken a general view of the contents of the celebrated work of Susruta, as far as the Latin translation has as yet been published; and of Dr. Wise's Commentary more briefly than we should have done if the authorities had more generally been given; because many of the opinions and statements derive their chief value, from being found in medical works which can be proved to have been in existence prior to the time of the Arabs. Any later opinions may have been derived, though this is not very probable, through the medium of the Persian translations of Arabic works, which are used everywhere in India by the Mahommedan practitioners of medicine. But the parts of the Ayur Veda which still remain, as well as the works of Charaka and of Susruta, prove that the Hindoos had a system of medicine, containing much of original observation and of opinion, before the Arabs had paid any attention to literature or science. Among the opinions and reasons, however crude, of these early medical writers, we cannot but observe a remarkable coincidence in many points, to opinions which were entertained in early times both in Egypt and in Greece. It still remains an interesting subject of inquiry to ascertain the causes of this general resemblance; whether all were indebted to some common source, as to the early civilized and powerful empires of Assyria, Babylon, or Persia; or whether medicine was cultivated first by the Egyptians, or by the Hindoos, and from the one spread to the other: the Greeks deriving their information only from those who were placed nearest them. At all events, we cannot but admit, after the evidence which has been adduced, that, however degenerated may be the natives of India at the present day, there can be no doubt, that at an early period they paid attention to, and had made advances in different departments of medicine. We must also express a wish that these early works should be carefully translated, and the synonymous scientific terms correctly applied, so that we might be able to weigh more accurately the evidences of originality, or trace the mass of curious information to some early source. We hope that the Sydenham Society may be induced to undertake the translation of these early works,

accompanied with medical and scientific commentaries, as has been so ably done by Dr. Adams with the translation of Paulus Ægineta.

In conclusion, we could not but have wished that the medical talent displayed by this early civilized nation should have opportunities of cultivation in the present day, but we call to mind, that we have in a former volume, reviewed a work embracing the Reports from the dispensaries in Bengal, where the Practice of the Native assistant surgeons, educated in the Medical College established by the East India Company in Calcutta, is given in detail and in their own words. Colonel Sykes read a paper before the British Association at Southampton, and now published in the Journal of the Statistical Society, March, 1846, on the further progress of these Europeanly educated native practitioners of medicine, where further proofs are given of continued improvement and of extended usefulness. As similar institutions have been established in Madras and in Bombay, there cannot fail to be raised a class of well-educated practitioners, who will carry the benefits of modern science and of greatly improved medical practice, into every part of the widely-spread dominions of the British East India Company.

ART. XVIII.

Zum Andenken an Dr. Johann Stieglitz, Königl. Hannoverschen Obermedicinalrath und Leibartz. Von Dr. K. F. H. MARX, Hofrath und Professor in Göttingen.—*Göttingen*, 1846.

A Tribute to the Memory of John Stieglitz, M.D., &c. By Professor MARX, M.D., of Göttingen.—*Göttingen*, 1846. pp. 172.

THE Memoirs of Stieglitz by Professor Marx is a charming biographical *brochure*, and we are quite sure would have been freely used by Dr. Mackness in his work reviewed in another part of this Number, had he had the good fortune to see it entire before publishing his translation, for we believe the greater portion of its contents was unknown to him. We shall therefore notice it here, as a sort of supplement to our former article, and do not doubt that should Dr. Mackness's work attain to a second edition, he will find room for longer and more numerous extracts from it.

The Memoirs contain, first, the information supplied to Dr. Mackness; secondly, a series of extracts from the letters addressed by Stieglitz to his friend the Göttingen Professor; and then an estimate of his character. An Appendix contains some of his earlier reviews (referred to in the letters), two curious letters by Heim, and a fac-simile of Stieglitz's handwriting. Respecting the latter, we can truly say that it much more closely resembles the Arabic characters than those in use in Europe, and the Englishman must be well practised in German writing to decipher one-half of it.

Stieglitz was in extensive practice at Hanover. He was court-physician, chief of the medical police of the kingdom, obermedicinal-rath, &c. After Professor Marx had been three years at Göttingen, he paid a visit to Hanover, and called upon Stieglitz before eight o'clock in the morning: this first interview is thus described:—

"I found a man of an uncommon aspect; serious and friendly; his ample fore-

head showed the thinker. He sat enveloped in a cloud of tobacco-smoke, at a table covered with books and official documents, and drawing vigorously from time to time at his pipe. Our formal interview speedily changed into an animated conversation; but he was so discursive, and referred to such varied matters, that he touched upon men and things rather as if he were investigating than communicating. As we were interrupted, he pressed me cordially by the hand, and invited me to his table, that we might again resume the broken thread of our conversation. . . . . The second visit, after the lapse of a few hours, was as if it were made to an old friend. Confidence was established, and an intellectual friendship cemented." (p. 17.)

From this time to his decease, a correspondence was kept up between the two friends. Extracts from the letters of Stieglitz are connected by a running commentary of the editor. We have marked a few passages as illustrating the everyday life and thoughts of the man. The following shows a crowded and defective state of the profession in Germany, or the writer is a *laudator temporis acti.*

"Towards the end of the last month I returned to Carlsbad; a journey I undertook for the sake of my wife, and not for my own pleasure or profit. . . . . I have become personally acquainted with from thirty to forty physicians, and have had consultation with many. The estimation of our calling, the predominant treatment of chronic diseases, and the power exercised over them by our art have excited in me many depressing thoughts. Everywhere I find physicians to be shrewd and adroit, well versed in the concerns of life and their modifying circumstances, but often filled with perverse and prejudiced ideas, taking no interest in science, unacquainted with the progress of medical science and practice, and not feeling the necessity of keeping up and renewing their earlier studies. Either old or new writers are read only by a very few. Very few have won my esteem." (p. 20.)

The medical journalism of Germany is also estimated lightly by Stieglitz. In one place he observes it is wonderful how rapidly the journals multiply and how wretched they are. In another he expresses a very indifferent opinion of the criticisms of the journalists. He was well acquainted with English writers, and was an extraordinarily rapid reader, and thought his time was as well spent on one of Sir Walter Scott's novels as on a book of mere facts.

Touches of worldly wisdom appear here and there; as the following on giving advice.

"Whoever has lived long in the world and been observant, has discovered that the giving of advice is dubious and insignificant. Every result is doubtful, and innumerable contingencies may happen. Who can know the external and internal condition of a person? In the long run every one takes that course to which his inclination leads him, or to which he is impelled by a good or evil genius." (p. 28.)

Or this, on faith in the future:

"Every one who has the power, and understands how to use and apply properly the present, can and must confide in the future. Sooner or later the fate of such a man takes a turn for the better, often in a quarter from which he least expects it, and important circumstances happen which elevate him." (p. 31.)

The treatment of incurable diseases:

"I often think some information is necessary as to the conduct of physicians with reference to incurable diseases—not as to what should be done, but what should be left undone.

"It often shocks me to witness extensive methods of cure propounded in steatoma, cancer, and innumerable other cases, in which it is well known no cure can be effected. A physician newly called in begins over again from the beginning what his predecessor has already practised. The constitution is shaken more and more, and the heroic remedies used can have none other than evil results." (pp. 35-6.)

Danger of giving an unfavorable prognosis in incurable cases:

"The patient will often have a distinct answer from the physician, whether he has to apprehend death, and whether it be now unavoidable. Reasons are given to prove the importance of this information, and with what fortitude and resignation it will be received. All ground of hope must not be taken away. In Berlin an officer of artillery shot himself on the steps of Selle's house, after the latter, on being earnestly pressed, told the truth respecting his phthisis, and not waiting even long enough to get to his own residence. I witnessed a scene in the first year of the present century of quite another kind, which impressed upon me the propriety of never expressing the full truth in cases of this kind. A man will not suffer that to be plainly spoken by another, of which he is fully conscious himself, if it be evil and goes against himself. He will at times think of the possibility of the contrary." (p. 38.)

What is wisdom:

"It is wisdom not to intermingle with public business or intrude into it. I have extended this doctrine for a long time even to circumstances within my own province, and limit myself to giving an opinion when desired officially, and doing the work I am required, without taking further notice thereof, to see whether my advice has been followed or not, and bringing no personal influence to bear on the matter. When a man has become old and had much experience, he arrives at last at this passive behaviour and indifferentism, in contradiction to his character and early struggles. The mode in which public business is now conducted and the trade-like manner of most statesmen, must lead every one to this line of conduct who values his own peace or has it in view, or who is not prepared to sacrifice his self-respect." (pp. 43-4.)

We might go on multiplying quotations of this kind; we forbear, however, with a recommendation of this little work of Professor Marx's to all who take a pleasure in the perusal of German literature. The price is a trifle. We cannot, however, close this brief notice without the closing paragraph of the Memoirs. Writing of his conduct towards his patients and professional brethren, Marx observes:

"His name alone gave confidence to the suffering; his presence, comfort and help. Faith and trust preceded and followed him. Kind and serious, sympathizing and inquiring, he approached the patient; with comprehensive searching and kindly questionings he firmly assured him. That which he prescribed was the result of the most careful thought. Where it was possible he used the simpler and milder remedies; if not, those by which he could more decidedly attain the distinct and desired end.

"So he completed a busy and useful life; to the state a faithful servant, to society an ever ready helper; to his own people and his friends a sure rock of defence.

"His memory will never fade from their hearts. And in the spot where he so successfully laboured it will be a sacred duty to maintain it ever alive; for he was very much to many, although all did not know his full worth. But he who has once seen the glowing sunshine on the Alps,—the summits glittering—while on all around is the calm and gloom of night, will never have the same effaced from his memory." (p. 122.)

Art. XIX.

1. *A Treatise on the Inhalation of the Vapour of Ether, for the Prevention of Pain in Surgical Operations, &c.* By J. Robinson, Surgeon-Dentist, &c.—*London*, 1847. 8vo, pp. 63.
2. *Notes on the Inhalation of Sulphuric Ether in the Practice of Midwifery.* By J. Y. Simpson, M.D. F.R.S.E., Professor of Midwifery in the University of Edinburgh.—*Edinburgh*, 1847. 8vo, pp. 11.

One of the most remarkable events in the history of medicine, regarded as a practical art, is certainly that which has excited so much attention in Europe and America during the last four months,—the employment of the vapour of ether as a means of abolishing pain in the practice of Surgery, Midwifery, and Medicine. And the results hitherto obtained seem to justify us in regarding the event as no less beneficial than remarkable. It is assuredly true, that by means of the new process, not a little of that dreadful suffering, heretofore inseparable from the performance of most surgical operations, has been abolished in the practice of the most eminent surgeons in Europe and America, during the three or four months just elapsed; and there seems every reason for believing that the benefits already experienced are likely to be perpetuated, perhaps greatly enhanced in amount, through all future time. It has been the ardent desire of philanthropists in all ages to save humanity from pain, in all its forms; and the means of dissociating the practice of Surgery from one of its most terrible forms, has often formed the subject of the cogitations and the stuff of the day-dreams of the benevolent surgeon:

> ———" what best may ease
> The present misery. . . . If there be cure or charm
> To respite, or deceive or slack the pain."

The utter futility of all the attempts to attain so desirable an end by ordinary sedatives, &c. is sufficiently shown by the fact, that for many years past no means whatever have been employed by surgeons with this view, —Nature being left to sustain, as best she might, by her own unaided powers, this worst and most intolerable of human ills,—this "perfect misery," as Milton truly calls pain.

That a means should be discovered, and discovered in our own day, calculated not merely to furnish positive relief in many of these terrible inflictions, but almost to exceed, in its practical working, the wildest dreams of the philanthropic enthusiast, is a matter so much within the domain of the marvellous, almost of the supernatural, that it is no wonder we should not be yet prepared to consider it, in all its bearings, with the cool blood of philosophy. Even in this era of steam and electricity, of macroscopes and microscopes, the discovery is one that arrests more universal attention, and excites a deeper interest, than any mere physical fact whatever,—not even excepting the magnificent achievement of Adams and Leverrier, which " yields the lyre of Heaven another string."

It is but an act of simple justice to the mesmerists to admit, that they alone, among chirurgeons of the present day, have toiled for the attainment of the abolition of pain in surgical operations. This rational and

benevolent aim of theirs ought to plead trumpet-tongued in their favour, while we are condemning, as we cannot fail to condemn, their manifold and monstrous aberrations from the path of common sense and philosophy. It must even be admitted, and we ourselves have admitted it—(see our 44th Number)—that the mesmerists have, to a certain extent, been successful in their aim, inasmuch as they have, in a small proportion of cases, and after a great deal of painful manipulation, succeeded in rendering patients, to all appearance, insensible to the pain of surgical operations. The evidence formerly adduced in support of this opinion has been recently much strengthened by the official report on Dr. Esdaile's experiments in the Calcutta hospital.* These attempts, however, praiseworthy as they are, will be entirely superseded, in future, by the new process of ETHERIZATION, which seems to possess infinitely greater advantages than the mesmeric process, without any of the great disadvantages of this.

We have already said that the time is not yet arrived for the calm and full consideration of this great discovery,—the true character, and value, and bearings of which can only be fixed after much more, and more varied experience than at present exists. On the present occasion, we purpose merely, by a few hurried observations, to do something towards meeting the desire for further information which our readers no doubt possess, and for the gratification of which, in some degree, at least, they may naturally look to the pages of this Journal. On some future occasion, we hope to lay before them something more worthy of them and of ourselves.

There seems to be little or no question as to the fact, that we are entirely indebted to Dr. Charles T. Jackson, of Boston in America, for the virtual, if not the actual, discovery of the use of Ether, as a means of destroying pain in surgical operations. To him, and to his coadjutor, Dr. Morton, we certainly owe the boon, whatever be its amount, of the practical application of the process now so universally employed. It is, nevertheless, true, that many others before them had, long since, not only imagined, but even made trial of the same or similar means, with somewhat similar intentions. We will here refer to a few of these foreshadowings of the great discovery, which are immediately accessible. Some of these, it will be seen, may fairly be considered as *anticipations;* but as they bore no fruit, they can only be regarded as the unvalued apples dropping idly and uselessly from the tree of knowledge: the Newtonian glance had not yet fallen on them.

In Fontana's papers, in the Philosophical Transactions, we find many experiments on men and animals, on the inspiration of different kinds of air. He himself (Phil. Trans., 1779, vol. 69, pp. 346-7,) made several experiments on himself with hydrogen gas (inflammable air). Taking it diluted, he found the process rather pleasant; but when he inspired the undiluted gas, the results were far otherwise: he became pale, confused, and fell on the floor insensible. Davy experienced precisely similar effects, but in a more intense degree, from breathing hydrocarbonate. "After the second inspiration (he says) I lost all power of perceiving external things, and had no distinct sensation except a terrible oppression on the chest. During the third inspiration, this feeling disappeared. I

* Bengal Hurkaru, Nov. 19, 1846.

seemed sinking into annihilation." (Researches, 1800, p. 468.) He was a considerable time in recovering from the effects of this dose.

Dr. Richard Pearson, of Birmingham, however, seems to have been the first, as far as we are aware, to have employed the inhalation of ether medicinally. (See his communication in the first vol. of Duncan's Annals of Med., 1796; also in vol. vii, of Simmons's Med. Facts and Observations.) He employed the remedy in phthisis and other pulmonary diseases, either simply or combined with hemlock. His mode of administering it was by inhaling it as it evaporated in an open vessel, simply by the mouth, or through an inverted funnel; or by holding a handkerchief wetted with it near the mouth and nose.

Dr. Beddoes also, in his work on Factitious Airs, published at Bristol in 1795-6, in five parts, among many other communications on the subject of inhalation of various kinds of airs in diseases, gives several communications from Dr. Pearson, on the inhalation of ether. In Part III of the same work, p. 40, there is a letter from one of Dr. Thornton's patients, in which the patient himself gives an account of the inhalation of ether, by Dr. Thornton's advice, and its effects in a case of pectoral catarrh. He says it gave almost immediate relief both to the oppression and pain in the chest. On a second trial, he says he inhaled two teaspoonfuls of ether, which, he adds, "gave immediate relief as before, and *I very soon after fell asleep*, and had a good night's rest." In Part IV, another curious case is given by Dr. Thornton, in which inhalation was prescribed for the relief of a very painful inflammatory affection of the mamma, and with very beneficial effect. In this case, however, the ether was used rather as a means of depriving the air of its oxygen, than as a direct agent; the effect of the inhalation on the patient being, as in the experiments of Fontana and Davy, to produce partial asphyxia. The results, however, are curious, and not without interesting relation to the present subject of discussion. Dr. Thornton says:

"I therefore filled a bell-glass with atmospheric air, and burning two tablespoonfuls of ether in it, I rendered it chiefly azote and inflammable air. She persisted in inhaling this for about five minutes, standing up, until the pulse was obliterated; the eyes became dim and no longer represented the objects of vision; the face was deadly pale, and swooning coming on she fell into the arms of a servant. . . . . In about ten minutes she revived. The pulse was feeble and only 98; and for the first time, she said, for some weeks, she felt her breasts cold and easy." (p. 154.)

At this time, and subsequently, Dr. Thornton was in the common habit of administering the vapour of ether to his patients, among other pneumatic means used by him; and we are informed by one of the most eminent philosophers of our time, that he himself had this remedy administered to him in his youth by Dr. Thornton. In Mr. Robinson's pamphlet, p. 15, Dr. Boot refers to a case in which it was used in the beginning of the present century, by the late Dr. Woolcombe of Plymouth. No doubt, its use was not very uncommon at the end of the last century, though, after a time, like most other fashionable medicines, it fell into disuse and was forgotten.

In all these trials, no one had directly in view the removal or abolition of pain, though this was attained indirectly in Dr. Thornton's case. But Sir Humphrey Davy, who, it is well known, first began his chemical

career by assisting Dr. Beddoes in his pneumo-medicinal researches at Bristol, seems not only to have contemplated such a result by means of medicamentous inhalation, but to have actually put it to the test of experiment on himself. The medium of his experiment, however, was not ether, but the nitrous oxyde. Sir Humphrey tells us that on two occasions the inhalation of the nitrous oxyde removed headache. He also tried its effect "in removing intense physical pain" while he was cutting a wisdom-tooth. He says, "the pain always diminished after the first four or five inspirations; the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure." (Researches, p. 465.) In a subsequent part of the volume Sir Humphrey adds this remark, rendered very striking by recent events:

"*As nitrous oxyde, in its extensive operation, appears capable of destroying physical pain, it may probably be used with advantage during surgical operations* in which no great effusion of blood takes place." (Researches on Nitrous Oxide, p. 556.*)

In the article *Ether* in the 'Dict. des Sc. Med.,' vol. xiii, published in 1815, we find the author, Nysten, speaking of the inhalation of ether as familiarly known, and as employed for the relief of some pulmonary diseases, and also *for mitigating the pain of colic*, &c. He even describes (p. 385), an apparatus for the purpose of inhaling it, which closely resembles some of those now employed. It is also well known, that many years ago it was not uncommon in some of the pharmaceutical lecture-rooms and chemists' shops in London, for the pupils to inhale ether-vapour as a sort of substitute for the nitrous oxyde, or laughing gas.

To persons acquainted with these various experiments and observations, and more especially with those of Pearson, Thornton, Davy, and Nysten, the advance to the perfect discovery of ethereal inhalation, as a means of destroying pain in surgical operations, would seem but a very small step; yet this step was not made till the present day; and there seems every reason for believing that it was first made, as we have already stated, by Dr. Charles Jackson. We extract the following account of Dr. Jackson's earliest proceedings in this matter, from the 'Boston Daily Advertiser' of March 1st, 1847. After some preliminary remarks, among which we find the important acknowledgment that he was "early impressed with the remarks of Davy, concerning the remedial agency of gaseous matters," he proceeds as follows:

"In my first successful experiment the conditions as above stated [viz. that the ether should be perfectly pure and the vapour mixed with a due proportion of air] were fulfilled, though the mode of administration was of the simplest kind, it is true, but yet efficient. A folded cloth saturated with the highly rectified ether was placed over the mouth, the air being drawn freely through it, and the inhalation was continued until I lost all power over myself and sank back in my chair in a state of peculiar sleep or reverie. I experienced at first a sense of coolness, then of exhilaration and warmth followed by loss of consciousness. But it was not until a subsequent trial that I became aware that this loss of consciousness was accompanied by insensibility to pain; and a severe bronchial irritation produced by the inspiration of a large quantity of chlorine gas was for the moment relieved, and the peculiar distress occasioned by that gas was not felt, so long as

* This first and very striking work of Davy contains an immense number of experiments on himself in regard to the inhalation of different kinds of air. It well merits the attention of the profession at the present time.

I was under the influence of ether, though as that passed off it returned. I had several times occasion to mention these facts to my friends, and it is now a year since I urgently advised Mr. J. Peabody, who was associated with me as a pupil in chemistry, to inhale the ether vapour as a means of preventing pain, which would arise from the extraction of two of his teeth. He consented to try the experiment, and was preparing some ether for the purpose, but on consulting the works in which the effects of ether are mentioned, he found all the authorities arrayed in opposition to my views, and that they warned against its inhalation, as I have before stated, and he therefore did not complete the experiment.

"About the last of September or early in October last, I communicated my discovery to Dr. W. T. G. Morton, an enterprising and skilful dentist of this city, whom I occasionally advised, and who called at my laboratory to borrow an India rubber bag, which he said he intended to fill with atmospheric air, and to cause a refractory patient to breathe it, hoping to act on her imagination, and induce her to allow him to extract a tooth. I dissuaded him from this attempt, and explained to him that I had discovered a process by which real insensibility to pain might be produced. I showed him sulphuric ether, and described the method of administering it, and also its effects on the system, assuring him, that if my directions were carefully followed no danger would ensue. I advised him to try its effects on himself, in order that he might better understand its mode of operation. He followed my instructions and was successful in the first trials, in the extraction of teeth unattended with pain, the results proving exactly as I had predicted. I also furnished him with a large glass flask with a bent glass tube as an extempore inhaling apparatus. I then proposed to him the trial of the ether in a surgical operation at the Massachusetts general hospital, where it was administered by Dr. Morton, and it proved successful; but some persons who witnessed the first operation doubted the entire freedom from pain, since the patient said 'he felt a scraping.' I was therefore desirous of testing it in a capital operation, the severity of the shock being the best test with regard to the degree of insensibility. Dr. J. C. Warren politely consented to have the trial made, and its results proved entirely satisfactory, an amputation having been performed under the influence of etherial vapour without giving any pain to the patient."

Since this time our readers need not be told how widely the practice has spread, and in what a countless number of cases it has been applied in every country, and applied, we may add, with an uniformity of success and safety, which, considering the nature of the process, is most extraordinary.

The modes hitherto adopted for applying the ether vapour are very various, and the forms of apparatus innumerable. The essential points to be regarded seem to be—1, that the ether be very pure; 2, that the tube conveying the vapour be sufficiently wide to admit a current large enough to fill the respiratory organs without effort; 3, that the vapour be mixed with a sufficient proportion of air to render it easily respirable, yet not so much diluted as to render it long in producing insensibility; 4, that the apparatus possess a means of regulating this proportion accurately, the vapour being always given comparatively weak at the commencement, so that the glottis and lungs be not over-irritated; 5, that the full strength of the diluted vapour be applied as speedily as it can be tolerated by the air-passages, the nostrils being then closed, so as to exclude all extraneous air; —a strong dose, rapidly given, seems to be at once the safest and most successful proceeding; 6, that inspiration be continued, within certain limits, until complete insensibility is attained, as evinced by obvious signs; and, when stopped, to be renewed, for short periods, as often as signs of awaking, manifest themselves during the operation.

The period of time required to produce the full effect of perfect sleep

and insensibility, varies considerably in different individuals; but much more, we believe, from difference in the mode of administering the agent, than from difference of individual susceptibility. When the apparatus is good, the ether pure, and the process directed by an experienced manipulator, the average period of inhalation to produce insensibility, may be stated at from two to four minutes: in a few rare cases double or triple this amount of time is required. As might be expected, children are sooner affected than adults; and, generally speaking, the influence is more perfect and more benign with them. It has frequently appeared as if the period of inhalation was much longer than what is just stated; and also that a considerable number of persons are altogether unsusceptible of the soporific influence. We are led, however, from a good deal of observation and investigation, to believe that nearly all these supposed exceptions are owing to imperfect administration of the agent. And we are inclined to think, that there are but very few persons indeed unsusceptible of this peculiar effect of ether. A general impression exists among operators, that persons much addicted to strong drinks, especially spirit-drinkers, are altogether refractory to the agent, or require a much larger dose to become affected. This may probably be true; but we have only met with two or three cases, during our investigations, which seemed to authorize this belief. In one case in which the ether was administered by a skilful and experienced manipulator, nearly half an hour elapsed before the individual came fully under the influence of the ether; this gentleman was accustomed to drink a bottle of wine or more per diem. In another case, that of a man who confessed that he took daily half a dozen glasses of gin or more, the soporific effect was not at all experienced after an inhalation of twenty minutes; although, in this case, the absorption of the ether into the blood was evinced by the *exhalation* of the etherial odours for a day or two after the trial. It is to be remarked, however, that it is not solely *the amount* of ether inhaled that regulates the supervention of the soporose state, but the amount absorbed within a given time. It is quite possible to inspire three, four, nay ten times the quantity of ether capable of producing sleep, without this state being induced, provided the vapour be taken in an extremely diluted form; and we believe that this over-dilution of the vapour and its consequent protracted inhalation, is a frequent cause of the *excitement* which supervenes so often in the practice of some persons, while it shows itself so very rarely in that of others. In these cases the patient may be made *drunk*—drunk in the first degree, but not *dead-drunk*, the condition required for chirurgical purposes.

The occasional occurrence of this *excitement* instead of the desiderated stupor, is regarded by some surgeons as a fatal objection to the practice. But we think this opinion is erroneous. In the first place, we believe that the excitement occurs extremely rarely when the process is properly regulated—probably not more than once in a hundred instances; and, secondly, there is no necessity for the surgeon operating at all in the cases in which it does occur in spite of all precautions: he may still have recourse to the old practice of operating on his patient awake. Our own observation, however, and the opinion of those who have had by far the greatest experience in the practice of etherization, lead us to believe that the proportion of persons in whom a state of excitement will frustrate operations, is extremely small.

The average duration of the state of sleep or insensibility, may be stated to be about the same as the period required to induce it—or a little less, say from two to four minutes: the period, however, occasionally greatly exceeds this, extending sometimes to half an hour, or even an hour. The awaking is generally sudden and complete; and, in the great majority of cases, the only effects it leaves behind are—a slight feeling of *muzziness* in the head, sometimes amounting to headache, and the odour and taste of ether in the mouth and nasal passages.

The immediate and obvious effects of etherization on the individual hardly require notice, as they must be familiar to all our readers, if not from personal trial at least from observation on others. All the usual phenomena of the deepest sleep supervene almost suddenly, gliding often into the profoundness of sopor, and verging occasionally upon, if not actually lapsing into, coma. The voluntary muscles become suddenly relaxed, the jaw falls, the arms hang down, the eyes roll upwards under the upper lid, the respiration becomes slow and laboured, and the face often becomes either pale or morbidly flushed. The aspect of things is, indeed, such as can hardly be contemplated, for the first time, without alarm: the individual seems, to the common eye, to be sinking into the sleep of death.

The actual effects of etherization on the functions, fluids, and organs of the body have not as yet been thoroughly investigated. Medical men have been hitherto so absorbed in the contemplation of the practical results, that they have had but little leisure or inclination to inquire into the philosophy of the thing. It may be stated, however, that the pulse is at first accelerated, and afterwards falls, but rarely to the natural standard; the respiration seems commonly to follow the same rule. The iris seems to be generally expanded, sometimes contracted.

In the state of perfect etherization we believe all sensation is abolished; in a less perfect state, an obscure perception of external objects remains, while the sense of pain is extinct. The psychical state is various. Generally speaking the sense of external impressions becomes at first confused, then dull, then false, with optical spectra or auditory illusions, general mental confusion, and then a state of dreaming or utter oblivion. In the majority of cases, the mind is busy in dreaming, the dreams being generally of an active kind, often agreeable, sometimes the reverse, occasionally most singular; and, frequently, a great deal is transacted in the few short moments of this singular trance. Many of the patients who have undergone the most dreadful operations, such as amputation of one or both thighs or arms, extraction of the stone, excision of bones, extirpation of the mamma, have readily detailed to us, and most with wondering thankfulness, the dreams with which, and with which alone, they were occupied during the operations. The character of the dreams seemed to be influenced, as in ordinary cases, by various causes, immediate or remote, present or past, relating to events or flowing from temperament:

> "Et quoi quisque fere studio devinctus adhæret,
> Aut quibus in rebus multum sumus ante moratei,
> Atque in ea ratione fuit contenta magis mens;
> In somnis eadem plerumque videmur obire."

A good many seemed to fancy themselves on the railway amid its whirl

and noise and smoke; some young men were hunting, others riding on coaches; the boys were happy at their sports, in the open fields, or the filthy lane; the worn Londoner was in his old haunts carousing with his fellows; and our merry friend, Paddy, of the London Hospital, was again at his fair, wielding his shelala in defence of his friends. Others, of milder mood, and especially some of the women patients from the country, felt themselves suddenly transported from the great city and the crowded hospital-ward to their old quiet home in the distant village, happy once more with their mothers and brothers and sisters. As with the dying gladiator of the poet, the thoughts of these poor people—

"Were with the heart, and that was far away."

Some seemed transported to a less definite but still happy region, which they vaguely indicated by saying they were in heaven; while others had still odder and warmer visions, which need not be particularized.*

For the purpose of obtaining information on all the points of this most interesting subject, we personally questioned all the patients in the London hospitals, who, at the period of our visits, still remained in the wards after the ether-operations. They were in all fifty-four, and the great majority had been the subjects of capital operations. They were unanimous in their expressions of delight and gratitude at having been relieved from their diseases without suffering. In listening to their reports, it was not always easy to remain unmoved under the influence of the conceptions thereby communicated, of the astonishing contrast between the actual physical condition of the mangled body in its apparent tortures on the operating table of a crowded theatre, and the really happy mental state of the patient at the time. The old story of the magician in the Arabian Tales seemed more than realized before us, the ether being like the tub of water, one moment's dip of the head into which produced a life-long vision in the dreamer's mind. We ourselves, on trying the ether, as in duty bound to do, were not favoured by any visions good or bad; our mental condition, was, if we may so speak, that of annihilation or utter oblivion; a piece seemed snipped out of the thread of vital consciousness, as if our identity had been cut in twain.

The physical and physiological changes induced in the system by etherization are, as yet, very imperfectly observed; and the rationale of its action is far from being well understood. That the ether is immediately absorbed into the blood, and thus acts on the brain and nervous centres, either directly or indirectly, is obvious enough. The absorption is well shown by the long-continued *exhalation* of the ether by the breath of the individual who has taken it. This exhalation continues a longer or shorter period, according to circumstances, from a few hours to a few days. The presence of the ether in the body is also shown by the fact that an amputated limb has been found to exhale the etherial odour long after the operation, and the same odour has been detected on dissection, in the interior of the bodies of some who have died subsequently to an operation. It is also the prevalent opinion of surgeons that the

* We made an attempt to ascertain whether the amount of dreaming, or the character of the dreams, might be influenced by the quality of the ether. We had at first some ground for believing that the dreams were more frequent and of wilder activity, when the ether was impure; but the general results failed to bear out the first impression.

arterial blood is blackened by the ether; and it would appear that in two persons who died after etherization, the blood was found fluid after death. That this last state of blood was produced by the ether, however, is extremely doubtful, as it is well known that a like fluidity of the blood often exists in cases in which no ether has been administered.* The blackening of the blood by the inhalation of ether, has been proved by M. Amussat in experiments on animals, who further observed that the natural colour of the fluid was speedily restored on permitting the animals to breathe pure air. Admitting this, it will remain a question, whether the absorbed ether acts directly on the blood, or whether the blackness arises from the partial asphyxia induced by the partial exclusion of oxygen. The same doubt presents itself in regard to the production of the sleep and insensibility. Are these effects the consequence of the direct action of the ether on the nervous pulp, or effects resulting, secondarily, from the direct action of the blood altered by the ether? Or are they the mixed result of the direct etherial action and of the action of the hyper-carbonated blood on the brain? In a word, are the etherized patients simply *dead-drunk*, or are they *partly drunk* and *partly asphyxiated?*

We have already noticed the effect of etherization on the respiration and the pulse. When complete it suspends all voluntary motion as well as sensation, and produces a relaxed state of all the voluntary muscles. Its effect in influencing the reflex, and what may be called the external automatic actions, is not yet well ascertained; but it seems to have exerted no influence in relaxing sphincters, &c. Some surgeons seem positive that the muscles divided in the amputation of limbs do not retract as under ordinary circumstances; but this, as well as the blackening of the arterial blood, is doubted or denied by other operators. That undivided muscles are generally relaxed however, has been proved in a very beneficial way by the easier reduction of luxations, and by the readier retention of the bowel within the abdominal cavity after the operation for hernia.† Admitting this were occasionally an evil, it would have still some contravening good—as the relaxation of muscles must tend greatly to facilitate the reduction of dislocations—as well as the reposition of the bowel in hernia. An instance of this last effect is recorded in the 'Gaz. des Hôp.' of Feb. 23. A case of strangulated hernia was brought to the hospital for operation: on administering the ether, the gut was returned by the simple taxis, the resistance of the muscles having been removed.

The grand effect of etherization in abolishing the sensation of pain

* Since this page was printed, we have obtained the opinion of the most eminent pathological anatomist in London, in relation to this point; he writes as follows;—"I have no hesitation in saying that fluidity of the blood in the corpse occurs so often in connexion with various morbid states that it could never be safely ascribed to the influence of ether. Among 106 post-mortem examinations which I once tabulated in relation to the characters of the blood, it was completely fluid in four, and these were: two cases of cirrhosis of the liver; a case of tumour in the cerebellum with amenorrhœa; and a case of poisoning by opium. But I have besides clear recollections of fluidity of the blood in other cases of cirrhosis,—in one or two of tetanus,—in delirium tremens,—in many instances of fever, in the general fatty degeneration of old people. It has always been in my mind that this state of the blood coincides with so great a variety of morbid changes, that I should never be able to rely on it as characteristic of anything especially."

† This was strikingly shown in a case at the Middlesex Hospital, where the bowels, after operation (without ether), owing to extreme irritability of the parts, were repeatedly protruded to a great extent. On placing the patient under the influence of ether, the protruded bowel, on being returned, remained in the abdomen perfectly quiescent.

need not be further insisted upon: this may, indeed, be said to be the sole effect for which it is employed, as all the other effects are merely contingent and subordinate. It has, however, been made a question, whether the subjects of operations are really, in all cases, unconscious of pain, or whether they are not merely *forgetful* of what happened in their etherized state, just as a drunken man forgets on the morrow the blows and bruises he had received the day before, and which were painful enough at the time. This question can only fairly apply to those states of imperfect etherization already referred to, in which the patients exhibit more or fewer of the ordinary signs of pain, such as crying, flinching, &c. Although in such cases the individuals generally deny having suffered pain during the operation, it seems not improbable that they did suffer to a certain extent, and forgot it. What renders this probable is the fact, that they have sometimes entirely forgotten, afterwards, a rational conversation in which they had engaged immediately on awaking from their trance. In the more numerous instances of entire sopor, there are no grounds for admitting the existence of suffering at all, even if we had not positive evidence in the case of the dreamer, that a very different state of feeling existed at the time.

We now come to the most important part of our inquiry,—the practical part—the true relations of etherization to medicine and surgery—the position which it ought to hold in medical and surgical therapeutics,—and, as a corollary of this, the real value of the discovery.

So long as pain is an evil and ease a good—so long, in other words, as man is man, must any means be prized that is capable of achieving the latter by the abolition of the former. As, then, the pain of surgical operations is certainly among the most terrible of its class, and as it is no longer doubtful that etherization has the power of abolishing this, what remains for our consideration is not so much,—whether this new means shall be hailed by us as a matchless and priceless discovery, and cherished and adopted as a blessed thing: this appreciation has already been made; this adoption has been consecrated by universal practice: what remains for consideration is—Whether the good is a pure good, or is counterbalanced by attendant evils of such a magnitude as to authorize us to reject it, partially or entirely? We have already said that the time is not yet arrived for giving a positive and final judgment on the merits of the case. Etherization may be said to be still on its trial, and the verdict not yet returned. We are much mistaken, however, if, from the evidence already obtained, we may not, with considerable certainty, infer what the verdict will be. The obvious, open, palpable, glorious good of etherization, is, to deliver the wretched victims of surgical disease from the additional torture of pain, while seeking the goal of health through the portals of chirurgery. The evils that have been said to accompany or follow this good have, however, been regarded by some eminent surgeons of so serious a character, as to cause them not only to reject etherization in their operations, but to denounce it publicly as a means that will be scouted from the field of practice in less than a twelvemonth! We confess that we have been surprised to hear this opinion; as we have not been able to discover in any quarter (and we have sought it in all) any rational grounds to authorize or justify it. Of the hundreds and

thousands—we might almost say of the hundreds of thousands—who have taken ether to insensibility, either out of curiosity and for experiment, or for the mitigation or abolition of chirurgical pain, in America and in Europe, we have been unable to discover, after the most extended inquiries, *a single case* in which the process *certainly* produced death, or left behind it consequences of serious importance that were *certainly* attributable to it. In a small proportion of cases there have, no doubt, been some unpleasant results, such as temporary depression of the vital powers, headache, more or less considerable for some hours, and even for a day or two; hysterical excitement in women for a similar length of time; slight bronchial irritation; nausea and sickness; and some other slight affections; but the actual proportion of patients suffering even in this slight manner has been extremely small; indeed, *wonderfully* small, when we consider the indiscriminate manner in which the practice has been had recourse to, with bad ether, bad apparatus, bad manipulators, and, speaking generally, with the whole subject in the chaotic state of a new creation, the principles not understood, the practice merely tentative and experimental. That so very few and such trifling ill effects have occurred, in such a state of things, is, to us, a most convincing proof of the general safety of the practice. So far from results of this uniformly innocent complexion being those which might have been anticipated from the rash and almost universal employment of a means avowedly capable of producing others of a very different kind, it is really surprising that actual death, not once or twice, but scores of times, has not been the consequence of this etherial epidemic. We have ourselves been constantly looking for such consequences, and we are still prepared to find them; but when they arrive, if they ever do arrive, we shall still have to consider well, before condemning the ether, whether the fatal event was a necessary consequence of its use or merely an accidental result from its abuse.

It is, however, maintained by some that these fatal results have already arrived; and, at the very moment in which we write, a coroner's jury (not the best judges, by the way, of a physiological or pathological event) have decided that in one case, at least, death has been the consequence of etherization. We must, therefore, bestow some of our attention on this part of the subject.

It is well known that deaths have followed operations in which etherization was employed; and out of the number that have taken place in England, *four*, at least, have been more or less publicly attributed to the effects of ether. The four cases to which we refer are: a case of lithotomy in one of the London hospitals; a case of amputation in private practice by an eminent London surgeon; a case of lithotomy at Colchester; and a case of the removal of a tumour at Grantham. The subjects of the foregoing cases survived the respective operations about the following periods:—the first, twelve days; the second, three and a half days; the third, more than two days; the fourth, more than one day. Authentic accounts have been published of the last two of these cases, one, by the operating surgeon in the Medical Gazette of March 5th; and one in the Times of the 19th of March, as given before the coroner; while of the first two, we have taken pains to obtain all necessary details from the best authority. We will here give a brief outline of the principal events of each case.

1. In the Colchester case of lithotomy, we are told by the operator, Mr. Nunn, that the patient "recovered from its (the ether's) effects after a short time, and continued in a quiet passive state, but *without decided reaction,* for twenty-four hours. At this period he had a chill which lasted for nearly twenty minutes." Not long after this chill, there supervened a state of collapse from which the patient never rallied, though he lived after it upwards of twenty-four hours. Nothing particular was found in the body on dissection, unless we consider with Mr. Nunn, "the fluid state of the blood" and the "flaccid state of the heart" as such.

2. In the Grantham case, (removal of a large malignant tumour from the back of the thigh,) the ether does not seem to have produced the usual full effect of insensibility, as the patient not only moved at every incision, but "struggled and nipped witness's hand," and declared afterwards, that she "felt pain when they cut." The operation was a severe and long one, lasting altogether, according to the testimony of a witness, "an hour all but five minutes," and according to the operator "about twenty-five minutes, including the tying of the vessels," the wound being "about six or seven inches long." The principal witness stated that the patient "had a little brandy and water before the operation was quite over, which she swallowed readily, and a little more when she was put to bed" . . . that "when put to bed she appeared conscious;" that "shortly after, she took a little gruel and said she felt better, but spoke in a very low and faint tone of voice;" that "she seemed quite conscious during the whole time from the operation till her decease." She never rallied, however, after the operation, but lived about twenty-eight hours after it. In this case, also, the blood was found fluid on dissection, and there was some congestion of the brain: both of which states were "in witness's [a surgeon] opinion caused by the exhibition of the ether:" there was no other unusual appearance.

3. In the case of amputation in private practice, the patient, a gentleman upwards of seventy, was placed under the influence of the ether by a most experienced man, Mr. Robinson, and took about the average quantity. He does not, however, seem to have been completely affected by the ether, as he gave signs of pain, and afterwards said he had felt some pain, during the operation. The immediate effects, such as they were, went off very speedily; as the patient was able to take wine before being removed from the table, and he seemed doing pretty well for a time, though never rallying satisfactorily. He, however, lived nearly four days, presenting various anomalous nervous symptoms, among others slight recurrent delirium; the stump did not put on the healthy reparative process.

4. In the case of lithotomy in the boy, all the primary effects of the ether passed off as usual, the patient living many days and dying from the effect of local inflammation, &c., the consequence of the operation. In this case there is no mention made of fluidity of the blood, in the account of the dissection with which we have been favoured by the eminent surgeon of the hospital.

Now we put it to the candid consideration of all experienced surgeons, whether there is anything in any one of these cases, specially different, or in any respect different, from what they have repeatedly seen after

severe operations performed before the employment of ether? In three of the cases we have the ordinary phenomena of "shock" or "sinking," varied as it has been ever seen to vary in different cases. In the Grantham case, a most unusually long and severe operation (and in which by the way, as well as in the case of amputation, it may be fairly questioned if the system was ever fairly under the influence of ether at all) we have scarcely any attempt at rallying, and gradual sinking to death within thirty-six hours. In the Colchester case, though the patient never rallied well, we have no decided sinking, until twenty-four hours after, when a severe nervous rigor supervened, followed by prostration ending in death in twenty-four or thirty-six hours. In the case of amputation, we have nothing like immediate sinking, but that anomalous nervous state, so well described by Mr. Travers as "prostration with excitement," eventually ending in death after five days. In the lithotomy case in the boy, we have nothing but what is witnessed every year in every hospital; feeble reaction in a bad subject followed by unhealthy inflammation and death, many days after the operation. (In our future remarks we shall not refer to this case: it does not belong to the category at all, and is included merely because the death has been publicly attributed to the ether.)

Before we make any further remarks as to the particular character of these cases, and the cause of the patients' death, we must be permitted to lay before the reader, some extracts from one of our most estimable books in surgery, published more than twenty years ago. This work is Mr. Travers's Treatise 'On Constitutional Irritation,' a classical production with which we had erroneously fancied all our surgeons were, as they ought to be, familiar.

The principal object of Mr. Travers's book is to describe and illustrate a very peculiar condition of system to which he gives the general name of *constitutional irritation*, a condition which, when it exists, modifies in a very remarkable degree, the ordinary effects of morbid influences on the system. Mr. Travers, as might be expected, draws his principal illustrations from surgical practice.

"It appears, (says Mr. Travers) that various casualties, the operations which they require, and operations for chronic diseases, are occasionally productive of a series of symptoms indicating a fatal derangement, suspension, or failure of the powers by which life is maintained. . . . . The peculiar state which they exhibit, I particularize by the term prostration. This is of two kinds—the one pure and progressive, the other marked by alternations of excitement. The first, prostration without reaction, supervenes upon a degree of shock so intense as to destroy the irritability of the vital organs. The second, prostration with excitement, is the result of a less abrupt, or less intense shock, and indicates a greater degree of vital power, the excitement being a partial evidence of the unexhausted irritability of the vital organs." (pp. 106-7.)

"The following is a category of the symptoms indicating the two forms of prostration:—

"1. Prostration without reaction is marked by universal pallor and contraction of surface, shuddering, very small and rapid pulse, astoundment of the mental faculties, generally a dilated pupil, shortened respiration, dryness of the tongue and fauces; indistinctness, and at length cessation of the pulse at the wrist; stupor, oppressed and noisy respiration, coldness of the feet and hands, involuntary twitchings, relaxation of the sphincters, confirmed insensibility, stertor, and death.

"2. Prostration with excitement is marked by the signs of languor and stupor, or drowsiness, in the commencement, to which, after a variable interval, succeed nausea, rigor, præcordial anxiety, restlessness, jactitation; a rapid and bounding pulse, oppressed respiration with frequent attempts to sigh, flushed countenance, contracted pupil, dry heat of skin, parching thirst, rejection of liquids taken into the stomach, incoherence and wildness of expression, sometimes amounting to fierce delirium. This state is succeeded by exhaustion marked by somnolency, a profuse chilly and clammy sweat, a haggard and livid aspect, a small irregular or fluttering pulse, innumerably rapid: panting respiration, passive convulsions, hiccup and subsultus, the stupor and stertor of apoplexy, and death." (pp. 111-12.)

The following are only a few of the striking cases with which Mr. Travers illustrates his subject:

1. "A lady, Mrs. S—, who, concurring, as a point of duty, with the advice of her surgeons, reluctantly submitted to the removal of a small tumour in her breast, unexpectedly, and without any apparent cause, died on the morning following the operation. It was then, for the first time, ascertained, that she had prognosticated her death." (pp. 15-16.)

2. "I saw a man, who was the subject of strangulated hernia, expire suddenly on the table, during the steps preliminary to the operation, which, from the state of the symptoms, and of the bowel, as ascertained by examination after death, might be said to afford the fairest prospect of relief." (pp. 17-18.)

3. "A man of colour, of middle age, rather above the common stature, robust, and apparently in good health, was received into the London Hospital, labouring under a moderate sized aneurism of the femoral artery. An operation was proposed to him, to which he readily assented. On entering the theatre, however, he fainted; some wine and water was given to him, which he distinctly swallowed, and the operation was proceeded in, the artery exposed, and the ligature applied, but not tightened. During the operation, it was observed, that no pulsation could be felt in the tumour, but this was accounted for by the fainting. Before tightening the ligature, it was suggested by the operator to wait until the pulsation was re-established: some increased attention was then paid to rouse the dormant energies of the patient, and, it was remarked, that the syncope had continued an unusual time. After the attempts had been some time persevered in, a more attentive observation proved that he was quite dead." (p. 18.)

4. "A man who had been bitten in the finger by a cat, and in whom symptoms resembling those of hydrophobia had been present for twelve hours, being in perfect possession of his mind, summoned an extraordinary resolution to command his spasms, while the excision of the bitten part was performed, and died, evidently exhausted by the effort, in three minutes." (p. 19.)

5. "Sir Astley Cooper, in his Lectures, relates the case of a brewer's servant, a man of middle age, and robust frame, who had suffered much agony for several days, from a thecal abscess, occasioned by a splinter of wood penetrating beneath the nail of the thumb, and who, a few seconds after the matter was discharged by a deep incision, raised himself by a convulsive effort from his bed, and instantly expired." (pp. 19, 20.)

6. "A man whose hand was amputated for a diseased wrist-joint, supported the operation with firmness, and was proceeding in all respects well, when, on the third day from the operation, he suddenly expired. On examination, the lungs were found to contain numerous small tubercles in an incipient stage; and a recent copious effusion of serous fluid had taken place betwixt the tunics of the brain." (p. 24.)

7. "A young man, the subject of amputation of the glans penis and preputium for a cauliflower excrescence of those parts, died in five days, without any manifestation of acute disease. His lungs were discovered, on inspection, to be sprinkled with minute tubercles in the first stage of their formation, of which no suspicion existed." (p. 24.)

8. "A child, three years old, was the subject of lithotomy at St. Thomas's hospital, in the summer of 1805. The operation was admirably performed, and did not exceed one minute by the watch. A slight shivering came over the patient on being replaced in bed, and the natural temperature of the surface was not restored. He was inclined to doze, and a little convulsed, and at two o'clock the following morning died. Although this child suffered considerably from the disease, he was otherwise healthy, and his death, which excited much surprise, was attributed to fright." (p. 89.)

9. "In December, 1807, a child, aged three years and a half, underwent the same operation under as favorable circumstances. An hour after being put to bed, he also chilled; a stupor came over him, but without convulsion; and he gradually sunk into a state of deliquium, and died before ten o'clock the same night." (ib.)

10. "A lad of sixteen was cut at St. Thomas's. Everything went on well in the theatre; the same chilliness and torpor ensued, he sunk rapidly, and died at nine o'clock the same evening.

"In each of these cases, [8, 9, 10] the unfavorable symptoms showed themselves about an hour after the operation; all of them watered, but not so abundantly as usual.

"In neither of the preceding cases was the calculus remarkably large, nor had any unusual hemorrhage or other untoward circumstance occurred during the operation." (p. 90.)

11. "In January, 1808, a young and delicate child was cut for the stone, at Guy's hospital. The operation was favorable, but symptoms of irritation, and ultimately, a state of stupor succeeded, and the child died on the morning of the third day, in strong convulsions." (p. 91)

12. "In 1822, a fine boy of eighteen months, from Essex, was the subject of a private operation for lithotomy. The stone, which was obling, was easily extracted. A somewhat freer hemorrhage than ordinary occurred at the moment of the incision, but it was immediately restrained after the removal of the stone, and was too inconsiderable to create anxiety. The child, although somewhat languid and drowsy during the remainder of the day, wetted freely, and passed the night without complaint; but early on the following morning was attacked with convulsions, and died suddenly. In the afternoon of the same day, the body was minutely and carefully examined; the incision of the prostate was clean and smooth, the bladder healthy, and no morbid appearance whatever presented itself." (p. 92.)

Now, if any three, or any one of these cases had taken place after the administration of ether, and had occurred in the practice of a surgeon of the *post-hoc* school, we should like to know what judgment would have been passed upon them by the operator or by the scientific coroner and his erudite jury. Of course, the ether would have borne the blame; and the medical witnesses might, we are certain, have found, in more than one of the cases, corroborative proofs of their opinions in "congestion of the brain" and "fluidity of blood." But Mr. Travers, knowing, as he does, both the science and the history of his art, is content to let nature bear her own burthen. A great surgeon, moreover, can afford, if we may so speak, to see his patients die in the common way of surgery, without thinking it necessary to look for some new-fangled excuse among the innocent precursors of his knife. Surgery and medicine are full of these scape-goats of ignorance and incapacity, and we fear they are not likely to be soon banished from their borders.

To the foregoing cases from Mr. Travers's work, we could, from our own stores, add others, showing this tendency to sinking in as striking a

point of view, under even slighter injuries. But who has not met with similar instances? Who has not seen or had authentic accounts of individuals dying after the removal of even a small common steatomatous tumour;* nay, after the extirpation of a corn? And, truly, when we look into the records of surgery, and see the vast proportion of individuals who die within a longer or shorter period after capital operations, we cannot refrain our special wonder, when we see even intelligent members of the profession frightened from their propriety by the two or three deaths that have followed operations under ether. The following extract from a paper published in the 'Edinb. Monthly Journ.,' for Jan. 1846, being the report of a speech of Professor Simpson, of Edinburgh, will perhaps teach some of our learned coroners and wise juries, and perchance even some surgeons, a little more caution in drawing inferences as to the fatal effects of etherization in chirurgical operations. We should like to know in what exact proportion of the astounding number of deaths here recorded, the fatal event took place within four or five days, or even one day, after the operation. We should also entertain a slight suspicion that, were the dissections of these cases recorded, a few *might* have exhibited both "congestion of the brain," and "fluidity of blood," although they took place before the epoch of etherization.

"Out of 89 cases in which ovariotomy had been either performed or attempted, 34 sunk, or nearly 4 in every 10 patients died.

"Out of 65 cases, collected by Dr. Cormack, in which the operation had been perfected, 25 died, or between 3 and 4 out of every 10 patients were lost.

"Now Malgaigne has shown, that out of 852 amputations of the extremities of all kinds (including those of the fingers and toes), which were performed in the Parisian hospitals from 1836 to 1841, 332 died, or about 4 out of every 10 proved fatal.

"Among these, out of	201	amputations of the	thigh, 126 died, or 6	in every	10.
... ...	192	...	leg, 106 died, or 5½	...	10.
... ...	91	...	arm, 41 died, or 4½	...	10.

Of the amputations of the thigh, in 46 cases the operation was performed for severe injury of the limb: of these 34 died, or more than 7 out of every 10.

"When we looked to the results of amputation nearer home, the results were not much more encouraging. In the Glasgow Infirmary, from 1795 to 1840, Dr. Lawrie has shown that out of 276 amputations performed, 101 proved fatal, or nearly 4 in 10 died.

"Among these, out of	128	amputations of the	thigh, 46 died, or 3½	in every	10.
... ...	62	...	legs, 30 died, or 5	...	10.
... ...	53	...	arm, 21 died, or 4½	...	10.

"In the Edinburgh Infirmary, during the four years commencing July 1839, there occurred 72 amputations of the thigh, leg, shoulder-joint, arm, and forearm. Of these 72 patients, 37 recovered and 35 died,—or nearly 5 in every 10. Of these amputations, 18 were primary. Out of 4 primary amputations of the leg, 1 patient recovered and 3 died. Out of 4 similar amputations at the shoulder-joint, 1 recovered and 3 died. There was one primary amputation of the arm; the patient died. There were eight primary amputations of the thigh; all the eight patients died. (See Dr. Peacock's Official Reports.)

"Mr. Phillips has collected the histories of 171 cases in which the larger arteries of the body were tied: of these 57 died; or about 3⅓ in every 10. Dr. Inman

* Every one has heard of the death of a celebrated lady of rank, within a few hours after the removal of a small tumour from her head, by Sir Astley Cooper.

has collected 199 cases of these operations; 66 died, or about 3⅓ in every 10. Out of 40 cases of ligature of the subclavian artery which he has tabulated, 18 proved fatal, or nearly 5 in every 10 died.

"In his work on Hernia, Sir A. Cooper records 36 deaths among 77 operations for that disease, or nearly 5 in every 10 died. Dr. Inman has collated 545 cases of operation for hernia; 260 proved fatal, or nearly 5 in every 10 of the patients died.

"In the earlier years of life lithotomy is comparatively a safe and legitimate operation, and few die. But it is quite different when the operation is submitted to at 40 years of age, and upwards. At and above this term of life, Dr. Willis has shown, from numerous statistical returns, that from 2 to 5 out of every 10 operated upon die.

"Even what we deem slighter operations, are sometimes attended by no inconsiderable danger to life. Out of 95 cases of excision of the mamma, referred to in Dr. Cormack's Journal for February 1843,—20 died, or 2 in every 10. In how many cases of the remaining 75 would the disease inevitably return and ultimately destroy the patient?

"Ovariotomy then is fatal in the proportion of about 35 or 40 in every 100 operated upon, but in most capital operations we singly have as high or even a higher mortality than 35 or 40 per cent. Amputation of the thigh is higher. So is amputation of the arm. Ligature of the subclavian, for aneurism, is higher. Tying the innominata is fatal in every case. The operation for hernia has a higher mortality. Lithotomy is as fatal in most hands after the middle term of life. Even amputation of the leg below the knee is scarcely more safe, or at all events as many, or more, die after amputation of the leg, in the hospital practice of Paris and Glasgow, as die after ovariotomy."

Keeping in view the facts now detailed, and the inferences to which they obviously lead, we may state, in a very few words, the merits of the question at issue.

On the one hand, we have three cases out of the many hundred of etherized patients subjected to capital operations, in which the patients, after the departure of all the peculiar effects or primary symptoms produced by etherization, such as sopor, insensibility, &c., succumbed with a set of other and equally peculiar secondary symptoms, well known to surgeons, well known to terminate frequently in death, and, in the cases in question, *presenting not one peculiarity to distinguish them from the old ordinary cases of "sinking from shock."* On the other side, we have many thousands of instances in which the same process of etherization was had recourse to, for slight operations, such as extraction of teeth, or for mere experiment, and in which all the same primary phenomena of etherization were as effectually induced as in the others, and yet, *not one example of the occurrence of the peculiar secondary symptoms referred to,* much less any instance of death. If, in the three fatal cases, the etherization was, in any way, the source of these peculiar symptoms, or the cause of death, is it not most extraordinary that in the other thousands of instances, it should not have given rise to some of the symptoms which in these three cases preceded death, or even to death itself? What was the *sole peculiarity* that existed in the two sets of cases that had relation to the process of etherization? Only the important one, that in the set in which the deaths supervened, there was a severe operation, and in the other there was not. Is not the conclusion irresistible—that in these fatal cases, *it was the operation, not the ether which killed the patients?*

In adopting this conclusion, we beg to guard ourselves against the imputation of denying that the process of etherization can be productive of injurious or even fatal results. We believe that it both can and will be productive of both. In what we have stated we have been merely attempting to show, that, according to legitimate reasoning, we are not justified in attributing the particular deaths in question, to etherization. It is not, moreover, in the way in which it has been presumed to act in these cases, that we expect ether will be injurious; but in its immediate and primary effects, as by inducing axphyxia and coma, from improper or excessive administration. As we have already said, we are surprised that events of this kind have not already presented themselves, more especially as the most impure alcoholic ether has been used by some operators, and a truly axphyxiating apparatus (viz., the common bladder and stop-cock, without a valve,) by others. The very originator of the plan, Dr. Jackson, not only contemplated such results, but even considered beforehand the best means for remedying the evils when they should occur. He recommends the inhalation of oxygen gas. But who has oxygen gas always at hand? And if we wait to prepare it, our patient may probably be dead. Sir H. Davy took nitrous oxide when he had nearly axphyxiated himself. We believe the open air, sprinkling with cold water, and, if necessary, the artificial inflation of the lungs by common air, are all that will be requisite, or that can be done. Very probably, also, individuals will be met with of such peculiar idiosyncrasies as to suffer injuriously, and even fatally, from the primary or secondary effects of the ether, even where the process was best administered. The non-occurrence hitherto of injury or death from any or all of these causes is, as we have already said, a singular proof of the innocuousness of etherization. An ingenious and learned friend of ours in Edinburgh, to whom the medical world is much indebted, and who thinks with us as to the innocuousness of ether, and the scientific authority of coroners' juries, supplies us with an amusing analogical illustration of this presumed innocuousness, which we will here give. The argument, it will be seen, proceeds on the assumption that intoxication by Scotch whisky taken into the stomach, is analogous to the intoxication produced by ether inhaled into the lungs. If the analogy is denied, the argument must fall to the ground. The fact, however, is curious, and here it is: "The other day (says our correspondent) I found on inquiry, that since Dr. Tait has been surgeon to our police here, not less than 27,000 people have been brought to the police offices *drunk*, and deeply so. Of these 27,000, *three* only have died, (except in metaphor,) and these three *from exposure to cold*, &c., *along with the whisky*. This is one death in 9,000. Now, could you give 27,000 black draughts to 27,000 patients, and show such a small list of killed and wounded? I take it that more than *three* would abscond from this life under diarrhœa. And so with regard to any other active medicine. *Intoxication*, then, would appear to be one of the safest *therapeutic* states we can induce!"

But as this is a somewhat ticklish line of argumentation for teetotallers, we must abandon it; and return to our text, with a word or two on the apocryphal deaths which have ensued from etherization, and which have been sufficiently numerous in London professional gossip. One day we had death from asphyxia; another, from coma; another from hemoptysis;

some from convulsions; a few from pneumonia; and one or two from actual incremation or explosion through the accidental firing of the etherial vapour within the air-passages! We have not had time to investigate all these terrible cases; but we may state that we traced the one which seemed the best authenticated—that from *hemoptysis*—from its full-blown majesty in after-dinner gossip, to its humble source in the hospital. And this was the case, as the man himself detailed it to us: a day or to after a successful operation for hernia, under etherization, the man pricked his gums while picking his teeth with a pin: and it was the product of *this* operation, not of the ether, seen in the spitting pot by the patient's bed-side, that was bruited about town, as of itself sufficient to settle the question of etherization in all future time! *Ex uno disce.*

But although etherization has not been found actually to kill, or even to give rise to any results seriously injurious to the patient, it may nevertheless have disadvantages, of more or less importance, in relation to the result of some particular operations, or in relation to the surgeon's performance of them. We have not time to enter upon any discussion of points of this kind, and must content ourselves with merely indicating a few of them. The future experience of surgeons will, no doubt, eventually settle satisfactorily all that is doubtful in these things at present.

1. In operating upon persons completely etherized, some surgeons, from the impression which they say is conveyed to them as if they were operating on the dead body, fear that their ancient knowledge may prove at fault under these novel circumstances, and that their hands may forget their wonted cunning. But the objection is invalid. The new race of surgeons will be educated in ether, that is, if ether, as we would fain hope, is destined to become an established power—the medicamentum doloris—in the armamentarium of surgery; and, surely the erudite hand of a Liston, or of any other of our admirable operators, will soon, like the dyer's, "be subdued to that it works in."

2. Some surgeons still cling to the old notion, that the *feeling of pain*, not the mere *expression* of it, during surgical operations, is beneficial to the patient, a promotive of recovery. We will not argue with such advocates any more than with the eel-skinners. We say, and we think, with the poet, according to our own interpretation of his words,—

> "Ponamus nimios gemitus: flagrantior æquo
> Non debet dolor esse viri, nec vulnere major."

But to put out of the question all the immediate horrors, the mental misery of pain, it is surely monstrous physiology to regard it as anything less than a most enormous practical evil in surgery, and as, in itself, productive of the most grievous results. Any one who doubts this, we refer to Mr. Travers's work, already so largely quoted. He devotes one whole section to the illustration of the dangerous and fatal effects of pain. It commences with the following sentence: "Pain, when amounting to a certain degree of intensity and duration, is of itself destructive." (p. 48.) And ends (nearly) with these: "Pain, in excess, exhausts the principle of life, so that either its continuance without intermission, or the superaddition of the slightest shock subsequent to its endurance for a certain period is fatal. In operations protracted by unforeseen difficulties, as in

cases of lithotomy, in which a stone is of such magnitude as to require crushing, the patient has begun to die upon the table." (p. 56.) So far, then, from deprecating the abolition of pain, on scientific grounds, in surgical operations, we are justified, by all science and all philosophy, physiological and metaphysical, as well as by experience, in regarding it as the direct source of much safety to the patients, and a fruitful preserver of life itself. And here we cannot help expressing a suspicion that the fatal results of two of the cases formerly noticed, might possibly have been obviated had the patients been placed under the full influence of ether.

Though, then, we utterly deny that pain is, in any case, useful *per se*, and maintain that it ought to be eschewed wherever it can be dispensed with, we are prepared to admit that in certain operations, it may possibly be productive of help to the surgeon, and therefore must be tolerated. The operation of lithotrity is said to be an instance in point, in which the surgeon, while extracting the foreign body, is warned by the pain, against mistaking a fold of the bladder for a fragment of the stone.

3. The process of etherization may be also inexpedient in some surgical operations, on account of the unconsciousness attending it; the voluntary acts of the patient, in obedience to the surgeon, being here conducive to the proper performance of the operation. This may be the case in certain operations about the throat and neck, and also in some other cases.

4. An objection has been made to the practice by some surgeons, on account of the influence it has on the operator's mind, by hurrying him and rendering him nervous, under the apprehension that the patient may wake up at any moment. This would be an objection of weight, if the insensibility could not be rendered sufficiently profound and permanent, which we believe it can, in almost every case.

But now, having given to surgery and to surgeons, as in duty bound, the lion's share of our article, we must, before concluding, bestow a few of our pages on medicine and midwifery.

Although certainly very promising in contemplation, and although often adopted, in various forms, by physicians, Pneumatic Medicine has never been productive of great practical benefits, and has consequently never thriven lustily with practising doctors. The most vigorous attempt ever made to establish it as a branch of practice, was that of Dr. Beddoes, at the close of the last century: but the brilliant hopes and promises of that enthusiastic genius, died with him and have not been since revived. To be sure, we have had sundry additions to his modified and factitious airs, or vapours, such as tar-smoke, iodine, chlorine, &c. &c.; and one of the immortal family of quacks is even now attempting to resuscitate in London the old favorite practice of the inhalation of "vital air," which once had so many partisans. The reintroduction of the vapours of ether into practice, under its present mighty patronage, is likely to give a new impulse to pneumatic medicine generally: we hope it will; as we are of opinion that it holds out to us not a little promise of good. Formerly, it was employed solely for its direct influence on the air-passages. It may, possibly, be still found useful in this way; but the point of view in which we are now regarding it, is in relation to its secondary effects on the nervous system and the blood, when administered so as to induce insensibility.

There are a good many cases in which this application of ether is likely to be useful; in several it has been already tried, and in some with beneficial effects. In most cases of violent pain, when the pain is persistent, it seems to deserve a trial. In pains of a spasmodic and neuralgic kind it holds out great promise of benefit; and in several of such cases, its utility has been already proved. In a case of *neuralgia* related in the 'Gazette des Hôpitaux,' the pain was removed "as if by enchantment," and did not return for some time. We have heard of similar cases in this country. In a case of *colica pictonum*, M. Bouvier, of Paris, used it with great benefit. This case had resisted the ordinary means of purgatives, warm baths, *opium*, etc., for three days. On being etherized, the patient slept forty minutes and awaked free from pain; he remained in this state for three hours, then slept for two hours more, and passed a good night. The pains did not return. (Bull. de l'Acad., 15 Feb.) *Dysmenorrhœa* is one of the diseases in which we should expect most benefit from ether. We know of four or five cases in which it has been used; and here the relief was immediate and complete. M. Bouvier tried ether in a case of *puerperal mania*, with very beneficial results. In this case there had been no sleep for a fortnight before using the ether: its use was followed on two occasions by quiet, if not sleep (un calme de quelques heures. *Ibid.*) M. Jobert also employed it in a case of simple insanity, with the effect of inducing sleep and restoring (temporarily) a state of rationality; no ill effects followed. The remedy was also tried in a case of recent hysterical mania in one of the London hospitals, with the effect of inducing sleep, after prolonged wakefulness. Do not these results justify us in expecting etherization to be an important therapeutical means in insanity?

In concluding this part of our subject, though we must still admit the general justice of the opinion given by Davy fifty years since, we trust the events just detailed justify us in believing that we have made some real progress since his time. "Pneumatic chemistry in its application to medicine is an art in its infancy, weak, almost useless, but apparently possessed of capabilities of improvement. To be rendered strong and mature, it must be nourished by facts, strengthened by exercise, and cautiously directed in the application of its powers by rational scepticism."—(Researches, p. 558-9.)

Having thus disposed of the question of etherization, as it regards the crafts of the Chirurgeon and Physician, respectively, it becomes, in the last place, our duty to endeavour to do like justice to the midwives, masculine and feminine. But, alas, if our liberality towards surgery in respect of space has made us stingy towards the doctors, we fear it must make us seem shabbier still anent our good friends the Howdies. And this is especially unfortunate and no less unfair, seeing that the very field of their daily and hourly labours is PAIN. While their brethren deal in this commodity only occasionally and as an exception, the midwives, at least in these days and these lands of civilization, live entirely on the traffic. To encourage ether, then, the queller of pain, would seem, at first sight, to ruin the trade utterly. But this is not the case here any more than in ordinary commerce; there would only be a change in the nature of the commodity, not an abolition of the remunerating process. And doubtless, our good friend, Professor Simpson, who must be held respon-

sible for the present sacrilegious attempt to do away with the primal curse on womankind, like a legitimate and faithful son of Apollo and Lucina, as he is, was well aware of this before he set about preaching the crusade of obstetrical etherization to his brethren. And, verily, the craft is here in no danger; even if the Professor's most sanguine anticipations should be realized, which we are told go to this extent,—that fifty years hence ether will be so universal in midwifery that *pain* will be the exception not the rule, and that the mothers of future men will bring forth, not in the travail and the woe of the mortal couch, but in Elysian dreams on beds of asphodel! Be this as it may, it is certainly a matter of surpassing interest—need we not say, of delightful wonder—to know that already, by means of etherization, many women have been freed from all the pains and perils of childbed, in the hands of Professor Simpson and his followers. In a communication which we have received from Edinburgh, dated the 22d of March, Dr. Simpson states that he had, up to that date, used etherization some forty or fifty times, with the most perfect safety and success. We understand that he has kept it up *for hours,*—in one woman four, in another six hours,—without the fœtal heart varying above ten or twelve beats during the whole time, the mothers in both cases recovering perfectly, and both, of course, astonished at being delivered without being aware of it. We believe that Dr. Simpson, in making these statements, still inculcates caution in the use of the new means; justly regarding all his own trials hitherto, bold as they are, as merely experimental, and as only first-fruits which, however delightful and promising, may not be the positive harbingers of an abundant and a wholesome harvest.

The question of the relation of etherization to midwifery, is much more complex than in the case either of surgery or medicine. In the former, we have, generally speaking, but to consider its safety and its capacity of destroying pain; in midwifery we have to take into account numerous important muscular actions, reflex and voluntary, and to consider and determine how far these are intefered with by the ether, and whether its interference is calculated to modify injuriously, or to impede, movements essential to the parturient process. It is well known that the muscular efforts involved in the act of parturition are partly voluntary, though mainly reflex and automatic; and it must have been felt, previously to actual trial, to be a very doubtful problem, whether a means having the power to destroy, for the time, all volitional acts, might not interfere most seriously with the expulsive process. We do not pretend to be very learned in these matters; and we are not aware that the whole parturient process, has as yet been so fully and satisfactorily analysed by obstetricians as to make the present point one of easy solution. At any rate, we have not room at present to enter upon its discussion; and we will, therefore, content ourselves with remarking, as we remarked in the case of surgical operations, that, in the actual stage of the inquiry at which we are arrived, we must be more guided by experience than reasoning. Assuredly, if the parturient act could be equally well—or nearly as well—accomplished under the influence of ether as without it, and if the after results were equally good in both cases, there would be no further question as to the employment of etherization in midwifery. Our wives would have it, in

spite of the doctors,—aye, whether there were law or Scripture in its favour. But the new means here, as in surgery, is only still on its trial; and it will, probably, be some time before the final verdict is returned. In the meantime, we are bound to say that all the evidence hitherto published is decidedly in favour of the safety and utility of etherization in midwifery; and we think accoucheurs are not simply justified in making trial of the ether in protracted labours accompanied with great suffering, but that it is their duty to try it—or, at least, to propose the trial of it to their patients. They may safely state the fact that, hitherto, not only no death, but no untoward event whatever, has been the consequence of the new practice, while the relief afforded by it has been uniformly great. Our present experience, to be sure, is not large, being, as far as we know, only the forty or fifty cases above referred to as occurring in Professor Simpson's practice, five cases detailed by M. Dubois to the French Academy; and a few others in our own and the foreign journals.

M. Dubois's opinion is, on the whole, not in favour of the employment of ether in midwifery, although he admits that he has seen no ill effects that he could, with certainty, attribute to it. He thinks, "that it should be restrained to a very limited number of cases, the nature of which ulterior experience will better allow us to determine." He, however, confesses that the result of the cases he has treated in this manner have lessened the fears with which be originally entered on the trial. We leave the Professor and the Baron—the doughty champions and learned representatives of the Obstetrics of Paris and Edinburgh—to fight the battle between them. Time, at least, will ere long determine which of the two is in the right. We are disposed to believe that neither is absolutely so; and that here, as in so many other instances of clashing opinions, the truth lies between.

The following are the general conclusions drawn by M. Dubois from his experience in obstetrical etherization.

"From my foregoing observations on the subject of ether, considered in its application to cases of midwifery, I feel myself justified in drawing the following conclusions:

"1st. That the inhalation of ether has the power of preventing pain during obstetric operations;

"2d. That it may also momentarily suspend the natural pains of labour;

"3d. That the state of ebriety induced by the inhalation of ether does not suspend uterine contraction when the latter is decidedly set in, and takes place at short intervals; and that it does not impede the synergetic action of the abdominal muscles.

"4th. That the state of ebriety appears to lessen the natural resistance which the perinæal muscles oppose to the expulsion of the head.

"5th. That the inhalation of ether has not appeared to exert any bad influence over the life or health of the child." (Lancet, March 6.)

We have taken no notice of the numerous and varied forms of apparatus that have been recommended and used for the administration of ether. Our readers must be sufficiently acquainted with them, as they have been almost all both described and figured in the weekly journals. The most ordinary type of these is that figured in Mr. Robinson's useful pamphlet,

which has at least the merit of success in its favour, as we believe Mr. Robinson has made more extensive use of etherization in his own practice than any other dentist in London, and has also been much employed by the surgeons in administering ether in their more important operations. One on the same general plan as Mr. Robinson's, but with what we consider as an improvement in the mouth-piece, and in the regulating valves, has been introduced by Mr. Squire, and is also much used. It is that employed by Mr. Liston in University College Hospital, and also in his private practice. Mr. Startin's apparatus is on a somewhat different principle, the air having to pass through water before it becomes charged with the etherial vapour. It seems to answer well, and is the apparatus that has been chiefly employed at King's College Hospital. Mr. Tracy, of St. Bartholomew's, has introduced one on a smaller scale, which has been used successfully by the surgeons of that hospital in their numerous operations. There are many others, planned by Mr. Smee, Mr. Weiss, etc. etc. Dr. Snow's apparatus, that which has been principally used at St. George's, has the important advantage of enabling the operator to know exactly the strength of the vapour administered, and to regulate its quantity, and also to ascertain the precise amount of fluid ether consumed. For a figure of this apparatus, and for important observations on the principles on which etherization should be generally conducted, we refer the reader to the 'Medical Gazette,' of March 19.

On reaching this, which we had looked to as the goal of our labours for the present, and finding that there still remained a small portion of our allotted space unoccupied, we at first thought of filling it with a summary of the most important conclusions supplied by all that precedes, and also such general inferences and deductions as might appear to us to be justified by the consideration of the whole subject of etherization. On reflection, however, we feel that such an attempt would have the appearance of giving to what we have written an aspect of greater formality and completeness than it deserves, and tend to divest it of that sketchy and fragmentary character to which alone it can lay claim. We are, therefore, glad to be able to substitute for such generalities, which, at the present time, must be necessarily imperfect, some extracts from three important papers on our subject, which have only reached us since the preceding article was in type. The first two of these give us the views of two very eminent physiologists, on the physiological action of ether, which we have merely hinted at in the preceding pages, (see p. 555). The third is valuable, partly on the same account, but principally in a practical point of view.

I. *Experiments on the Effect of Inhalation of Ether on the Nervous System of Animals.* By F. A. Longet.

This is the most elaborate and important memoir that has yet appeared on the subject of ether. We can only find room for a few of the general propositions in which the results of the experiments are summed up.

1. In etherized animals, there is absolute momentary suspension of

sensibility, as well in all parts of the cerebro-spinal axis usually sensitive, as in the nervous trunks themselves.

6. The action of ether on the nervous system is much more directly and completely stupefying than alcohol, which merely renders the sensibility more obtuse without suspending it entirely, at least in the nervous centres.

7. Ether abolishes, momentarily, but completely, the excito-motor, or reflex action of the spinal marrow and medulla oblongata; and consequently acts in an opposite manner to strychnine and opium, which exalt it.

9. The functions of the encephalic centres always are suspended before those of the spinal marrow, and return before them.

10. Ether supplies a new means of isolating, in the living animal, the seat of general sensibility from the seat of the intellect and will.

11. In animals, we can so graduate the action of ether as to produce, at will, two stages, which I name—1, *Etherization of the cerebral lobes*;—2, *Etherization of the annular protuberance*.*

13. Ether is only preventive of pain when it acts on the annular protuberance.

14. In animals which have suffered etherization of the annular protuberance, this organ always recovers its functions as the perceptive centre of tactile impressions, before it becomes itself a sensible organ.†

15. The course of the phenomena of etherization is far from being the same in men as in animals.

16. The process of *de-etherization* of the annular protuberance may begin while *etherization* of the cerebral lobes still continues: this explains the cries that take place towards the end of some operations which commence amid the most perfect quiet,—cries, however, of which the patient retains no recollection on awaking.

17. The *true chirurgical period* corresponds to that of *etherization of the annular protuberance*, or absolute insensibility.

* The following are the phenomena in animals (dogs and rabbits) which M. Longet considers as showing the etherization of these two parts respectively :

"1. In etherization, when the animal is no longer able to stand, it falls on its side stupefied, and then sinks into a profound sleep, is no longer conscious of external impressions, or able to perform any voluntary motions ; though at the same time it still cries, and also winces on being pinched, but without *awaking*, so as to react, in an efficient and voluntary manner, against the external violence : this stage of the process I call *etherization of the cerebral lobes*, and of the other parts of the encephalon,* except the annular protuberance and *medulla oblongata* (le bulbe rachidien).

"2. When, in the further continuance of etherization, the animals no longer cry, or move, or feel, even when the most sensitive part of their nervous system is twitched or torn ; I call this stage *etherization of the annular protuberance*, the effects of which are united to those of the preceding stage."

† Recouvre toujours son rôle de centre perceptif des impressions tactiles, avant de redevenir lui-même organe sensible.

* Cerebellum, tubercula quadrigemina, corpora striata, thalami optici.

18. For some time after the faculty of sensation is restored in etherized animals, there is transient exaltation of the sensibility.

20. At a particular period of the experiments, the blood becomes almost black in the arteries: *insensibility always shows itself previously to this occurrence.*

21. If, after the point of total insensibility, inhalation is continued, the animals (rabbits), *cæteris paribus,* die within the space of from six to twelve minutes.

22. On the contrary, on mixing a greater quantity of air with the vapour, the period of insensibilty may be kept up a long time (three quarters of an hour and more) without injury to the animal's life.

23. Ether introduced into the stomach does *not* produce insensibility in animals.

24. In etherization, the functions of the *ganglionic nervous system* appear to be over-excited, and this system appears to become a sort of *diverticulum* for the nervous power which has, for the time, abandoned the *cerebro-spinal system.*

25. The death of etherized animals is, perhaps, owing to a sort of asphyxia originating particularly in the respiratory nervous centre (*le centre nerveux réspiratoire.*)—*Archives de Méd., Mars,* 1847.

II. *On the Effects of inhalation of Ether on the nervous centres.* By M. Flourens.

M. Flourens' experiments were made on dogs. After the animals were rendered insensible by the inhalation, the spinal marrow and medulla oblongata were laid bare and submitted to excitation and injury of various kinds. The experiments were—1, on the spinal marrow; 2, on the medulla oblongata; and 3, carried to the extent of producing death. The following are the conclusions drawn from each of the first two series of experiments, and also from the whole.

"Ether has the faculty of destroying temporarily, in the spinal marrow, the principle of sensitiveness* (principe du sentiment) and motion. The principle of sensitiveness always disappears first. When the effect of the ether passes off, the spinal marrow recovers its ordinary powers."

Under the action of ether, the nervous centres lose their powers in regular succession; first, the cerebral lobes lose theirs, viz., the intellect; next, the cerebellum loses its, viz., the power of regulating locomotion; thirdly, the spinal marrow loses the principle of sensitiveness and of motion; the medulla oblongata still retains its functions, and the animal continues to live: with loss of power in the medulla oblongata, life is lost.

"It is impossible [continues M. Flourens] to observe a single case of etherization without being struck with the similarity of its phenomena to those of asphyxia. I subjected two dogs to the simplest form of asphyxia, by confining them in a limited

* We purposely use this less common word, and also give the original, for fear of misleading.

quantity of atmospheric air. The result was, a state of asphyxia similar to etherization. On baring the spinal marrow, the animals felt nothing, nor did they when the sensorial portions (parties sensoriales) of the cord were pricked or cut. On pinching the motor portions, there were only a few feeble muscular contractions. There is, therefore, a real relation, a marked analogy, between etherization and asphyxia. But in ordinary asphyxia, the nervous system loses its powers under the action of black or deoxygenated blood, while, in etherization, it loses them, in the first place, under the direct action of the ether. This is the only difference; for in both, there is the same loss of sensitiveness (sentiment) and voluntary motion, the same continuance, at least for a time, of the respiratory movements, —in a word, the same survival of the medulla oblongata and medulla spinalis. In this way, etherization lays open to us the true mechanism of asphyxia, in other words, the *successive death* of the nervous centres in asphyxia. And it is in this successive progress of the death of the nervous centres, that the great value of these new experiments consists. Etherization, like mechanical experiment, isolates respectively, the intellectual faculties, the co-ordination of muscular motions, sensibility, mobility, life. In the etherized animal, one point alone—*nodus vitalis* —survives; and while it survives, all other parts live, at least, with a latent life, and are capable of resuming their complete life (*leur vie entière.*) This point dead, all dies." (*Gazette des Hôpitaux*, 20 *Mars*, 1847.)

At the sacrifice of excluding other important matter from our pages, we will also here transcribe the greater portion of a valuable paper of Dr. Snow, that has only this day (March 26) appeared in the London Medical Gazette.

We strongly recommend this paper to the perusal of our readers. Dr. Snow has paid very great attention to the whole subject of etherization, and has had much practical experience in applying the agent. The reader will observe that several passages in Dr. Snow's paper go to the elucidation of points but slightly or doubtfully touched on by ourselves in our article. One paragraph on "the psychological phenomena" will be found to have an interesting relation to the experiments of M. Longet and Flourens.

III. *On the Inhalation of the vapour of Ether.* By John Snow, m.d.

"In those instances in which I have watched the pupil of the eye narrowly, I have observed it to dilate, as the patient is getting under the influence of the vapour. This dilatation is, however, but transitory, and the pupil usually becomes somewhat contracted, and the eye turned up, as in sleep, as soon as the patient becomes insensible to pain. The breathing at the same time becomes deep, slow, and regular, and there is an absence of voluntary motion and a relaxation of the muscles, the orbicularis muscle ceasing to contract again on the eyelids being raised by the finger. An operation may be commenced in this condition of the patient, with confidence that he will remain as passive as a dead subject. This having been found to be the case, in order to maintain the insensibility without further increasing it, I am in the habit of partly turning the two-way tap to dilute the vapour; and it has seemed to me that by turning it about half way, so as to admit an equal quantity of external air, and reduce the vapour to about 25 per cent., that object has been attained: but more extensive experience is required on this point, and perhaps the proportion required may vary in different patients. This method of continuing a more diluted vapour I have

found to keep up the insensibility better than leaving off the process and resuming it by turns. But if the respiration becomes too slow, or at all stertorous, or if the pulse becomes very small or feeble, the nostrils should be at once liberated, and the admission of fresh air will afford immediate relief. I should think it unsafe to fasten a mask on the face, by means of a strap and buckle going behind the head, or to use any means that would interfere with the instantaneous admission of air, for on one occasion I saw an animal killed by ether by a momentary delay. It was placed in a small glass jar, and when it appeared to have had as much of the vapour as it could bear, I attempted to take it out, but could not reach it with my fingers, and whilst turning round for some means of extricating it, it expired.

. . . .

"In nineteen cases out of twenty in which the pulse was carefully noticed, it increased in frequency during the inhalation, often very much, becoming as frequent as 180 in the minute in some patients in whom, from debility, it was frequent before the process began. Generally the pulse has also become smaller and more feeble. In one instance, that of a lady reduced in strength by malignant disease, it became smaller, but not more frequent; and as soon as the inhalation was discontiuued, it became fuller and stronger than before the inhalation began. The pulse generally recovers its volume almost directly the inhalation is discontinued; in several instances, as in the above, becoming stronger than before; but it remains frequent for some minutes. . . .

. . . . .

"I have seen two cases in which the depressing effect of the inhalation was considerable, and was not followed by reaction directly it was discontinued..... A lady, 41 years of age, in pretty good general health, the patient of Dr. Frederick Bird, inhaled ether on the occasion of having a tumour removed connected with the external generative organs. She inhaled for eight minutes, during which time it was observed that the respiration was feeble and slow. The pulse, however, which had been about natural before the inhalation, became feeble and very frequent, and the patient began to struggle as if suffering from want of breath; the process was discontinued, although she did not appear insensible, and the operation was commenced. She flinched and cried out at the first incision, although she did not afterwards remember the pain. She became very faint during the operation, although there was but little loss of blood, and it was necessary to give brandy, and lower the head to the horizontal posture. Consciousness soon returned, and as some sutures were made in the skin, she spoke coolly of beginning to feel a little pain. The feeling of faintness continued more or less all night, but her recovery was very good. The apparatus in this instance was placed in water at 70°, being lower than the temperature of the room. Two fluid ounces of ether were put in, and three drachms remained; consequently 13 drachms were inhaled, equal to about 709 cubic inches of vapour; and as it was washed ether, each 115 cubic inches would be combined with 100 cubic inches of air; consequently only about 616 cubic inches of air were breathed, making 1325 cubic inches of air and vapour: but in eight minutes the patient ought to have breathed about 2400 cubic inches of air alone. The ether in this instance appeared to act as a sedative to the function of respiration, and the small amount of air breathed may perhaps account for the depressing effects.

"In two or three instances there have been some struggling and a distended state of the superficial veins, the skin being rather purple, and the conjunctivæ somewhat injected. In one instance this seemed to arise from cough being excited by the vapour, on account of the bronchial membrane being in an irritable state, and in the others I believe it arose from obstructed respiration, which in future may be avoided, rather than from the direct effect of the vapour. By the

kindness of the surgeons to St. George's hospital, I have had the honour of giving the vapour of ether at thirteen surgical operations—most of them important ones—in the hospital during the last six weeks, having the valuable advice of the surgeons, and occasionally also of one or two of the physicians to the hospital, to aid me in so giving it. It has been successful in altogether preventing pain in all the cases but one or two, and even in these there was but very little of the pain that there otherwise would have been; and there have been no ill effects of any kind following the inhalation of the ether. I allude to these cases to remark that five of the patients were children of various ages, from the fifth year upwards, and that they inhaled more easily than the adults generally did; that they were more quickly affected, generally becoming quite insensible in less than two minutes, and always without any of the struggling which sometimes occurred in the adults. For a variety of reasons, and from close observation, I have arrived at the conclusion, that this difference has not arisen strictly from a different effect of ether on subjects of different ages, but from a cause within our control. The same inhaler was used in all, consequently the tubes were wider in proportion for children than for adults. I have described all the passages of the apparatus as not less than five-eighths of an inch in diameter; but such is the description rather of what I wanted, than of any instrument I have used. Valves and tubes such as were already in existence have been made use of, and the caliber in some part of its extent has always been contracted to half an inch, and this I consider only enough for a child, but not for the adult. As only half, and often not so much as half, of what is inhaled is air, it is particularly requisite that the tubes should be wide. I am now getting elastic tubes, valves and mouth-tubes, made purposely for the apparatus, three quarters of an inch in diameter, as wide, in fact, as the barrel of a fowling-piece, and intend to give ether as fair a trial in adults as hitherto, I believe, it has had in children only. The pipe admitting air to the ether will be five-eighths, and all the passages for the air expanded by vapour, three quarters of an inch in diameter. It may be supposed that there is no occasion to make the tubes larger than the trachea, but something ought to be allowed for the friction of the air against the interior of the tubes.

" With respect to the psychological phenomena produced by ether, I have observed that consciousness seems to be lost before the sensibility to pain, and if an operation is commenced in this stage, the patient will flinch, and even utter cries, and give expressions of pain, but will not remember it, and will assert that he has felt none. Metaphysicians have distinguished between sensibility and perception—between mere sensation and the consciousness or knowledge of that sensation, though the two functions have, as they supposed, always been combined. Ether seems to decompose mental phenomena as galvanism decomposes chemical compounds, allowing us to analyse them, and showing that the metaphysicians were right. During the recovery of the patient, consciousness, which first departed, generally returns first, and the curious phenomenon is witnessed of a patient talking, often quite rationally, about the most indifferent matters, whilst his body is being cut or stitched by the surgeon. I have never seen this insensibility to pain during the conscious state except where consciousness had been previously suspended. In the paper on the capillary circulation, in the 'Medical Gazette,' to which I have alluded above, I offered the opinion that the pain of inflammation depended on a great increase of the natural sensibility of the inflamed part. Under the influence of ether we sometimes see the converse of this, viz., what would be pain reduced to an ordinary sensation; thus, some patients, whilst recovering their consciousness, feel the cuts of the surgeon without the smart. A nobleman, the patient of Mr. Tracy, of Hill street, Berkeley square, described the lancing of an abscess as the sensation of something cold touching the part;

the manipulation of the abscess, which at another time would have been painful, he did not feel at all.

"If the patient will remain silent during his recovery from the effects of ether, as he generally will, it is better not to trouble him with questions till he has perfectly regained his faculties, as conversation seems to increase the tendency to excitement of the mind that sometimes exists for a few minutes as the patient is recovering from the effects of ether. This kind of inebriation is sometimes amusing, but is not a desirable part of the effects of ether, more especially on so grave an occasion as a serious surgical operation; and therefore anything that may prevent or diminish it is worthy of attention. The children have all appeared to recover their consciousness very quickly, and without any kind of aberration of mind.

"Any organic disease which impedes the flow of blood through the heart and lungs would seem to contraindicate the exhibition of ether by inhalation, and I should consider a hurried state of the circulation, such as that induced by strong labour-pains, likewise to offer an objection to the process.

. . . .

"In concluding, however, I should wish to observe that I am inclined to look upon the new application of ether as the most valuable discovery in medical science since that of vaccination. From what I have seen, I feel justified in the conclusion that ether may be inhaled for nearly all surgical operations, with the effect of preventing pain, not only with safety and without ill consequences, where due care is taken, but in many cases with the further advantage of improving the patient's prospect of recovery; the pain of an operation forming often a considerable part of what renders it dangerous, and many patients, after ether, having seemed to recover better than might, without it, have been expected. In the amputations performed at St. George's Hospital, whilst the patients were under the influence of ether, it has been remarked, as was stated by Mr. Cutler, on February 11th, that there has been an absence of the painful spasmodic starting of the stump, which usually renders it necessary for a nurse to sit and hold it for some hours after the operation."

(Med. Gazette, March 26th, 1847.)

PART SECOND.

Bibliographical Notices.

Art. I.—*On Indigestion and certain Bilious Disorders often conjoined with it. To which are added, Short Notes on Diet.* By George Child, m.d., Physician to the Westminster General Dispensary.—*London,* 1847. 8vo, pp. 219.

We cannot say that we have met in this work with any novel facts, or with any masterly grouping of old ones. The book, in short, is just another of those (of which there are already a great deal too many) which fifty or a hundred practitioners of medium intelligence, industry, and observation could easily write. When we say this, we merely mean to intimate that in Dr. Child's volume we are not to look for any essentially new views, new inductions, or new plans of treatment. The work is divided into twenty chapters, of part of the contents of which we propose to give a cursory notice.

In chapters first and second, we find nothing whatever new. We think Dr. Child's definition of dyspepsia, at the very outset, may be objected to as at least incomplete. It is "habitual uneasiness while the food in the stomach is being converted into chyme." Now though, according to the strict etymology of the word dyspepsia (which, however, Dr. Child does not refer to), the above definition may pass, yet practically and as regards the word indigestion (which the author allows to be a synonyme of dyspepsia) the case is different. For undoubtedly indigestion is something *more* than mere habitual "uneasiness" in the process of chymification. It is also *imperfect* chymification, or we may say (rather, however, to illustrate our meaning, than to express ourselves with philosophic accuracy) it is *morbid* chymification. There are, indeed, cases in which chymification is obviously most imperfect, but which are characterized by little or no pain or uneasiness, at least in the stomach itself.

At p. 7 et seq. Dr. Child notices the effects of food undue in quality or quantity, as leading to indigestion, and here he sets himself to repudiate the notion that "the diet of the rich is more calculated to produce indigestion than that of the poorer classes," and to cite, in order to controvert, the everlastingly-quoted words of the Roman satirist, "innumerabiles esse morbos miraris? Coquos numera." The author then proceeds to observe that as the means of the rich enable them to purchase better articles of food than the poor, and as in the case of the latter "the same dish is often warmed up again" (p. 9), so, for these and sundry other reasons,

the rich, as regards diet and cookery, are better off than the poor. Here, again, we think there is slight special pleading, or rather a misapprehension of the true spirit of the objection which the author is combating. No one ever, we believe, would contend that a good article of food is preferable to an inferior one: a sufficiently cooked to an imperfectly cooked dish. But the point aimed at in the query of the satirist and the real *practical* part of the question is, whether the plain and even coarse diet of the poor is not found, *provided only it be sufficient in quantity*, to lead *actually* to less disease, and to the formation of fewer of those artificial tastes, from which disease almost unavoidably results, than the more recondite cookery of the rich does? We think that, looking at the simple and actual results of the two "systems," the question admits but of one answer, though, perhaps, handled as it is by the author, it is one fitted rather for discussion in a popular than a professional volume.

At chapter vii, p. 83, "the various pains" which characterize indigestion are treated of. Some of these pains, singular in themselves, are very singularly named and described by the author.

Taking up separately these various pains, the author gives his theory of their respective causes, and lays down what he considers the appropriate treatment. This piecemeal method of treating the subject, this attempt to make it be believed that these "pains" are susceptible of an exact pathogenical and pathological classification, is apt to be extremely hurtful in practice. Persons who are imposed on by this dogmatic nomenclature of the author (we do not use the word dogmatic in its offensive meaning) are led to suppose that a far fuller and distincter insight can be gained into the complexities and obscurities of dyspeptic derangements, than can be practically obtained. In no part of the work, perhaps, is this endeavour to give precision to subjects actually incapable of it, more visible and more unsuccessful, than in the observations on "bilious headache," "gastric headache," and "gastrobilious headache" (pp. 114 to 129 inclusive), which affections, both from the topical proximity and nervous connexions and sympathy of the organs concerned in them, and from our still great ignorance of the physiology and pathology of these, are totally insusceptible of diagnostic distinctions so precise as those attempted to be laid down by Dr. Child.

To a great extent the therapeutical directions are distributed through the work, following the descriptions of the several forms of dyspepsia, to which those directions apply; but chapter xix, page 191, is devoted to the "general treatment of indigestion," in which, however, we do not find anything materially new or important. We cannot, however, refrain from giving one quotation from this part of the work, with a view to afford the reader an opportunity of judging of the inductive powers of the author. Often a little circumstance lets us into a knowledge of an author's capacity and mode of drawing conclusions from premises. The passage we refer to occurs at page 196, and is as follows:—"The well-known virtue of nitrate of silver, internally administered, in removing morbid irritability of the stomach, led me to expect that its external application would prove a highly important counter-irritant in dyspeptic congestion. Accordingly, I have often rubbed it on the epigastrium, but I must confess I have been

disappointed with its effects." It would not be easy to write a sentence more open to criticism than the above, but the reader will discover this without our aid.

Chapter xx is occupied with directions of a purely popular character, in regard to diet, &c.

We have been struck, in this volume, with the little reference to what may be called the hygienic treatment of dyspeptic disorders, which, of all others, are most benefited by such treatment. Change of scene and place and occupation, mental distraction, relaxation, excitement, fresh air, exercise, walking, riding, jumping, racing, dancing, are little, if at all, alluded to, yet a large proportion of cases of "dyspepsia and the bilious disorders often conjoined with it" would vanish under the adoption of these means alone, along, of course, with plain and moderate diet; while, *without* these means, no variety or amount of *medicinal* treatment will much or at least radically avail. But, alas! in most of our practical works, "the trail of the serpent is over all:"—drugs—drugs—nothing but drugs; trade—trade—nothing but trade; while that which mainly appertains to the more refined and higher philosophy of our noble and majestic art, is rejected as valueless, or passed by unheeded and unknown.

Art. II.—*THE WHY AND THE WHEREFORE; or, the Philosophy of Life, Health, and Disease. New and Original Views explanatory of their Natural Causes and Connexion; and of the Treatment of Disease upon a few General Principles, based upon the Laws of Nature and Common Sense*; *with Rules for the Preservation of Health and Renovation of the System. The Fruit of Thirty Years' Observation and Experience.* By Charles Searle, m.d., m.r.c.s.e., and late of the E. I. C. Madras Establishment.—*London,* 1846. 8vo, pp. 266.

Dr. Searle dates his book from Bath; we infer, therefore, that he is practising as a physician there. He proposes to present the reader "with a complete system of the science and practice of medicine, and of the philosophy of life and health." His reader is not, however, expected to be the professional and educated man, and therefore a competent judge of the author's merits as a writer and as a practitioner, but rather the uneducated (we mean technically uneducated) layman. This is amply shown by the following long-winded paragraph:

"Being of opinion that the principles of the subject are within the comprehension of every intelligent person, and that a distinct knowledge of the principles of any subject is essential to its successful practice; and seeing the lamentable ignorance that exists in these matters, and the charlatanism which prevails, I have been induced to address myself to the public rather than to the profession, not from any disrespect to its members—far otherwise, for a more enlightened and liberal-spirited class of men nowhere exists—but with the view of laying open to the public the delusions of incompetent pretenders, and of imparting that amount of knowledge which every individual ought to possess, on a subject of such pre-eminently personal importance, and especially so as regards the causes and

prevention of disease, and of those calamities we see daily recorded in the public journals, such as persons falling down dead in the streets." (Preface, p. v.

We think this a fundamental and fatal mistake in the plan of Dr. Searle's work, for mistake we believe it to be, and not a premeditated attempt at quackery. This is much to be regretted; for if Dr. Searle had directed the energy and industry which he evidently possesses to a more judicious plan, he might have done good service to the public and the profession; but as the matter stands, his book is too abstruse for the general reader, and too theoretical and generalizing for the professional reader; so his labours go for nothing. Dr. Searle might have written in elegant language on dietetics; he has written *de omnibus rebus et quibusdam aliis*. He might have made the established principles of pathology popular and intelligible; he has attempted to make popular some theoretical notions of his own. He might have laid down a safe system of elementary therapeutics; he has, we fear, made public a somewhat dangerous system—and on this point we need only refer his recommendation of opium as a remedy in mental anxiety. He might at least have written scholarly; he has written with "muggishness," to use a word we see in print for the first time in his book.

An example of "muggish" writing, addressed, be it observed; to "the public," for the noble purpose set forth in the foregoing quotation, is subjoined.

"153. Hysterical breathing or other spasmodic affection of the muscles of the tongue, the voice, or those muscles associated in swallowing, (circumstances concomitant with the mental excitement, and connected with the irritation in the brain involving that portion of it from which the nerves of these organs arise—the medulla oblongata and summit of the spinal marrow—inducing the patient to protrude his tongue and to utter discordant sounds, occasioning difficulty of swallowing or breathing,) if not to be relieved by a full dose of opium, which should be administered if necessary, exhibit the advanced condition of inflammation, when cupping or leeching the back of the head to a small extent daily, and a blister kept open between the shoulders or behind the ears, are the proper remedies, following them up by the insertion of a seton at the summit of the neck." (p. 114.)

Very explicit and useful information for "the public" here, and no less elegantly conveyed to the same "public"! But what do our readers think is the mode recommended to the decrepid "public" for the "renovation of the system"? Why just repeated bloodletting!

366. "*Renovating influence of bloodletting.* If what I have said is correct, we have, in the renewal of the stream by bloodletting, which not only removes the bad, but facilitates the entrance of the good and fresh materials, (absorption both of air and of nutriment being increased *in proportion as the vessels are emptied,*) [!!] a most valuable agent of renovation of the system, and one which, if judiciously employed, bids fair, I am of opinion, to relieve, if not to restore to perfect health, a large proportion of the decrepid and incurable which are everywhere to be found, but it will be argued, &c." (p. 257.)

Now we feel certain that Dr. Searle would himself condemn such doctrines as this if propounded by another writer; or if old age and decrepitude should overtake him, it is not probable that he would submit to the treatment which he advises for others, and be daily bled.

It is always to us a painful duty to write in disparagement of any work which we believe to be the production of an honest, and well-meaning, and respectable practitioner, as we believe Dr. Searle to be, and we cannot but express our regret that he has published his views in their present form.

ART. III.—*The Potato Plant, its Uses and Properties : together with the Cause of the Present Malady. The Extension of that Disease to other Plants, the Question of Famine arising therefrom, and the best Means of averting that Calamity.* By ALFRED SMEE, F.R.S., Surgeon to the Bank of England, &c.—*London,* 1846. 8vo, pp. 174. With 10 Plates.

OUR author's promises as set forth in this title-page are large enough, and form a striking contrast to his amount of performance. Everybody knows that of the thousand-and-one hypotheses which have been proposed to account for the fearful infliction that has fallen upon the cultivators of the potato, each singly, however specious it appeared at first sight, has been proved to be deficient in that universality of application, which could alone warrant its reception as the *vera causa.* Nothing daunted, however, by the failure of his predecessors, Mr. Smee plunges boldly into the investigation; convinced that a medical man is the one to make the discovery for which vegetable physiologists and agriculturists have vainly sought, and that his own qualifications for the search peculiarly marked him out as the fortunate benefactor of mankind, whose name should be handed down to succeeding generations as long as the potato shall be cultivated for the food of man or beast. "The business of a surgeon," he says, "is essentially locomotive, and his duties are practised over an extensive space. It frequently happens that I have had to traverse London in two or even more directions in a single day, which circumstance has given me abundant opportunities of making my observations in different localities." We should like to see these vast potato-fields which Mr. Smee is so clever in perambulating between the extremities of our smoky metropolis, where we thought that nothing grew but houses and "humans." But his researches have not been confined to London alone. They have extended even as far as Clapton, which (we may inform our country readers) is no less than four miles from the Bank. "During the summer months," he informs us, "I was living at Springfield, Upper Clapton, where I had the advantage of a large garden, wherein were several plots of potatoes, which I was in the habit of observing the first thing in the morning, again on my return from London, and frequently the last thing at night. In the neighbourhood, moreover, were larger potato grounds, where I used to enjoy the air and study the disease in the evening; and it has curiously happened, that I have made my observations on the potato-plant in the same garden in which I considered the experiments for my former work on Electro-Metallurgy;" a coincidence which will doubtless impress the public with additional confidence in the author's results.

Finding a few diseased potato-plants at Clapton, covered with aphides,

Mr. Smee seems to have at once concluded that he had found out the true secret of the disease; which is just as if he had argued from the presence of lice upon the persons of a dozen or two of hospital-patients affected with pneumonia or rheumatism, that they were the cause of the malady. We have carefully searched his treatise for that collection of evidence on the subject which is necessary to establish anything like a satisfactory relation of cause and effect; but we have been utterly disappointed. No locality is referred to as furnishing the aphides, save the potato-grounds at Clapton; no results are adduced of inquiries made in distant parts of the country; no proof is given that aphides placed upon healthy potato-plants, and allowed to propagate there, will produce *the* potato-disease in question. We cannot too strongly express our surprise, that a gentleman of Mr. Smee's scientific attainments should have ventured, after all the experience of former failures in accounting satisfactorily for the disease, to make such confident assertions regarding its cause upon so meagre an amount of evidence; when a very short delay would have been sufficient to enable him to procure such confirmatory testimony as the nature of the case admitted from every quarter of the kingdom, the attention of almost everybody being alive to the subject, and any new theory being caught at with avidity. We are not passing judgment upon the merits of the doctrine itself, for we do not consider ourselves competent to do so. The question is entirely one of evidence; since the *à priori* probability seems to us rather in Mr. Smee's favour than against him. And we understand that *since* the publication of his book, he has collected a large amount of evidence which he ought to have got together previously. But our sole concern is at present with the Treatise itself; and whilst we admit that it contains much valuable and interesting matter bearing upon the question at issue, we must repeat that it has not the slightest claim to be regarded as a justification of its author's assumptions.

ART. IV.—*The Nature and Faculties of the Sympathetic Nerve.* By JOSEPH SWAN.—*London,* 1847. 8vo, pp. 55.

WE have every respect for Mr. Swan as an able and industrious anatomist; but we cannot feel a similar confidence in his physiological deductions. There is a vagueness about his language, when he is speaking of *function,* which forms a striking contrast to the clearness of his descriptions of *structure,* and which frequently (as it seems to us) misleads not only his readers but himself. In the preface to the pamphlet before us, he very clearly points out the difficulties attendant on the study of this division of the nervous system, and indicates the sources whence information may be obtained regarding its offices in the system. And in the second chapter, in which he describes the chief variations in the structure and arrangements of the sympathetic system in different classes of vertebrata, he gives many interesting anatomical particulars, which are valuable as data for physiological reasoning. But at the very commencement of his inquiries into the operations of the sympathetic, he betrays the want of those clear ideas of the relation between the animal and organic functions, on which

alone any correct notion of its influence on the latter can be erected. "The great object of the sympathetic nerve," he says (p. 4), "is to furnish the parts it supplies with an appropriate nervous excitement of such a quality as will ensure their functions without disturbing any other portion of the nervous system." Now the whole gist of the matter lies in the simple phrase "ensure their functions." Every one knows that the sympathetic system is distributed chiefly to the heart and sanguiferous system, and to the intestinal canal. Does Mr. Swan assert that the heart cannot beat,—that the arteries, capillaries, and veins cannot convey blood,—that the several parts which they supply cannot draw from the blood the materials of their growth and nutrition,—and that the peristaltic movements of the intestinal tube cannot continue,—without the constant influence of this system of nerves? Such would appear to be his meaning, from various expressions scattered through the pamphlet, though he nowhere (that we have discovered) formally states these views. The following passage, which succeeds the one we have just quoted, will give a fair specimen of Mr. Swan's style; and we think that our readers' judgment upon it will correspond with our own:

"It connects in different degrees all the parts of the nervous system as an harmonious whole, but brings them in so slight a degree in communion with the sensory, as to allow only a perceptibility that can appreciate and respond to impulses without permitting them to proceed beyond the viscera. By preventing sensation, it becomes favorable to the production of involuntary motion, so that impulses on the lining membrane of the viscera, when sufficiently strong, are responded to, and the contraction of the muscular coat takes place. For these purposes it has a peculiar conformation which differs more or less from [that of] the other parts of the nervous system. Although it communicates with several cerebral nerves, it assimilates most to the fifth and spinal nerves. From these and their centres it probably derives some of their essential or diffusive influence for fortifying its vital powers, but admits only just as much of their faculties as corresponds with the functions of the parts it supplies. It is not less extensive in any of the four superior classes of animals, in proportion to the parts it actuates, but its structure is more or less complex, and in the same degree its faculties are more distant from, or approach nearer to, those of the rest of the nervous system, and accordingly are more or less independent. When its faculties are insufficient for the organs, having more complicated functions than those it generally promotes, branches of the fifth, the par vagum, and spinal nerves, are combined with portions of it; or when any organs, supplied by cerebral and spinal nerves, require a more general and higher excitement from the heart and arteries, they receive more branches from the sympathetic." (pp. 4, 5.)

We might easily show the vague and unsubstantial character of every one of the statements contained in this paragraph. What clear notion can be attached to "a perceptibility that can *appreciate* and respond to impulses," if no consciousness of those impulses be excited? What proof exists, that any such reflex action is performed by the ganglia of the sympathetic system? What definite meaning can be drawn from the "essential or diffusive influence for fortifying its vital powers," supposed to be derived by the sympathetic from the fifth cerebral and the spinal nerves? What ground is there for the assumption that the "faculties" of the sympathetic are "insufficient for the organs" it supplies, save the fact that other nerves are transmitted to them?—or what indication exists of the "more general and higher excitement from the heart and arteries" asserted

by Mr. Swan to be required by some organs, save the larger number of branches of the sympathetic proceeding to them? A clearer case of reasoning in a circle never came under our notice. We might quote an abundance of passages of the same kind; and upon the whole we feel constrained to say, that, except the few anatomical details contained in the second chapter, the pamphlet contains nothing but a series of vague and unmeaning speculations, couched in language which is a great deal too positive.

ART. V.—*Elements of Chemistry, including the Actual State and Prevalent Doctrines of the Science.* By the late EDWARD TURNER, M.D. F.R.S. L. & E. Eighth Edition. Edited by Baron LIEBIG, Professor of Chemistry in the University of Giessen, and WILLIAM GREGORY, M.D. F.R.S.E., Professor of Chemistry in the University of Edinburgh. Part I. *Inorganic Chemistry.—London,* 1847, 8vo. pp. 676.

THAT this work should so completely hold its ground, in spite of the competition of such Treatises as those of Professors Graham and Kane, to say nothing of the excellent Manuals of Fownes and Gregory, is of itself a sufficient testimony of the excellence of its original plan, and of the completeness with which it has been kept by its present editors, *au courant* with the rapid progress of Chemical Science. It is scarcely requisite, therefore, that we should do more than notice the appearance of the first part of a new edition; in the preparation of which Professor Gregory has obviously been at great pains to embody every new discovery of sufficient note, relative to the departments which it includes. The remaining portion of the work, embracing organic chemistry, is announced as speedily forthcoming.

The following extract from the Preface will be interesting to many of our readers, as affording a marked proof of that growing conformity of opinion upon fundamental questions, which is one of the most satisfactory indications of the real progress of science:

"Since the publication of the last edition, many continental chemists, including Liebig, Wöhler, Gmelin, and their numerous pupils, have finally adopted the British equivalents or atomic weights for those substances in regard to which a difference existed. This difference, therefore, no longer exists as far as concerns the chemists above named, and many others; who in their works now admit as we do water to be H O, not as formerly H_2 O; and chloride of potassium to be K Cl, and not as formerly K Cl_2, &c. In short, the atomic weights of hydrogen, nitrogen, chlorine, bromine, iodine, and fluorine are now, by those chemists, assumed to correspond with their equivalents, where formerly the equivalent was made to contain two atoms." (p. iii.)

On the other hand, the editors have abandoned the old mode of regarding the combining equivalents of phosphorus, arsenic, and antimony, as twice their atomic proportion; and have thus substituted the simple formulæ PO_5, As O_3, and Sb O_3 for the more complex T_2 O_5, As_2 O_3, and Sb_2 O_3, hitherto employed.

PART THIRD.

CONTRIBUTIONS TOWARDS THE ADVANCEMENT

OF THE

Natural History and Treatment of Diseases.

II. A SKETCH OF THE NATURAL CURE OF DISEASES.

BY WILLIAM MACKENZIE, M.D.

[The following paper was long since published in the Glasgow Medical Journal (No. v, Feb. 1829) of which the accomplished writer was then Editor. It bears so appropriately on the general subject to which this part of our journal is for the present devoted, that we make no apology for reprinting it. We should be under still greater obligation to the author, if he would favour us with new and still more extensive illustrations of the important practical principles advocated by him.]

As the greater number of diseases which attack the human frame are susceptible of cure by the operations of Nature alone, while no one disease can be cured by the powers of Art alone, and as in all our attempts to cure diseases by artificial means, we imitate, or ought to imitate, the modes of cure followed by Nature, there is perhaps no question more truly of importance to medical practitioners than this :—What are the natural processes by which diseases are removed?

Before attempting to answer this question, it may not be improper to premise, that in speaking of the restorative power, or powers of Nature, and of the natural cure of diseases, I have no intention to admit the existence of any intelligent power or powers resident in the body, superintending or operating in the cure. The *vis conservatrix* and *vis medicatrix Naturæ* have sometimes been spoken of in such a manner as might lead us to suppose that this was actually meant—that it was not merely acknowledged that the body has in itself a power or condition, depending on its structure and on the revolutions of its functions, by which, in many cases, it resists the injuries which threaten it, and on many occasions corrects or removes the disorders induced in it, but that this preserving and remedying power was an actual agent superadded to the constitution of the body.

It will at once be seen that I refer in these remarks to the opinions of the celebrated Stahl, who explicitly founded his system on the supposition, that the power of Nature, so much talked of, did not in any degree depend on the structure and functions of the body, but belonged entirely to what he styled the *Rational Soul.* He maintained that, on many occasions, the soul acts even independently of the state of the body; and that without any physical necessity arising from that state, but purely in consequence of its intelligence, the soul

perceiving the tendency of noxious powers threatening, or of disorders anywise arising in the system, immediately excites such actions in the body as are suited to obviate the pernicious consequences which might otherwise take place. This is a very fanciful, and I believe an entirely groundless hypothesis; but there is so much appearance of intelligence and design in the operations of the animal economy, that many medical authors have very much countenanced the same opinion. It may suffice at present to observe upon these notions, that the admitting of any such intelligent governor of the animal economy, would oblige us to reject all anatomical and physiological reasoning concerning diseases, and would render the whole practice of medicine capricious, and even dangerous. We see, in fact, the preposterous effects of such a system in the practice of Stahl himself and his followers, who, trusting much to the constant attention and wisdom of nature, proposed what they called the art of curing diseases *with expectation*, used, therefore, only very inert and frivolous remedies, were extremely reserved in the use of such general and powerful means of cure, as bloodletting, vomiting, and the like, zealously opposed the use of some of the most efficacious medicines, such as opium and Peruvian bark, and, in fact, converted the healing art, as far as they could, into a mere curious contemplation of diseases and of death.

I go on, then, in answer to the question already stated, to remark in the first place, that in some instances the natural cure of diseases is so direct and prompt, that we are unable to discover any process by which it is effected. Pain ceases to be felt—and the disease is at an end. Spasm relaxes—and the muscle returns under the control of volition. In epilepsy, we see the return to health take place almost as suddenly as was the attack of the disease;—in the midst of the most violent symptoms, that calm comes on which announces the termination of the attack. The heart interrupts or relaxes its wonted action, and as the common name of the disease denotes (ςυν κοπτω,) the patient is struck down, blind, pale, and cold;—the heart resumes its labour, and animation is restored. In cases such as these, the cure is so simple, the steps of recovery so few and short, that we may be allowed to regard them as instances of the mere cessation of disease

In the second place, some diseases undergo a natural cure by means of the *Revolutions of the Functions*. As an example of this, we may take the disease of intoxication. When a person has swallowed a large quantity of ardent spirits, there follow all the symptoms produced by a narcotic poison. For a time the force of the circulation is increased, but this is soon followed by languor, delirium, and stupor, attended by nausea, vomiting, and headache. A tendency to apoplexy is produced, and a temporary want of power over some of the voluntary, and occasionally in some of the involuntary movements of the body. In consequence of this palsy extending to the muscles of respiration, the disease of intoxication sometimes proves mortal; but in general a cure is effected by the natural revolutions of the functions of the body. We are unable to trace the processes completely by which this disease is produced and afterwards cured, but part of them is sufficiently known. It is known that the ardent spirits are carried into the circulation, for alcohol has been obtained by distillation from the blood of animals to whom it had been administered. It is also known, that mixed with the blood, it first of all excites the heart to more frequent action; and either directly, in the blood circulating through the brain, or indirectly, by operating on the nerves of the stomach, excites the brain to inordinate action, and to all the false fire and unmeaning fury of the drunkard. This is followed by congestion of the blood passing through the vessels of the brain, and this is succeeded by temporary palsy and insensibility. The method in which the body gets free of the poison is partly by its being conveyed in the blood to the kidneys, by which organs it is separated along with the urine, and thus excreted, and partly by being conveyed to the per-

spiring organs, namely, the skin and the lining membrane of the lungs, so that mixed with their secretions, the sweat and the vapour of the breath, it is thus let free from the blood, and consequently from the body, which it had so much deranged.

This is the method in which Nature relieves herself of this disease, and of the poison by which it was occasioned; and it is probable that in this very way she relieves herself of other poisons, and of still more formidable and tedious diseases. We are unable, it is true, to detect the poison of fever; but we have scarcely a doubt of its existence, of its absorption by the lining membrane of the lungs, of its circulation through the body in the blood, exciting languor, weakness, pain, rapid motion of the heart, disorder of the brain, oppression of all the functions, and frequently death, while in favorable cases, we observe, that by augmented secretion of urine, or increased flow of perspiration, the disease is terminated, and the poison probably expelled from the system. It is this natural cure of fever which the medical attendant hails with so much gladness. It is for this he watches—and returning again and again to the bedside of his patient, feels the skin, and looks at the eye, and examines the tongue. If he discovers the least kindly moisture exuding from the surface of the body, he hails it as the signal of life, and welcomes it with a heart as happy as that with which the alchemist would have welcomed a drop of gold which he had long anxiously looked for, and almost despaired to see.

And in the cure of fever we but imitate this natural cure. We adopt a cautious and symptomatic treatment, and endeavour rather to assist the slow proceedings by which Nature frees herself of the disease, than to force it to retire. In the treatment of fever, no doubt, some have attempted to set up new actions in the constitution, by which to suspend, or even wholly to remove, the disease. But when we find that in almost every case of this disease, the vital powers are of themselves sufficient to effect the most perfect recovery, ought we not rather to adopt the indirect method of cure, watching the curative operations of Nature, supporting and aiding her in these operations, setting aside impediments, that the vital powers may exert their beneficial influence in a free and undisturbed manner, removing or alleviating dangerous individual symptoms, until the disease be surmounted, and the curative process completely and happily terminated? I have had opportunities of witnessing both plans of treating fever—the direct, in which an attempt is made by active treatment to dispel the disease—and the indirect, in which the natural cure is imitated—and I have no hesitation in saying, that though the former might serve more to flatter the self-complacency of the practitioner, the latter was by far the more successful.

The body is almost altogether fluid. Nine-tenths of its weight are so, and only one-tenth solid. The fluid parts are in a perpetual state of change, being decomposed by one set of functions, and recomposed by another; and although it may be doubted how far these changes extend to the solid fibres or parenchyma of the body, it is unquestionable that our fluids, by means of digestion, absorption, circulation, respiration, and secretion, are in constant revolution. By these processes there is effected an uninterrupted decay and restoration of the body; and we cannot doubt, that the natural cure of diseases depends very much on the existence and on the perfection of this revolution. Nay, it is extremely probable that one of the principal intentions served in this mode of carrying on life, is the prevention and removal of diseases. It is also chiefly by substances entering into the course of this same system of circulations and of changes, going through the absorbents into the blood, with the blood passing through all parts of the body, and coming at last to the kidney and to the skin, that we are able to imitate those cures of diseases which are the most perfectly effected by the restorative powers of Nature.

In the third place, the natural cure of diseases appears, in some instances, to be governed by *Revolutions of Time.*

That some diseases are periodic—that they come, and go, and return, and are again removed, at regular intervals of time, admits of no manner of doubt. It is also indubitable that the healthy actions of the body are, in a certain degree, associated with periodic revolutions. The periods of sleep and of menstruation are striking examples. The ordinary repletions of the body, and the ordinary evacuations are also periodic. The instincts of man, though less under the control of seasons than those of the inferior animals, are called only into periodic activity. Even the noblest powers of the mind, in the noblest of the species, as in Milton, are subject to periodical returns of exhaustion and of force. This periodical tendency necessarily attends the morbid actions as well as the healthy actions of man. Rheumatism, fevers of different kinds, lunacy, gout, and several other diseases, manifest their periods of return and their periods of departure in the most unquestionable manner. Whether or not these phenomena of health and of disease actually arise from the diurnal, lunar, and annual mutations of the earth, and its satellite, affecting the living body, I shall not pretend to decide.

In ancient times, the heavenly bodies, and especially the sun and moon, were supposed to influence all diseases, but particularly mania and epilepsy. When the Newtonian doctrine of universal attraction was first promulgated, Dr. Mead revived and supported the ancient doctrine with great learning and ingenuity. It was regarded merely as an ingenious conjecture and possible fact, till Dr. Darwin, by interweaving it with his peculiar doctrines, once more endeavoured to give it an air of serious importance. Dr. Balfour brought it forward as capable of direct proof. His opinion is, that the influence of the sun and moon, when in a state of conjunction, which is named sol-lunar influence, produces paroxysms in continued fever, in all cases in which a paroxysmal diathesis exists; and as this influence declines, in consequence of the gradual separation of these luminaries from each other, and their getting into a state of opposition, a way is left open to the system for a critical and beneficial change. Dr. Stoker put Dr. Balfour's doctrines to the test of 276 patients, between July 6th and September 6th, 1817, in Dublin. He has given us his tables, and observes, that "very little coincidence indeed is to be remarked from a view of these tables." *

That there is nothing impossible in sol-lunar influence—that the attraction of the sun and moon, which raises the ocean into mountains, may also affect the nice sensibilities of animal bodies—and that medical meteorology, more carefully studied, may yet come to unfold the origin, and trace the apparently capricious courses of many diseases, we may readily admit. Indeed this science, including, as it does, the effects of changes in temperature and humidity, as well as of seasons, on the human constitution, must be regarded as yet in its infancy, and as affording more scope for original observations and inquiries than any other branch of medicine.

Even, however, in the present state of our knowledge, the periodic departure of some diseases is a fact of high importance to the medical practitioner. It not merely enables him to pronounce a more accurate prognosis than if he were ignorant of the fact; but he seizes with advantage the period of intermission for the exhibition of the remedies which are found to operate against the tendency which all periodic diseases have to return.

In the fourth place, as Mr. Hunter has given to interstitial and progressive absorption, the title of the Natural Surgeon, and as there are various other processes of an analogous kind, materially concerned in the cure of diseases by nature, I shall include them all under the name of the processes of Natural

* See Good's Study of Medicine, vol. ii, p. 90. Second edition. London, 1825.

Surgery. They distinctly differ from the processes of health; and are so remarkably useful in the preservation of life, as to have deeply attracted the attention of medical philosophers. The most important of them are—1st, the closure of divided blood-vessels; 2d, adhesion, or union by the first intention; 3d, granulation, and union by the second intention, including the union of fractured bones, and cicatrization; and, 4th, interstitial, progressive, and ulcerative absorption.

The means employed by Nature in stopping hæmorrhagy from wounded blood-vessels, are, the retraction of the vessel, if completely cut across; the contraction of its open extremities; the formation of a coagulum within the vessel; and the injection of the surrounding cellular substance with blood, and the coagulation of that blood. I need not say how minutely these different particulars in the natural cure of hæmorrhagy have been studied; nor how ingeniously and successfully they have been imitated and improved upon, in the artificial means adopted for the closure of wounded blood-vessels. The natural cure of internal hæmorrhagies is accomplished by means analogous to those by which external hæmorrhagies are arrested.

When a bone is fractured, a muscle torn across, a tendon ruptured, or an incision made into any part, steps are immediately taken by Nature for reparation. When the incision is made by a clean cutting instrument, and the sides of the wound are not widely separated, the union is more directly and speedily effected; but even then, the means are essentially the same as in union by what is termed the second intention, or in the healing of fractured bones, torn muscles, or ruptured tendons. A quantity of blood is extravasated into the line of separation between the divided parts, whatever these may be, and into the cellular membrane around the injured parts; when the blood effused is not very considerable in quantity, and the parts are not much injured, the parts which had been divided gradually approach each other. In a few hours, the surfaces of the fracture, laceration, or wound, are covered with a layer of fibrin. If the two divided surfaces have been kept in contact, the fibrin is found adhering to both, and serving as a slight bond of union between them. That this fibrin intervening between the surfaces of a wound, fracture, or laceration, is derived partly from the vessels divided by the injury, can scarcely, I think, be doubted, although its origin has more commonly been supposed to be the capillaries situated close to the divided surfaces. The next step in the process of reparation is the organization of the effused fibrin. It becomes penetrated by arteries, veins, and absorbents, shooting from the vessels of the neighbouring parts. The completion of this part of the process is the completion of adhesion, if the sides of the wound had luckily remained or been brought and kept in contact; but if this had not been the case, the organized fibrin assumes the appearance of red fleshy points or granulations, while a secretion of pus flows from the exposed surface, and the union is completed by gradual approximation and contraction. In the case of fractured bones, the effused fibrin first of all becomes cartilaginous, and then osseous.

That variety of absorption by which a part, or the whole of the body is wasted, has been called by Mr. Hunter interstitial; because it is removing parts of the body out of the interstices of that part which remains, leaving the part still as a perfect whole; although, in some cases, it must be confessed, it is carried on till not a vestige of the part remains. Progressive absorption, again, is that process by which pus, and extraneous bodies of all kinds, are brought to the external surface. Ulcerative absorption may be regarded as a substitute for mortification; and it seems to take the place of this loss of all action, from a degree of vigour superior to that which exists in the parts where mortification takes place. These modes of absorption are often united, or succeed each other. Although the last of them would often appear to be doing mischief, by destroying parts which are of service, yet in all cases we

may refer consecutive absorption to some necessary purpose. We may depend upon it, as Mr. Hunter observes, that those parts have not the power of maintaining their ground, and that it becomes a substitute for mortification.

As for progressive absorption, Nature has not only made what might be called an instinctive provision in the parts to remove themselves, so as to bring extraneous bodies to the skin for their exit, and thereby guarded the deeper seated parts, but has also guarded all passages or outlets, into which we might perhaps suppose, though extraneous bodies were discharged, no great mischief could follow. Thus, an abscess in the cheek, close on the internal membrane of the mouth, and some way from the skin, shall not, as we might perhaps have thought it should, open into the mouth, but shall push outwards, and at last come to point and break externally.

Cicatrization is a process of Natural Surgery, in which there is invariably betrayed a great degree of economy, for we never find the new formed skin so large as the sore was on which it is formed. Indeed, when the looseness of the surrounding skin permits it, almost no new skin is formed, as in the scrotum.

In the fifth place, Nature often cures or prevents one disease by producing another; or, as this process has been termed, by *converting one disease into another;* and this is a plan, which, in the practice of medicine, we frequently endeavour to imitate, exactly as we attempt to imitate also the complete removal of diseases which Nature accomplishes in other cases.

Diarrhœa is a disease, and yet it is a means not unfrequently adopted by Nature for the cure of dropsy; and we often imitate this cure, by giving hydragogues. If a hæmorrhoidal discharge which had continued to flow for a number of years at accustomed periods should cease, the individual is apt to become affected with giddiness, pain in the head, and a threatening of apoplexy; but if the discharge of blood from the rectum returns, these symptoms of impending apoplexy are removed; and if the discharge does not of itself return, we often apply leeches round the anus with the best effects. These are instances of *related diseases,* or of the cure of diseases by *conversion;* the dropsy being converted into a diarrhœa, the apoplexy into a discharge of blood from the rectum.

It would appear that there are two varieties of conversion. The one may be called *sanative*, for it produces health; and the other *insanative,* for though it removes one disease, it does not restore health.

For instance, a patient with headache is seized with spontaneous epistaxis, by this means the natural state of circulation within the head is restored, the headache is removed, the epistaxis ceases, and health is completely recovered. This I should call a sanative conversion.

Again, I once saw a man with diabetes become suddenly affected with dropsy. He had œdema, an evident collection of water in the cavity of the peritoneum, the symptoms of hydrothorax, and even those of hydrocephalus. In fact, I never saw so universal a case of dropsy. As soon as the dropsical symptoms made their appearance, the diabetic symptoms entirely ceased, the urine was reduced to an ordinary, or even less than ordinary quantity, and lost entirely its sweet taste. I should call this an insanative conversion. The change—the cure of the diabetes—was not accompanied by any tendency to health. By means of diuretic medicines, the dropsy was removed, and that instant the diabetes returned. After some time, the diabetes again suddenly stopped, and the dropsy returned. I am not certain but that this morbific conversion was repeated thrice, till at length the man died, his constitution being completely worn out by these alternating diseases.

Dr. Parry, in the first volume of his 'Elements of Pathology and Therapeutics,' has related a number of striking cases illustrative of this subject. Of these I shall select a few.

Headache, depression of spirits, and other nervous affections with which a young lady had been long afflicted, were completely cured by the measles.

In a gentleman labouring under gout, the fit was immediately removed by an accidental catarrh from cold.

In a lady, chronic rheumatism of the right shoulder was immediately and permanently relieved on the appearance of jaundice, with pain, from a gall-stone, which did not pass.

A headache, of some years' duration, subsiding, was followed by a cough, accompanied with incessant and wasting hectic fever. After the man had been long confined to his bed, and death was every day expected, the headache began slightly to return; and as it became established, the cough and fever receded, and the patient regained his flesh, but continued subject to headache as before.

In a lady, various nervous symptoms disappeared on the commencement and gradual increase of a vascular fulness of one mamma. The progress of this disease was suspended in its turn by rheumatism in the hip; and again, the rheumatism disappearing, the affection of the mamma rapidly increased.

In a gentleman, long accustomed to violent vertigo and pain in the head, these affections were constantly relieved on the coming on of œdematous swellings in the legs and feet.

In a lady, mania, which ended in suicide, alternated with œdematous swelling of the ankles.

In a gentleman, epileptic fits, which used to occur at least once a-week, were suspended for three weeks by pneumonia; but returned with additional frequency after the pneumonia had ceased.

Bronchocele, in a lady, gradually disappeared during the progress of a fatal inflammation of the liver.

In a young man, long continued cough, accompanied with fever, night-sweats, and emaciation, ceased on the spontaneous occurrence of inflammation and ulceration under the scapula.

From having noticed, agreeably with the observations of physicians and of the vulgar in all ages, that the occurrence of certain diseases is preventive of some and curative of others, one author, Sir George Smith Gibbes, of Bath, has attempted to generalize this propensity in nature. He has concluded, as disease sometimes appears to be curative of disease, that one disease is always necessary to the cure of another, that just as many functions undergo a secondary derangement as are necessary for the cure of the primary one, and that no diseases occur but such as are curative in their effects or in their tendency.

The work in which this doctrine is maintained I have not had an opportunity of perusing; but an abstract and refutation of Sir G. S. Gibbes' opinions are given by Dr. Pring in his 'Exposition of the Principles of Pathology.' Notwithstanding his general condemnation of the doctrine, Dr. Pring allows that some good has arisen from this attempt at systematizing; observing that men, from attachment to their systems, became zealously attentive to everything which can reflect credit on their opinions, and not unfrequently, by showing phenomena in a new or more conspicuous light, serve the cause of science, although they fail in their immediate object. Sir G. S. Gibbes is fully entitled to the benefit of this remark. From observing particularly the curative tendency of diseases, he has indicated the propriety of not resisting secondary diseases in many instances, and of imitating them, when they tend to be curative, in many others.

Such, then, is a very short and imperfect sketch of the modes or processes by which Nature effects the removal of diseases. I have touched upon each, more for the purpose of recalling those interesting facts to the recollection of my readers, than with the hope of laying before them a single idea which can be new to them. I may perhaps be permitted to say, however, that I appre-

hend the natural cure of diseases to be a matter too little studied by us all, and especially too much neglected by those whose business it is to introduce beginners to the profession of the healing art. In fact, too much attention is bestowed, in the first instance, on what Art, and too little on what Nature can do; whereas, the "*quid natura faciat, aut ferat,*" ought first to be well understood, and then the attention turned to the subsidiary contrivances of medicine. As our profession is now studied, interference—perpetual and busy interference —is too much encouraged; observation and caution too much neglected; nor is it sufficiently impressed on the minds of those who mean to practise medicine, that the whole of the Healing Art is but an imitation of the different modes of Natural Cure, and that in general our cures are cures by conversion,—in other words, that we alleviate or remove one disease, in most instances, only by means of another which we excite, and very rarely effect anything like direct relief. I know that this view of the Healing Art is not a flattering one; but there can be no doubt that it is a true, and might be, a useful one. A rational Theory of Medicine can be built only on the Natural Cure of Diseases.

Glasgow, 24th September, 1828.

II. ON THE OBSERVATION OF NATURE IN THE TREATMENT OF DISEASE.

BY ANDREW COMBE, M.D.

One of the Physicians in Ordinary in Scotland to the Queen.

(*In a Letter to John Forbes, M.D., F.R.S.*)

Edinburgh, Feb. 26, 1847.

My dear Sir,—Soon after the publication of your last Number, a friend put into my hands Hufeland's 'Enchiridion Medicum,' published in Berlin, in 1836, but which I had not previously seen. It is a practical work, and embodies the results of fifty years' extensive experience as a physician. The opening chapter, entitled, "Nature and Art," embodies views so much in accordance with those recently advocated in your Review, that the following translation of it may prove not uninteresting to your readers.

"'*Natura sanat, medicus curat morbos.*'

"All healing of disease is effected by Nature, art is only her assistant and cures through her.

"In the same way as an internal morbid condition of organic life—an internal process of disease—lies at the foundation of and produces the external appearances (symptoms) of every disease, even so also an internal curative process—an exertion of the organic life for changing and converting the abnormal into the normal condition—lies at the root of every curative act, and alone renders it possible.

"This holds in the case of all diseases without exception. In visible or so-called surgical diseases nobody calls it in question. Every surgeon admits that it is not he who heals a broken bone, a wound or a bruise, but that it is the vital power of Nature which especially effects the result through her wonderful processes of exudation, agglutination, suppuration, separation of the dead parts from the living, and regeneration, and that his business is only to take care that these operations be allowed to go on regularly, and all obstacles to the fulfilment of her design be removed. But precisely the same rule holds with the internal diseases, the immediate relations of which are hidden from our observation, only with this difference, that we cannot see these healing-processes and organic changes with our eyes. This is true not

only as to acute diseases, viz., those attended with a considerable degree of excitement, but also as to chronic, only more slowly and in a less striking manner. In slight diseases we see daily that recovery ensues without any help from art. The same thing happens also in severe and even in the worst cases. There is no disease, from the most acute inflammatory fever to the most deadly plague—no disease of either suppression or excess in the secretions and excretions, no dynamic or other affection, which has not already been cured by Nature alone. In what way does art contribute to recovery? We bleed in inflammation, lower the tone of the system, and believe that we have thereby cured. But we have only removed the obstacles and hindrances, the superfluity of blood and of excitement, and thereby placed Nature in the condition required to complete the internal healing process which must always go on if our cure is to be effected. We support the strength of the system in adynamic and low nervous diseases, and believe thereby to make a cure; but in reality we only excite the *vis medicatrix Naturæ* sufficiently to enable her to complete the internal restorative processes, which are necessary to recovery. Even the direct cure of diseases by means of what are called specifics, is the work of Nature, as the remedy only operates antagonistically, but the reaction and change for the better thus excited, take place only through the co-operation of the internal healing power of Nature. In the *dyscrasien* also, even where a specific poison is received into the system, the healing-power of Nature may effect a recovery. Is it necessary to recall the thousands of cases in which venereal affections have been recovered from without any medicine, and even as at present with the studied avoidance of mercury? But also in the deepest seated or most severe forms of constitutional syphilis, what can mercury accomplish without the co-operation of that internal healing power, which effects the expulsion of both the poison and the remedy, and restores that normal condition of the organization which is necessary for the production of healthy secretion and nutrition? How often do we find that the use of mercury in every form is of no avail, till by the combined use of strong nourishment and tonics we have restored the energy of the vital powers to the degree required for the due operation of the internal healing-process, and even of the mercury itself!

"This internal healing-power often shows itself in those remarkable changes, crises and metastasis, which arise most unexpectedly and surprisingly from it alone, and which often at once put a stop to or modify a long and severe malady which has resisted all the efforts of art. The patient whom in the evening we believed destined to certain death, falls during the night into a profuse perspiration, and is found in the morning entirely out of danger. In a severe acute disease, against which we have in vain exhausted all our resources, an external abscess suddenly shows itself, and the disease vanishes. Yes, what crowns the *vis medicatrix Naturæ*, is her frequent victory over the most different, contradictory, and unreasonable methods of cure. Do we not see daily in the country, men recover either without assistance from art, or with only the most senseless treatment? Even under professional treatment, I have long been convinced that most cures are effected, under the advice of the physician indeed, but in a very small proportion in consequence of that advice.

"This then is the meaning of the great word *crisis*, which reaches us from hoary antiquity with so elevated and so mysterious a meaning. It is not the critical evacuations, not the consequent external change, but the internal healing-process, the internal ripening of the disease, the operation of the internal assimilating, secreting, metamorphosing and renovating power of Nature which lie at the foundation of all the external phenomena. It is this which the word expresses, and it is this which is the meaning attached to it when used by all true-to-nature, profound, and unprejudiced physicians, from Hippocrates down to Sydenham, Hofmann, and Boerhaave.

"A system of medicine which embraces Nature in this sense; which, in all

that it does, recognises and respects the higher laws of life and the self-acting power of Nature; which considers itself not as the *agent* but as the *instrument* of the internal healing power; which recognises the indications for interference, and decides on acting only from the wants and claims of diseased Nature; which looks upon everything that takes place in the system, whether disease, or the results of its own curative process, or the operation of medicines, as vital actions;—in one word, which itself lives in life, and which, as it recognises every thing that lives to be elevated by life to a higher sphere of existence, confines its operation accordingly to this sphere, and so becomes one with the *vis medicatrix*;—such a system of medicine I call *physiatrik.* The meaning generally attached to this term is equivalent to the *vis medicatrix Naturæ*, but I understand it to mean *a system of medicine founded on this vis medicatrix.* This is the only true system of medicine, and is founded on the eternal laws of Nature. It is it which, from the time of Hippocrates downwards, has ever been the ideal (object) of the true *Iatricists* (?), and which, through every change of the systems of the schools, has existed in the mind of the genuine practitioner. It is it whose disciple I confess myself, and it is it to which I have always belonged.

"Hence then may be discovered the true meaning of art, its relation to nature and to the position of the physician. As certainly as the *vis medicatrix Naturæ* is the foundation of every cure, (which indeed could not be accomplished without her) so certainly is her operation lightened by art, through which alone she is sometimes rendered capable of effecting a cure. Herein lies the value and the necessity of art. The stricter conclusions from it are as follows:—

"1. Art can sometimes entirely remove the disease and render the internal action unnecessary merely by removing the exciting cause, *e. g.* by the removal of a foreign body, a poison, or any gastric accumulation which excites disease.

"2. The *vis naturæ* is occasionally so exalted, and its operation so stormy and active, that it may either exhaust itself or cause injury to some important organ. In such cases art may reduce the excitement to the degree required for the production of a perfect crisis, and for warding off dangerous accidents.

"3. Nature may on the other hand want sufficient strength to fulfil completely the internal healing process. In such cases art steps in, and by proper strengthening means, supplies and sustains the tone required, and so enables the internal sanative process to go on.

"4. Art may remove obstacles which either prevent or obstruct Nature from carrying on the internal healing process. Under this head may be arranged the important condition of a suitable diet and regimen, the quiet and repose necessary during febrile excitement; the avoiding of vitiated air, bad food, &c. &c.

"5. Art may assist Nature in her struggle against particular conditions of disease, with remedies peculiarly applicable to these conditions.

"6. Art may support Nature during a crisis which she has commenced, and enable her to complete it.

"7. Lastly: there are morbid causes and conditions unconquerable by Nature alone, *e. g.* the syphilitic poison and mechanical injuries. Here art alone is of service either by the use of remedies to counteract the effects of the poison, or by mechanical and surgical help.

"Such is the object of the art of healing, and such are its limits. The physician should not be the *magister* but the *minister Naturæ*, her servant or rather her assistant, ally, and friend Hand-in-hand he should march on with her to the fulfilment of the great work, never forgetting that it is not he but she who performs it, respecting her, having her ever before his eyes, and disturbing her as little as possible.

"There are two errors which arise from this, and against which the physician must be always on his guard.

"The first is, DOING TOO LITTLE, negative treatment, or leaving *all* to Nature. This is a fault into which the new homœopathic school in particular often falls, and which may have the most melancholy consequences where active and positive treatment is really requisite to save the patient. It is proper in itself only where no rational indication for treatment can be discovered; where only time and patience are required for a cure; or where Nature is quite able for the whole crisis as it proceeds through its course in certain definite stages, as in measles or benignant smallpox, &c.

"The second error is, DOING TOO MUCH, or the using of bloodletting and other active measures to such an extent as to damage the system more than the disease itself would have done."

Such, then, are the practical conclusions deduced by Hufeland from his long and varied experience, and which, after making allowance for the transcendentalism which obscures the meaning of two or three passages, seem to me to deserve the serious attention of your readers. Their general coincidence with the opinions lately advocated in your own pages is so striking, as to afford an additional presumption in favour of their truth; while, on the other hand, the objections urged with so little effect against you, seem to me equally invalid as applied to Hufeland. It would, for instance, be very difficult for any unprejudiced and reflecting person to read his summary with care, and still believe that his views lead to a *do-nothing* system of treatment. That Hufeland's principles are opposed to the *abuse* of active measures is indeed sufficiently manifest. But they are so far from condemning activity in a right direction and when really called for, that their proper application demands the constant exercise of greater watchfulness and discrimination in both observation and treatment, than are commonly met with. In like manner, it would be difficult for any candid and reflecting person to object to him, as has been done to me, that a treatment grounded on the indications of Nature must be erroneous, because if we never "thwart Nature," but, on the contrary, "always assist her efforts," we shall thereby be the means of hastening the death of the patient in those numerous cases in which "her efforts tend only to *destroy* life." This objection, indeed, is even more futile than the other, because it rests on a mere perversion of the proposition it is supposed to refute. It would be, not science, but downright folly in any one to contend seriously that we ought in any circumstances to *assist Nature in her destructive efforts;* and it is surprising that mere common sense should not have sufficed to prevent such a meaning being gravely ascribed to a few isolated sentences in your Review, even if their true meaning had not been rendered apparent by the context. Hufeland's statement, with which I concur, is, that in every disease there is an effort made by Nature *for the preservation of life and the restoration of health,*—an internal healing process,—without which recovery would be impossible, and which, as it takes place in accordance with fixed general laws, will be promoted or counteracted in proportion as the treatment resorted to shall, or shall not be in accordance with these laws, and with the *sanative* efforts of Nature. That such sanative efforts *are* made in every case is proved, not only by hourly experience, but by the recoveries occasionally effected by their unaided means, even in the deadliest epidemics of plague and cholera. Keeping in mind, then, that it is the sanative and not the destructive effort which is spoken of in the passages objected to, it still remains as consonant as ever to the soundest dictates of reason and experience, that the primary aim of the physician should be, by careful observation, to make himself acquainted with

the plan and course of Nature, that he may be thereby enabled to co-operate with her and promote her efforts to effect a recovery.

As remarked in a former letter, the field of controversy would be greatly narrowed and the chances of agreement increased, if the objectors would consent to attach the same meaning to our propositions which we intend them to convey; and, doubtless, the discussions which have taken place, will have some effect in bringing about this most desirable end. But the differences between us are not altogether verbal or insignificant; they involve an important practical principle calculated to exert no small influence on the future progress of the healing art. But even if I were able, which I am not, to do full justice to the subject, it is not within the limits of a Review that it can be discussed with the requisite clearness and precision.

I remain, &c.,

ANDREW COMBE.

III. ON THE POWERS OF NATURE IN THE CURE OF DISEASES. IN REPLY TO DR. COMBE.

BY J. A. SYMONDS, M.D. ETC.

(*In a Letter to John Forbes, M.D., F.R.S.*)

Bristol, March 1, 1847.

MY DEAR SIR,—I am glad that Dr. Combe has honoured my letter with his comments, because they give me an opportunity of correcting a misapprehension into which he has fallen, and at the same time of enforcing some opinions which I have already expressed. In the remarks I am about to make I hope to emulate the good spirit in which I believe Dr. Combe's letter to have been written, although I shall not hesitate to allow myself an almost equal latitude of criticism.

Dr. Combe is in error when he supposes that my letter to you was intended to be a reply to his communication. Had he read it carefully and with a distinct remembrance of your Essay, he would have seen that my remarks were directed against such injurious inferences as seemed likely to be drawn from your paper. For after expressing my satisfaction that you had yourself made a statement corrective of the misunderstanding occasioned by your first paper, I alluded respectfully and in general terms to Dr. Combe's and certain anonymous epistles, which appeared to me "to set rather too much in the direction of the expectant method." All that I said bearing specifically upon Dr. Combe was at the close of my letter, and had reference to his suggestion of an experimental investigation of the claims of homœopathy. And as I shall have no better opportunity in the present letter, I must in this place observe that he has rather evaded than answered my question: "What is to induce us to administer a scruple of jalap for a looseness, and a grain or two of opium for a lethargy?" If there be truth in the similia-similibus doctrine, opium which causes a lethargy ought to cure one. To me it seems trifling with the subject to say, "See whether the medicine which cures a disease will produce it in a sound person." This was not the method of the homœopathists. If so, what an immense amount of chance experiments must have been made! to say nothing of their superfluousness. For if the drugs had really cured the various diseases in the homœopathic nosology, it would be useless to try their morbific qualities in order to establish their healing virtues. The line really pursued was that intimated in my question, which question I was justified in putting to any one who recommended us to test the like-by-like doctrine.

Dr. Combe endeavours to elude the difficulty by saying that he proposed a method just the reverse; forgetting that the fallacies would be much greater. That a particular medicine cures a particular disease may be true of quinine and ague, and of one or two others, but our trials would be few indeed if reduced to specifics. We do, however, know the physiological action of medicines. If opium narcotizes it ought to cure a coma. Dr. Combe himself says, "having tried their action in health, try the same remedies *in the usual doses* in the treatment of disease, with as much care and discrimination as possible, and record the results." Now the action of opium has been tried over and over again in health, and its effects are so similar to those of disease, that a case every now and then occurs in which it is very difficult to distinguish between them. Are you then prepared to give this drug, with never so much care and discrimination, in your next case of simple apoplexy? But I will not press this point further; I am sure Dr. Combe is too good a practitioner to let his catholicity towards the homœopathists induce him to try dangerous experiments by the bed-side.

To return to his critique. He says, "it seems to me a wise precaution, before proceeding to refute a supposed antagonist by either argument or ridicule, to ascertain very clearly whether a difference exists, and if so, in what it really consists." (p. 257). Most wise indeed! but, alas! for human consistency. The nearer the church the farther from God! For in the very next sentence he declares that I "condemned as wholly inert and absurd plans of treatment conducted in accordance with Nature." I have sought in vain throughout my brief letter for anything approaching to a warrant or excuse or even colour for this sweeping charge. So that I must infer that Dr. Combe so fully relied on the general truth of his maxim, and was so conscious of his habitual attention to it, that unfortunately for me in this particular instance he did not think it worth while to put it in practice; or perhaps he thought "exceptio probat regulam." To condemn treatment in accordance with Nature, would be to condemn what one has been learning and teaching all the days of one's medical life. For in what does it consist? *Generally*, in being founded on what is known of the structure and functions of the human body, and *particularly* in the encouragement of the natural tendencies to reparation. Who has not been taught this rudimentary information? Surely we need no ghost to arise and tell us this. I am happy to say that it does not fall to my lot to meet with practitioners so grossly ignorant, that if a bronchitis were relieving itself by free expectoration they would try to check the secretion, —or if intestinal hemorrhage ensued on congestion of the liver, would administer astringents; while, on the other hand, were Nature allowing a delicate elderly patient to be worn down with bronchorrhea dependent on a relaxed state of the mucous membrane, they would not hesitate to give him sulphate of zinc, and if the intestinal bleeding proceeded from a follicular ulcer they would take care that sugar of lead or oil of turpentine should be applied to the bleeding surface as fast as possible, many an unhappy patient having died of this natural method of relief.

Dr. Combe avers that I seldom make "any special reference to the standard of Nature." I beg his pardon; I repeatedly referred to Nature, but not merely to *reparative* Nature, which seems to be the only nature in Dr. Combe's kingdom; I took some pains to direct attention to *morbific* Nature. He intimates that I am not "familiar with the study of Nature as an *aim*;" by which I presume he means as an *end*. Certainly I have studied it chiefly as a *mean* to the cure, and relief, and prevention of diseases. But it is hard to conjecture what exactly Dr. Combe intends by advocating so emphatically the observation of Nature in the study and treatment of disease. Surely, it is rather late in the day for a clear-headed, well-informed writer like Dr. Combe to preach "Homo naturæ minister et interpres." Is it not as if some professor

should go to Oxford, and tell the men who are construing Juvenal and Æschylus, that he hopes they have learned their accidence? Study Nature! Talk prose! Have we not been doing it all our lives? What were those curious inquiries into the roots and juices of plants in our early days—those porings over muscles and nerves, vessels and viscera, in vaulted cellars,—those hospital walkings and dispensary trudgings,—those clinical labours, when, from the initial rigor to the terminal rhonchus, every phase of the case was carefully observed and faithfully depicted in those long, ledger-like note-books, and then the work of Nature further studied in the mortuary, with all aids and appliances from scalpel, scales, and measure;—and, since then, the night-watchings many by the bedside, and the reflections of sleepless hours on the day's observations,—and the endless interrogations of Nature, if haply one might attain to such knowledge of her doings, that here one might aid them, and there control or repress them? Or, to put individual experience aside, what have been the labours of the Baillies, the Laennecs, the Andrals, the Louis, the Brights, the Carswells, the Hasses, and Rokitanskis, and Vogels, and a host of recent histologists?—painful searchers all into the dark mysteries of Nature! Physiology is good, but it is not the one thing needful. There is pathology also—there is a dark as well as a bright side of Nature. It has pleased Infinite Power and Goodness to mix up evil with good in this world, and to place man in it with instinctive and other instructions, to do all he can towards extirpating or abating the one, and fostering and increasing the other, and to hope for a better state of being in which the sad enigma will be unriddled, and partial ill be resolved into universal good. Study Nature! yes—but let it be no narrow, one-sided study! Measure the *vis vitiatrix* as well as the *vis medicatrix!* It is better to be on our legs facing evil, defying it, and wrestling with it, than to sit simpering in the easy chair of optimism and crying "Peace, peace—when there is no peace."

Misled by his fancy that my letter was a critique upon his own, Dr. Combe repeatedly alleges, that I regard a method of treatment in accordance with Nature as equivalent to doing nothing at all. But *you* no doubt perceived that I was really inquiring whether diseases might be left to take their own way, a question naturally suggested by your original article, in which you had made a vigorous and able reclamation for the somewhat neglected *vis medicatrix*. After exposing the absurdity and nullity of the homœopathic system, you asked, how are the apparent cures under this system to be accounted for? And you concluded very reasonably, that, if the cases had got well, they had got well of themselves—not from the interposition of what Dr. Combe chooses to call "his natural treatment," but from no treatment at all. In my remarks I said, that if the statements of homœopathic success could be depended upon, they were most momentous; and the question which Dr. Combe thinks so superfluous, was inevitable. But I soon satisfied myself from an analysis of Dr. Fleischmann's table, that there was no need for any concern about this document and the possible inferences from it, and my conclusion was subsequently confirmed by Dr. Balfour's excellent Report. I then discussed the question on other grounds, and endeavoured to show, not only that Nature is often inadequate to the cure of diseases, but also that she is often doing mischief. Among the instances I adduced was that of sympathetic disorders in dentition, upon which Dr. Combe thus comments: "In teething, *Nature's efforts* are directed to effecting a passage outwards for the advancing tooth. If, from any circumstance, she proves unable for the effort, sound principle would assuredly direct us to *aid her* in it, and remove the obstacles which obstruct her progress; and I am at a loss to know what more rational or direct *aid* we could lend her in her own way, than making the opening through the gum, which she has failed to accomplish for herself." (p. 259.)

It is remarkable that men of "strong sense," but "blinded by a precon-

ceived notion," may fancy that they are dealing with facts when they are only using metaphors; for such are these phrases, "Nature's efforts," "failing to accomplish for herself," &c.—convenient enough as phrases, if we are not misled by them. Now, in this case, the practitioner who recognizes the cause of the illness and removes it, may if you please be acting "in accordance with Nature," but the plain fact is, that he knows his work and does it. As the tooth is growing upwards and the gum is not absorbed fast enough, injurious pressure is the consequence, which the surgeon's lancet removes. This unavailing "effort of Nature" is in more correct language an imperfect process of Nature, remediable by art. So with strangulated hernia, instead of saying that the surgeon "assists Nature to overcome the obstacle," it would surely be at once more correct and more simple to say that he removes it. "To act in accordance with the laws of Nature" comes then to this, that we should know the construction and action of the organism which we wish to rectify, that we should recognize the impediments, and remove them as delicately and tenderly as possible. If an engine is out of order, and the overseer goes about rashly twisting a screw here, drawing a bolt there, putting on the steam, taking it off again, in a hap-hazard fashion, we see at once that he is an ignorant bungler; but if he ascertains some particular part to be clogged or rusted, and clears away the obstacle, he acts, if you so please to term it, in accordance with the laws of the machinery. But there is no difference of *system* here. It is the difference between action founded upon knowledge, and action founded upon ignorance,—between rational therapeutics and blind empiricism.

The word *Nature* is the parent of many logomachies. Often it is a collective term, representative of all things around us, and of all their qualities and actions. It is also often used to express the causes of things being and doing as they are and do. When the mind fails to discover an efficient cause of an event, which is then an ultimate fact, we say it is the nature of the thing to be so—Nature has so ordered it; or, if we ascend to the First Cause of Nature, we say that it was the will of God that it should be so. But there is a strong instinct in the mind to interpose a power or intelligent agent in the production of events. Hence originated those hypotheses of φυσις, Archæus, anima regens, vital principle, &c., now vanished into the world of shadows. If this tendency is yielded to, philosophical investigation is continually liable to the risk of being cut short. The process of digestion may seem to be sufficiently explained by providing a *vis concoctrix*, and the complex congeries of actions in walking by a *vis ambulatrix*. If phrases of this kind are used only as provisional terms, pending the results of further inquiry, or for the mere purpose of classification, they may be very convenient; but, employed as expressions of causation, they cannot fail to do harm.

Dr. Combe, as we have seen, speaks of Nature as making efforts to do this, that, and the other; but I do not think he would say that, in teething, the epileptic paroxysms, the frightful laryngismus stridulus, and the slighter troubles of strangury and colic, are efforts of Nature. Yet they are not artificial: they are the morbid products of irritation in the cerebro-spinal axis, proceeding from the extremities of the trifacial nerve in the jaws. In strangulated hernia those terrible tormina, which add so much to the patient's sufferings, are the violent contractions of the tube, excited by its contents, which have been unduly retained in consequence of the mechanical impediment, and which become unusual stimulants to the muscular fibres, just as in the case of the bladder when some sudden stricture has occurred at the neck or in the urethra. Were there no obstacle, these "efforts," as Dr. Combe terms them, would be effectual in propagating the contents, but, as it is, they are prejudicial, and need to be quieted. But how came this obstacle? A fold of intestine slipped into the inguinal canal, the internal ring having been too patent, and some of the contents became gaseous and so distended the gut that

it could not return by the narrow orifice through which it had entered. These events resulted from the natural constitution and properties of the parts, and we might therefore say that they were efforts of Nature. But, from long-established habits of thought, we recoil from such an expression. We instinctively transfer to external nature our own motives. A man has died of a wound; we find in the main artery, which had been divided, and through which his life had gushed away, an imperfect coagulum. We say that Nature had tried to save him, but had failed. We examine the trachea of a child who died of croup. The windpipe is all but entirely blocked up by the albuminous exudation, but we do not say, "Nature very nearly succeeded in choking the poor child." On the contrary, if we find never so small a portion of false membrane detached from the mucous lining, we exclaim, "See, Nature had made an effort to save the child." Or, take the case of a patient dead of typhoid fever. Death had been immediately induced by the hemorrhage from a Peyerian ulcer; on examining one of the elliptical patches in the ileum, we find that, at the base of the ulceration there is nothing but the thin layer of serous membrane. But we do not say, "Nature had very nearly killed the patient by spontaneous perforation." On the contrary, the least flake of albumen on the peritoneal covering of the gut would be enough to make us take it as a hint of Nature's kindly intention of strengthening the part by an adhesion to an adjoining surface. Such views occur to us, partly because the sanative purpose is ever uppermost in our minds, and partly because in normal anatomy and physiology we are familiar with most wonderful and extensive provisions for the safety and well-being of the living organism.

Nature is often put in opposition to disease. A morbid formation or disordered action is termed unnatural, which means that it is a deviation from the ordinary state or course of events in Nature. Yet, to show how *relative* the term is, we have only to point out that the very same action which, under some circumstances, would be pronounced unnatural, is, under other circumstances, termed natural. Suppuration in the brain around a tuberculous deposit is intensely morbid; suppuration of the subcutaneous cellular tissue in a boil or carbuncle is the natural mode of relief. Many analogous instances might be taken from hemorrhages; in fact, whenever in the course of disease any change takes place for the better, or tends to a favorable issue, if it is not obviously due to artificial interference, it is called a work or effort of Nature—Nature being so habitually associated in our minds with all that is good and beneficent. This association is so general as to indicate, were other proofs wanting, that there is a large preponderance of good in the existing state of things, and that evil and disease are exceptional. Still, *that they do exist* the philosopher is bound to concede. Imperfection of structure, incompleteness of action, tendency to decay, and downright destruction, meet us too obviously and too often to allow of our evading the conclusion that these faults, for some wise and in many respects unknown purpose, enter into the plan of the universe, just as the happy and tender affections in the lower animals are mixed up with the ferocities of beasts of prey and with the terrors of their helpless victims. One purpose, however, appears in this arrangement:—the opportunity offered for the play of man's intellectual resources. In the triumphs of his inventive faculties and laborious exertions over the defects and hard conditions of surrounding nature, he finds his most exalted gratification. The pleasure-grounds and the conservatories of the English gentleman show beauties of fruit and flower which uncultivated Nature can nowhere set forth; and, though the imagination may see the forms of Phidias imprisoned in blocks of marble, they would have remained there for ever had not the consummate artist set them free.

I am very glad that Dr. Combe is so anxious to do away with the impres-

sion that he would advocate a do-nothing system of treatment; but I repeat that I am quite at a loss to understand in what respect the treatment which he calls *natural* differs from the practice which has been taught and pursued by every one who founds his practice on a scientific acquaintance with the human frame in health and disease. I should certainly use a different phraseology. In the case of teething, all that is active on the part of Nature is tending to evil. Instead of aiding her exertions, you do your best to repress them, and you do that, the want of which in the work of Nature has been the cause of mischief. Of the treatment of fractures Dr. Combe has very just notions, and, instead of speaking so tenderly as he does on other occasions of "assisting Nature," I am glad that he ventures to talk of "rousing" her "to successful exertions." I agree with him also in thinking that you cannot put in a cement instead of the nutritive material out of which the new bone is to be formed, for this is one of the conditions necessary to the work in hand. No more can you fashion the marble statue without the marble,—or produce fine apples without the crab-tree. But the procuring of the marble, and even a chisel to boot, is but a small step in advance towards the shaping of a Pallas Athene, and there is a long and painful interval of labour and ingenuity between a sour crab and a Ribston pippin. The exudation-cells are essential to the union of the fractured bone, but they are not all. Let the man with his broken leg lie where he fell; his leg may be mended, but when he rises it is as likely as not to be two or three inches shorter than the other. Nay, let him be laid in bed, and Nature be aided, as Dr. Combe would say, by putting the limb in a good position; yet, unless it be confined by strong splints and bandages, it is very possible that some of "Nature's efforts" may consist of spasmodic twitchings, which jerk the ends of the bone out of place. Are these to be *aided* or *corrected?*

One remark is made by Dr. Combe, which surprises me greatly. I refer to what he says about removing a patient from the influence of a morbific cause, instead of allowing the cause to be still in operation, and only striving to counteract its effects. Can Dr. Combe really believe that any members of the profession are so inveterately stupid, or so unspeakably ignorant as to act in this fashion? Is there a practitioner to be found who, if he had the choice, would give bark to a patient in ague, instead of taking him away from the malarious district, though he might still require bark afterwards? or if the thorn were within reach of his forceps, would not make haste to pluck it out, instead of applying unguents and cataplasms? If Dr. Combe meets with such practitioners, I am sorry for him, "fallen upon evil days."—Alas! for the modern Athens! the city set on a hill! the city of the prophets. And can it be that with all the traditions and records of wisdom, from the days of Cullen and Gregory, and with all the light yet radiating from Alison, Christison, Simpson, and others, there are any medical minds within those walls, sunk in a state of such "total eclipse?" If so, it must be blindness from "excess of light."

Dr. Combe repudiates the idea of advocating a negative system of treatment in acute visceral inflammations. Though I was not replying to him in my former communication, I certainly had an impression that he had spoken very lightly about pleurisy, and very favorably of poultices, and milk and water; and on referring to his first letter, I find that impression to be not very far from the truth. Now I have seen many cases in which, because the patients had neglected to seek aid soon enough, or the disease had not been early recognized, Nature had enjoyed full and free play, and a dislocated heart and a lung solidified by compression were the consequences. Yet the patients had been lying in bed, had allayed their thirst with cooling drinks, and been comforted with all sorts of hygienic prettinesses.

It is unquestionable that some twenty years ago, even acute phlegmasiæ

were often treated with needless severity, and that blood was squandered most profligately by those who fastened their faith to the lancet only. But we have outlived those sanguinary days. Thanks to the help of those grand adjuvantia, calomel, opium, and antimony, it is long very long since we have seen a case requiring the repeated venesections common at the time of our noviciate. Peculiarities of constitution and other circumstances may compel us to pretermit the measure; but let the Paris and Vienna cases be what they may, I confidently appeal to the observant practitioners of this country, whether experience has not convinced them that if they are obliged to forego an early bleeding, the future management of the case becomes one of far greater difficulty than when it has been practised. I do not hesitate to say that the patients of a young practitioner may be endangered by the infusion of scepticism into his mind, as to the value of the orthodox treatment. For my own part I have an antipathy to the shedding of blood; it is the greatest possible relief to find that a case is one that does not require this remedy. Consider then what may be the effect of remarks, that insinuate a distrust of the necessity of active depletory measures on inexperienced practitioners with the same tendency, which from observation, I believe to be a very common one. In the early period of an illness, it is perhaps only from a nice balancing of indications that the inflammatory character of the disease is decided; but the patient shrinks from the lancet, the friends are averse to it, and now the doubts in the practitioner's mind, as to whether after all a bleeding is so very important, will turn the scale, and the bleeding is deferred. Twelve or fourteen, or, in country practice, twenty-four hours slip by before the next visit. And now the necessity is obvious enough, unless indeed the opportunity has been lost altogether. Imagine the case to be one not of pleurisy, but of peritonitis, and then judge what disastrous results ensue from the loss of time. I am sure I have my practical brethren with me when I express extreme jealousy of remarks, however enlightened may be their author, which tend to unnerve rather than to brace the young combatant in these life-and-death encounters.

I cordially acknowledge the great benefits conferred by Dr. Combe on the laity in those admirable works of his, on 'Physiology applied to Health,' on 'Digestion,' and on the 'Management of Infancy.' I doubt not that thousands gratefully associate with the name of Combe the formation of habits that have enabled them to be their own conservators of health, and to go through their daily duties with ease and cheerfulness. But when Dr. Combe comes forward to lecture the profession on the importance of *studying Nature*, I beg leave to tell him that though Lord Bacon has been dead more than two centuries, he yet speaks, and in tones more intelligible and commanding than ever; that his sublime testamentary prediction has been long fulfilled; that not only did the "next ages" reverently take up his name and words, but that every succeeding age has resounded them yet louder and louder; that they are lisped by children, and syllabled by strange tongues on

"Sands, and shores, and desert wildernesses,"

and that no class of practical philosophers have more obediently remembered the words of the great master, than the followers of medicine from the days of Sydenham and Baglivi downwards. Dr. Combe has this time mistaken his mission; this is not his rôle; and we must bid him "*sui plausu gaudere theatri.*"

I remain, my dear Sir, yours very sincerely,

J. A. Symonds.

IV. THE NATURAL HISTORY AND TREATMENT OF DELIRIUM TREMENS.

BY JOHN WARE, M.D.

[Delirium tremens is one of those acute diseases, the natural progress—in other words, the Natural History of which is generally complicated and obscured, or at least rendered doubtful, by the effects of the treatment most commonly had recourse to in this country, viz. that by opium. It has been, therefore, thought a matter of practical importance to give an account of this very interesting and often dangerous malady, as it shows itself uninfluenced by treatment. The admirable Memoir* from which the following extracts are taken, was published in Boston in the year 1831; and the reader will find the principal data on which its conclusions are founded, in our Number for January, 1839, p. 268. We are by no means prepared to assert that opium is, in no case, beneficial in delirium tremens; or that it will not sometimes procure sleep when this would not ensue spontaneously; but we are certain, from personal experience, that the opiate treatment has often failed, that it has been occasionally very injurious, and that the sleep which ushered in restoration has frequently, at least, been the mere concomitant or sequent of the medicine, and not its effect. At any rate, it is an essential element in the philosophical knowledge of the pathology and treatment of every disease, to be aware of its natural course, progress, and result.]

Although delirium tremens occurs in various states of the constitution, and in various diseases, and is to be looked upon as a possible event in almost all cases of indisposition among drunkards, yet there is a remarkable similarity in the phenomena presented by the affection, and in the course of symptoms through which it passes, whatever may have been the original state of constitution or disease from which it has proceeded. Its approach is often indicated by the existence of certain symptoms from the very commencement of indisposition. It is particularly likely to take place in those who have suffered from irritability of the stomach and frequent vomiting. Indeed, it often makes its appearance after having been preceded by no other symptom of disease, and comes on as soon as the vomiting ceases. There is commonly also in the beginning of those cases in which delirium finally ensues, a tremor of the hands and limbs, and more frequently of the tongue; a tremulousness of voice producing some indistinctness of articulation; a general anxiety; a hurried manner of moving and speaking; imperfect and disturbed sleep; and startings and twitchings of the limbs. These signs are by no means infallible. They are sometimes observed where delirium does not follow. But where they exist from the very first, are not diminished by the treatment adopted, and do not leave the patient with the other symptoms of his complaint, an attack of delirium tremens may be reasonably expected.

But, on the other hand, it frequently happens that the attack is not indicated by any such symptoms in the early history of the case. The patient appears to be getting on perfectly well, and the original disease to be subsiding in a satisfactory manner, when suddenly it becomes manifest that an attack of delirium tremens is threatened. In either case, however, whether there have been any premonitory symptoms or not, the disease follows very much the same course. The patient first complains that he has not slept well, that he has been disturbed all night by unpleasant dreams, that he has been hard at work, but that matters have not gone right, and his concerns have troubled and perplexed him. During the next day, perhaps, he is tolerably comfortable, has some appetite, moves about his house or place of business; yet he is uneasy and restless, and exhibits those appearances which have been already

* Remarks on the History and Treatment of Delirium Tremens. From the Transactions of the Massachusetts Medical Society. 8vo, pp. 61.

described as indicating the approach of the disease. This continues for one or two days; each night being worse than the preceding, whilst in the day there is an increase of the anxiety, restlessness, and trembling of the limbs, tongue, and voice.

The night is then passed with only one or two short naps, from which the patient awakes with some strong impression upon his mind, of the fallacy of which it is difficult or impossible to convince him. His sleep has been filled with dreams of dangers and perplexities and annoyances, innumerable and indescribable. From this state he passes into that of complete watchfulness and delirium. The dreams of his sleeping become the fancies of his waking hours; and in his delirium he conceives himself to be engaged in the same occupations, beset by the same difficulties, and surrounded by the same dangers, that he has described as giving a character to his dreams. In fact it is difficult in many cases to point out the precise time at which the mind passes from the dominion of the conceptions which have been engendered in sleep, to that of those which are the offspring purely of the disease.

At whatever period this state of entire watchfulness and delirium begins, we are to date from it the commencement of what may be denominated *a paroxysm of delirium tremens.* Yet it will sometimes happen, that, on the morning succeeding the night, from the last continued sleep of which we are to date the commencement of the paroxysm, the patient does not exhibit any unequivocal marks of the delirium by which he is affected. The attendants inform us that he has had but little sleep, and has been very *crazy*, but we find him sufficiently rational to give an account of his feelings, and fully aware of whatever is going on about him. Still his aspect and manner are such as to convey to the mind of one accustomed to the disease, the true state of the case, even although there may be no actual exhibition of delirium during the period of the visit.

Most frequently, however, at this time there are occasional wanderings of mind, though not a continued state of delirium. Thus, while sitting by the patient, we perceive his eye become intently fixed upon some remote spot in the room, or outside a window, as if it had been suddenly caught by some remarkable object;—or he will speak in a loud and quick voice, as if making answer to some one who has addressed him from without, or from behind; or he will start up hastily from his seat or from the bed, and run to another part of the room, or to look beneath the bed, as if in pursuit of something. These impressions are, during the early part of the day, evanescent; but in the latter part the delirium returns, and becomes constant. It increases in violence till about the middle of the night, and then diminishes towards the morning.

On the morning of the second day the delirium is still complete, and is not altered in its character; but the patient is milder and more tractable than during the night. He is as fully possessed of the strange imaginations which have entered into his mind; but he is more easily influenced by his friends, and is more amenable to authority. The second night is generally worse than the first, and there is less abatement of the disease in the ensuing or third morning, and in the early part of the day; still there is some alleviation of symptoms, like that of the day before. The third day is passed much in the same way as the second; but if the disease is to have a favorable termination, the delirium of the third night is less violent than that of the preceding, and the paroxysm terminates in sleep, sometimes in the course of the evening or first part of the night, but most commonly not until the latter part of the night or in the morning. When the disease is about to terminate unfavorably, the delirium continues undiminished until the fatal event takes place.

This description has been taken from cases which were left to take their own course, uninfluenced by medicine. In all essential points it will apply to a majority of cases. Still there are many variations in the time of day at which the paroxysm begins and terminates, in its length, and in other

particulars, which cannot be included under any general account. Thus its duration is sometimes less and sometimes greater than that assigned to it. Especially it is apt to be prolonged in those who have had repeated attacks, and in one such case I have known it to extend to nearly six entire days.

During the first part of his sleep the patient is generally uneasy and restless, his breathing is irregular, and is sometimes almost like that of a person dying. During the first few hours, he often wakes once or twice, perhaps gets up and renews the exercises of his delirious state, or else takes merely a little drink, but in either case, goes soon to sleep again.

Soon after getting into a sound sleep, the breathing becomes deep, slow, and sonorous; a profuse sweat breaks out, and for a long time the whole body is bathed with it. After six or eight hours the patient awakes tolerably rational, and sensible of what is going on about him, but generally with some impression left on his mind of the imaginary scenes through which he has passed. He continues for the next twenty-four or even forty-eight hours, to sleep during the greater part of the time. At the end of that period, his restoration appears complete, so far as the peculiar symptoms of delirium tremens are concerned; for he may still be the subject of other affections which have preceded the paroxysm, and which remain after it has subsided.

Almost invariably the occurrence of sleep at the close of the paroxysm is indicative of a favorable termination. In some rare cases, however, the patient actually dies after falling asleep, particularly where sleep has been procured by opium; indeed the only cases which I have seen or known, in which the disease has terminated in this way, have been treated by large doses of opium. In such a case no peculiar symptoms indicate a different result from that which we usually promise ourselves when the patient falls asleep, till after sleep has taken place. But then, instead of gradually passing from a disturbed into a more tranquil and natural slumber, he becomes first more unquiet and restless, moans, breathes with difficulty, and falls at length into a state of complete coma, from which he never awakes.

The disease terminates fatally in several other ways. Sometimes the patient is carried off by the sudden accession of convulsions, and this event is particularly to be looked for in those cases which have begun with them. They also occur very unexpectedly in cases which promise favorably, and which have afforded no ground for anticipating them. Sometimes the patient, after continuing the violent exertions of his delirium to the very last moment, without any of the peculiar signs of approaching dissolution, falls back and expires immediately. Sometimes, during the continuance of the delirium, death comes on from the effects of some disease with which it happens to be complicated, and dissolution occurs in the same way that it would from that disease alone.

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There has been much uniformity of opinion among physicians concerning the object to the accomplishment of which the treatment is to be directed during the paroxysm. This object is the procuring of sleep. The absence of sleep is one of the most remarkable symptoms of the disease. When it terminates favorably, it terminates in sleep. It is not without foundation, therefore, that the treatment has had for its primary indication to bring about this termination. The patient, it has been emphatically said, "*must sleep or die.*" There is no doubt that this is true. But may it not have been too hastily concluded from this undeniable position, that sleep must be procured by the assistance of art, or the patient will die? It is possible that the common impression which has been produced on our minds concerning this is erroneous in two points of view: 1, We have concluded that sleep is the cause of the salutary change which takes place in the disease; and 2, that sleep, in whatever way induced, will have the same effect, and that it is therefore to be induced by artificial means.

In order to determine, concerning any disease, what influence our remedies actually exert upon it, we must first ascertain what will be its course and termination if suffered to go through its usual series of changes without the interference of art. This is a point in the history of diseases to which reference should always be had in deciding upon the principles, or calculating the efficacy of the treatment to be employed. This is particularly desirable in those diseases which, like that now under consideration, have but recently become the subjects of medical observation and inquiry.

I have witnessed a considerable number of cases of delirium tremens, in which the patient, after the establishment of the paroxysm, has been left to contend with it, without the administration of any remedy whose tendency was to cut it short, or in any decided way to modify its symptoms. The active treatment has been confined to the period of indisposition preceding the paroxysm; and after its accession articles of a negative character alone were administered, with the exception sometimes of purgatives. The result has uniformly been, that the disease has gone through that regular course, which has been already described in the former part of this paper, and has terminated in the manner there described, at a period seldom less than sixty or more than seventy-two hours from the commencement of the paroxysm.

The termination in these cases has also been almost uniformly favorable, except where there has been a combination of the delirium with some acute disease in itself dangerous, or where it has appeared in connexion with some fatal chronic malady. This course has been pursued, I do not mean to say without any deviation, but without any deviation which I believe to have essentially affected the result, in about fifty cases of the several classes which have been described; and although several deaths have taken place among them, none are recorded, except among cases, which I have arranged, whether justly or not, in the third and fourth classes.*

It may be stated, in confirmation of the opinion now expressed concerning the natural tendency of the paroxysm to terminate in a spontaneous and salutary sleep at the end of a certain period, that, even in the reports of cases which have been submitted to the public as evidences of the efficacy of various modes of practice, sleep has not actually taken place sooner than it would have done in the natural course of the disease, if the history which has now been given of it be founded on correct observation. In the cases which I have formerly treated with opium, and which have at last terminated well, a salutary sleep has not actually taken place till toward the close of the third day, let the quantity of opium be what it would. I have indeed seen sleep induced by opium at an earlier period, but it was premature; it passed into a state of coma, and the patient died.

I am satisfied, therefore, that in cases of delirium tremens, the patient, so far as the paroxysm alone is concerned, should be left to the resources of his own system, particularly that no attempt should be made to force sleep by any of the remedies which are usually supposed to have that tendency; more particularly that this should not be attempted by the use of opium. I do not undertake to say that it can be never right to administer opium for the removal of the paroxysm itself, but I believe it can be rarely necessary, and I have not yet seen a case in which I think that it was.

* Dr. Ware classes his cases as follows:

1. As the disease occurs as an immediate consequence of a particular excess or excesses in persons not otherwise disposed to disease.

2. As the result of habitual intemperance, without any particular or extraordinary excess.

3. As the disease occurs in connexion with other regularly-formed diseases or as the consequence of injuries, and still having the natural tendency to end in sleep.

4. An irregular form, of a recurrent or less permanent type, not ending in sleep, coming on in the course of other diseases.—Ed.

V. REMARKS ON THE DISEASES OF VIENNA, AND ON THE COMPARATIVE EFFECTS OF THE HOMŒOPATHIC, HEROIC, AND EXPECTANT OR SIMPLE METHODS OF TREATMENT.

BY GEORGE W. BALFOUR, M.D.

(*In a Letter to John Forbes, M.D., F.R.S.*)

Corstorphine, March 13, 1847.

MY DEAR SIR,—You will oblige me by giving a place in your Journal to the few following observations, which I intend as partly explanatory of some statements in my Report (Br. and For. Med. Rev, No. XLIV), and partly as a reply to objections that have been made to some of these statements by your own correspondents and by others. My animadversions on the Homœopathic treatment would be much more effective if I could vouch for the accuracy of a statement recently made to me—which I myself believe, but cannot positively assert to be true—viz., that in the Wiesen district hospital of Vienna, a physician has been treating all his patients with *aqua colorata* only, and, on being accused of doing so, and therefore threatened with dismissal, has retained his situation by producing his books and showing better therapeutic statistics than his colleagues!

Peritonitis.—The startling results obtained by Fleischmann in peritonitis, caused me to make it the subject of strict inquiry, and I found that its idiopathic and too fatal form, so frequent here, was unknown, or at least very rare in Vienna. There it occurs as a subacute, tubercular inflammation usually of circumscribed extent. Such cases I have seen recover under the use of *extract. graminis*, and, although no cases occurred during my visits to the Homœopathic Hospital, I have no doubt that the infinitesimals would be equally efficacious. The disease is truly peritonitis, but of so peculiar character that there cannot be a more flagrant instance of the deceptive nature of the statistics of names, than to compare it with ours.

Gout. This disease, or, as it is called, "die gichtische Dyscrasie," as evidenced by symptoms of portal congestion, is exceedingly common, and exercises its modifying influence on many diseases. Its recognition in relation to those of the eye, forms, indeed, one of the peculiarities of the Vienna Oculistic School. The prevalent use of fermented liquors, wine, and beer, and greasy sauces, are looked upon as its causes. It is at best a hypothetical erratic gout, and comparison with ours leads but to misconception.

Bronchitis. This is a comparatively rare disease in Vienna, and, compared with the frequency of pneumonia, it is most strikingly so.

I could name some other peculiarities of the Vienna class of diseases; no less striking, and still less easily explicable. Thus, during the ten years preceding 1845, we have in Fleischmann's tables, as published in the Austrian Journal of Homœopathy, 4 inflammations of the aorta, all cured; 2 of inflammation of the spinal cord, cured; 3 of ulceration of the stomach, discharged unimproved, and 2 cured; 4 cases of white swelling of the knee-joint, cured.

Intermittent fever. Of the 30 patients who had intermittent fever and were treated by Fleischmann, the average number of attacks was 4·7; while under Skoda's treatment, the average number could not be more than 3, and was most probably under this. The number of his patients was, I believe, at least as great as Fleischmann's—if not greater; his cases were as severe; the patients were afflicted with the same epidemic, during the same season, inhabited the same town,—many of them the same quarter—and belonged to a similar rank in life. They were treated in undoubtedly less advantageous circumstances with respect to food and lodging—yet all were cured. In no

one case was the number of attacks more than 4: and in none did the fever recur while they continued under observation: they were therefore cured speedily, safely, and effectually. If it be true that none but homœopathic remedies are capable of effecting this, then Skoda's remedies must have been homœopathic—but they were not so, were not infinitesimal.

Were homœopathy true, it would be very extraordinary that a physician practising empirically like Skoda, should have so simply and effectually cut short the disease, while a scientific homœopathist of experience, with his carefully and scientifically selected array of China, Quinin, Ipecacuanha, Nux vomica, Arsenic, and Aconite, was unable to do so. It is still more singular that, although it is evident from the results of Skoda's practice that Quinin was indicated in the greater number of cases of this epidemic, it fared no better with those of the homœopathic patients who got this remedy infinitesimally than with the others.

In corroboration of the obvious inference from these facts, we might quote the confessions of the homœopathists themselves, or of those who have been reconverted to allopathy. Thus, Kopp states that this disease can neither be certainly, quickly, nor pleasantly cured by the infinitesimals. Nay, he quotes avowed homœopaths, Gross, Rummel, Œgidi, and Hauptmann, to the same effect. It must be admitted, however, that while acknowledging this, and in certain cases flying to allopathic doses of quinine, these gentlemen look upon their non-success as merely an imperfection of the youth of their system, and hope yet to discover the true, i. e. homœopathic, specific for ague. Gross expressly states that ordinary homœopathic remedies only cured ague exceptionally, and that antipsoric remedies alone fulfilled his expectations. But, as Kopp says, this is too bad to submit the poor patients to a method of treatment which allows of the renewal of the dose only after 4 or 6 weeks.* Kopp also tells us as the result of his own experience (s. 184), that even where the remedy (China) was homœopathically indicated, and given in infinitesimal (Hahnemann'scher) doses, it had no influence over the appearance or diminution of the intermitting fever; while, on the contrary, when given in substance in sufficient dose (as sulphate of quinine, 2 grs.), it speedily cut the fever short. I think there can be, therefore, but one opinion as to which method of treatment for this disease is the preferable. But what are we to think of a system whose very foundation-stone is so unstable? For, as is well known, it was the action of cinchona that first led Hahnemann to think of his new theory.

Dysentery. In my Report under the head of dysentery, one case will be found in which the amelioration of the symptoms was curiously coincident with the administration of the Homœopathic remedies. I say coincident, because in the first case, which greatly resembled it, the same remedy was of no avail; and we also know that however medical men have been accustomed to regard the disease as demanding the most active treatment, more especially in warm climates, we know that many cases of dysentery may, nay do, recover without it. I may refer for an illustration of this fact to the case of a patient, a physician, as narrated by himself. Away from all friends and without medicine, he was seized with dysentery while voyaging on the Nile in an open boat; after a partial recovery he relapsed, and using nothing but hot fomentations finally recovered, after about a week's illness, and this during continued exposure. He adds, "during a precisely similar attack in India, I got 216 grs. calomel, was bled at the arm, had 50 leeches applied, and used mercurial frictions; in spite of this heroic treatment I recovered, but in such a state as to require 6-8 week's furlough to recruit."†

Again, Dr. Smith ‡ mentions the case of a Frenchman who cured himself of

* Denkwürdigkeiten in der ärztlichen praxis, s. 290.

† Cumming's Notes of a Wanderer in Search of Health, p. 277.

‡ On the Diseases of Peru, Edinb. Med. and Surg. Journal, vol. lvi.

a dysentery after 13 relapses, by semistarvation; and of a medical man who trusted to this alone for the cure of his patients, and found it undoubtedly successful in many cases, though in others it was too reducing. There can, therefore, be no doubt that dysentery *may* recover without any active treatment whatever. Skoda's treatment consists in giving injections of tincture of opium or sulphate of zinc, and, if these fail, of corrosive sublimate, which he looks upon as the *ne plus ultra.* In corroboration of the occasional usefulness of these stimulating injections, Dr. Smith (loc. cit.) mentions the case of a boy who, instead of a little rum and water as a drink, got a whole syringeful of pure pale rum per anum. The boy became intoxicated, fell asleep, and awoke quite recovered from a dysentery of some months' standing. In Skoda's hands these injections prove very successful.

Pneumonia. In the cases of pneumonia observed by me in the Homœopathic Hospital, there were one or two coincident cures; in most, however, the symptoms were either aggravated or run their course, or merged in others, apparently uninfluenced by the remedies. I question not the homœopathic correctness of the selection of the remedies, for most of the cases were treated with *Phosphor.*, and Fleischmann has said, that any pneumonia incurable by Phosphor., is as yet incurable homœopathically, and other homœopaths have corroborated this. Who am I, therefore—heretic, as I avow myself—that I should gainsay this deliberate opinion of the initiated, founded on the experience of years? In regard to the aggravations, I acquit the remedies of all blame in their production, just as I deny them all merit in the ameliorations and eventual cure. Surely it is much more natural and consonant with reason to believe that these and all the other cures hitherto of the homœopathists are natural cures—effects of the too-much neglected *vis medicatrix.* And I will even go so far as to doubt whether Fleischmann himself will not here agree with me; for I have heard him say that, out of 1000 cases treated, he did not believe that above 200 were *cured;* the rest got well. And where is the proof that even these 200 did not also get well? Were not even the most remarkable cures merely coincidences? Every quack medicine, however ridiculous or inert, either in an allopathic or homœopathic point of view, has also, as everybody knows, its numerous extraordinary cures, or coincidences. It would be remarkable if homœopathy had none. Even under Skoda's treatment such occur; yet, according to our ideas, his treatment is equivalent to nothing; according to the homœopathists, it must be worse than nothing. And yet, as we have seen, the patients recovered perfectly, aye, and speedily,—the average duration of treatment in acute cases being under a fortnight, about 9 to 10 days. A few cases of a chronic character remained in the state of hepatization for weeks, but these had been generally some time ill previous to admission. The most remarkable fact with respect to the pneumonic patients treated without active interference is, that however much they may suffer for a time, on the disease breaking, the recovery is extremely rapid, insomuch that the general health seems almost as strong in a day or two as it was before the attack: there is no tedious convalescence. This remark applies both to those treated by Skoda, and by the homœopathists. The completeness of the recovery was ascertained in one by dissection. The patient, a woman, died a week or two after dismissal from a heart complaint under which she had long laboured. Both of her lungs were found perfectly healthy, and permeable by air in every direction, although one had been completely hepatized.

Skoda does not, however, always trust to Nature. When the dyspnœa is very great at the commencement of pneumonia, or pleurisy, he bleeds, but never after hepatization or effusion has taken place; as he believes that abstraction of blood lessens the chance of a speedy and complete absorption of

the exudation, which of itself will relieve this symptom. If the cyanosis be accompanied by a secretion of tough mucus, an emetic is given.

In reference to the relative mortality and cure of pneumonia in different situations, and under various methods, the following rough approximations, taken from data most at hand, will be admitted to be of some importance:

The admissions and deaths from pneumonia in 1842, in the undermentioned hospitals, were as follows: Edinburgh, 42 a. 16 d.; Aberdeen, 10 a. 3d.; Dumfries, 10 a. 2 d.; Glasgow, 33 a. 9 d.; Dundee, 27 a. 4 d.; Inverness, 2 a. 0 d; Perth, 1 a. 1 d.; total 125 admitted and 35 dead—28 per cent., or about 1 in 3·54.* This is a very large proportion of deaths, but a certain part, perhaps a large part, of this is due to other causes than the treatment, such as the age of the patients, their previous privations and intemperate life, and, it may be, the existence of chronic disease.

From the statistical reports on the health of the army, it appears that the admissions and deaths from pneumonia in Britain and our temperate colonies, have been as follows: United Kingdom, 657 a. 37 d., or 1 in 18; Gibraltar, 2515 a. 56 d., or 1 in 45; Malta, 1370 a. 44 d., or 1 in 31; Ionian Islands, 2186 a. 81 d., or 1 in 27; Nova Scotia and New Brunswick, 1505 a. 56 d., or 1 in 27; Canada, 2774 a. 99 d., or 1 in 28; Cape of Good Hope, 673 a. 22 d., or 1 in 31. The average age of the men may be taken at from 25 to 30. Their condition is, of course, superior to the ordinary run of our hospital patients, though perhaps their diathesis is therefore all the more "phlogistic," and, though not free from the effect of intemperance, they must be generally free from chronic complications. Their station and manner of life is not superior to that of Fleischmann's patients, yet his extended statistics are inferior to any of the above. During 10 years he treated 347 pneumonic patients. with 20 deaths—5·70 per cent., or 1 in 17·35., and this, it will be understood, among patients of a younger class than in most of the other cases. Where, then, is the boasted superiority of homœopathic treatment? Again, if we take Skoda's average of the last three years, viz. 392 admissions and 54 deaths, i. e. 13·7 per cent., or 1 in 7·26, to be a sample of what can be done with ordinary unpicked hospital patients, by a little less active interference and trusting more to Nature, and find that we have a reduction of the mortality to one half, may we not expect the results to be still more astonishingly favorable among patients in a manner picked with respect to age and constitution, I demand once more, and in a bolder tone, where is the boasted superiority of homœopathic treatment?

I will not occupy your space with any extended observations in defence of the general inferences in my Report, as they have been attacked by the homœopathists. It is hopeless to make any impression on them. Before concluding, I must, however, say a few words on the present aspect of homœopathy, as confirmed by the opinions and practice of some of its own advocates. There is, indeed, confusion and war in the very camp. The old original doctrine of Hahnemann is virtually abandoned by all. The new theories are legion. The holders of the one theory are ruthlessly falling foul of those of the others, the ultra-infinitesimalists are warring with the more rational dose-givers; the "spezifikers" are rejected by "those who have more deeply studied the subject."†

The only thing they seem agreed upon, is the necessity of *proving* the medicines and choosing their remedies according to the principle of similitude. But they who will be at the trouble to examine the records of these provings, will see how much absurdity is mingled with a little sense, and will also perceive how difficult in most cases must have been the choice of a remedy had not the

* Vide a paper by Mr. Thomson, Edinb. Med. and Surg. Journal, No. 158.

† Vide Madden, Homœop. and Med. Ref., &c. &c.

despised allopathy supplied them with her experience. And they who will take courage—it does not require much—and *prove* the medicines on themselves, as I have done, will see how little is to be dreaded from infinitesimal doses; and how little is to be trusted to a system which substitutes mere nervous feelings constantly occurring without the use of drugs, for manifestations of the physiological action of those drugs.

I consider that the extension of homœopathy to the veterinary branch of medicine furnishes a very excellent proof of its inutility, in so far as the indications for the use of the remedies are taken from the results of provings on man, and many of these, particularly vegetable ones, have actions so different in kind and degree on animals from what they have on man; not but that there, as in human medicine, it will prove strikingly useful, because there, even more than in human medicine, the heroic system is carried too far. A little more attention to the events of the stable might suffice to convince the homœopathists of the utility of active treatment when skilfully and judiciously employed, as well as of its injurious influence when the contrary is the case. Here, at least, the influence of imagination is wanting; the action of the drugs, remedial or not, is developed simply, and uncomplicated by mental influence.

To conclude, while I believe that physicians have, to a certain extent, been deceiving themselves for years—centuries shall I say—as to the efficacy of many drugs, have been masking symptoms by heroic treatment, and often falsely believing they were thereby curing diseases, I believe, at the same time, that homœopathists are now deceiving themselves infinitely more—are allowing Nature to kill or cure as she pleases—are walking up to the first part of Chomel's golden axiom, that "the first law of therapeutics is not to do harm," and turning their backs on the equally important second law, which commands them "to do good," and which, so long as they remain homœopathists, in the present, or even in any sense of the word, they are for ever shut out from doing.

I remain, my dear Sir, yours truly,

George W. Balfour.

P.S. The following errata have crept into my last letter:

Page 567, line 2d from bottom, *between* practicable *and* as, *insert* certainly requisite.

Page 577, line 22d from bottom, *instead of* for three weeks he had a regular fever, *read* three weeks previously he had had one or two attacks of regular fever.

Page 584, line 25th from top, *before* elsewhere, *insert* over dull spaces—respiration clear—bronchial.

THE LONDON AND PROVINCIAL MEDICAL DIRECTORY. 1847.

In the present edition of this very useful work, we have the Directory for the Country, conjoined with that for London. We think this union highly proper; as the profession is one, wherever the members may be living. This being the first year in which the names of the provincial practitioners are enrolled, the part of the Directory devoted to them is much less perfect than the other. We doubt not, however, from the manifest zeal of the editors, that next year will show a great amendment in this respect There is an *addition* we would recommend to the notice of the editors, which, we think, would be a very great improvement; it is—to give an alphabetical list of the *provincial towns*, with the surnames of all the medical men resident in each. This would not require much space, as a line or two would suffice for each place; e. g. "Manchester: *Bardsley, Black, Turner, Noble, &c, &c.*" Many must feel that it would often be a great convenience to know the names and qualifications of all the practitioners in any one town.

The plan proposed would effect this readily; as all the names under the head of any town could be successively referred to, in their alphabetical series, in the body of the work.

DR. ASHWELL'S TREATISE ON THE DISEASES OF WOMEN.

To the Editor of the British and Foreign Medical Review.

Sir,—Having read in the January Number of the Review your notice of my Second Edition, I beg your insertion of the subjoined statement.

You observed that there is an identity of the second and first impressions of my work 'On the Diseases of Women,' in all except the First Part—(208 pages.)

The explanation is a very simple one. The work appeared (for my convenience) in Parts, one thousand of each having been printed. Now, as a long time elapsed between the publication of the first and third portions, it was found, when the whole of the complete books had been sold, that there were left on hand about 250 copies of the third, and somewhat less of the second parts.

At this time, as there was not a copy in the trade, Mr. Highley suggested, to prevent the book being long out of print, that the first part, comprising 208 pages, should be rewritten, and by this means a small second edition would be obtained, and the demand for the work temporarily supplied. I beg to thank the profession for their good opinion of the book, and to assure them that no pains shall be spared to make it worthy their continued approval.

I am, Sir, yours respectfully,

Samuel Ashwell.

Grafton-street, March 22d, 1847.

BOOKS RECEIVED FOR REVIEW.

1. Remarks on Medical Organization and Reform. By E. Lee, Surgeon. Lond. 1846, pp. 121.

2. Transactions of the Medical and Physical Society of Bombay, for the Year 1844. No. VII. Bombay, 1844. 8vo, pp. 206.

3. Het Speeksel uit een Physiologisch, Diagnostisch en Therapeutisch oogpunt beschouwd. Door S. Wright, M.D. Naar de Koogduitsche Bearbeiding. Van Dr. S. Eckstein. Amersfoort, 1846. 8vo, pp. 220.

4. Memoria sobre el Cuerpo de Salud Militar, seguida de un Projecto de Reglamento del Mismo. Por Pedro Vanderlinden, M.D. Mexico, 1845. 8vo, pp. 48.

5. Reglamento del Cuerpo Medico-militar, espedido en virtud del decreto de 12 de Febrero, 1846. Mesuco, 1846.

6. Der Vaccinprocess und seine Crisen. Von A. F. Zöhrer. Wien, 1846. 8vo, pp. 213.

7. Pathologische Studien. Door Dr. C. Gobée. Eerste Deel, 1843. Tweede Deel, 1844. Utrecht. 8vo, pp. 237, 279.

8. Opere di Maurizio Bufalini, Professore della Clinica Medica. Vol. I. Parte I, II. Firenze, 1844. 8vo, pp. 337, 1846.

9. Elementi di Pathologia umana di Luigi Bosi. Tomo primo, 1843. Tomo secundo, 1844. Ferrara, 1843-4. 8vo, pp. 449-516.

10. Nuovi Elementi di Medicini eclettica del Dott. Niccolo Celle. Pisa, 1841. 8vo, pp. 487.

11. Homœopathy viewed in connexion with Medical Reform. By H. R. Madden, M.D. Lond. 1846. 8vo, pp. 168.

12. Chemistry and Physics in relation to Physiology and Pathology. By Baron Liebig, M.D. F.R.S. London, 1846. 8vo, pp. 116.

13. Guy's Hospital Reports. Second Series. Vol. IV. London, 1846. 8vo, p. 498.

14. A Guide for the proper Treatment of the Teeth. By W. K. Bridgman, Dentist. London, 1846. 12mo, p. 88.

15. The Moral Aspects of Medical Life, consisting of the 'Akesios' of Professor Marx; with Biographical Notices, &c. By James Mackness, M.D. London, 1846. 8vo, pp. 348.

16. Chemistry of the Four Seasons. By T. Griffiths. London, 1846. 8vo, pp. 495.

17. Practical Observations and Suggestions in Medicine. Second Series. By Marshall Hall, M.D. London, 1846. 8vo, pp. 360.

18. Practical Remarks on Near Sight, Aged Sight, and Impaired Vision. By W. W. Cooper, Surgeon. London, 1847. 8vo, pp. 216.

19. Observations on Hydropathy; with an Account of the principal Cold-Water Establishments in Germany. By J. S. Buchnan, M.D. Berlin and London, 1846. 8vo, pp. 170.

20. Liebig's Question to Mulder tested by Morality and Science. By Dr. G. T. Mulder, Professor of Chemistry in the University of Utrecht. Translated by Dr. P. T. H. Fromberg. Edinburgh, 1846. 8vo, pp. 124.

21. The Motives to Industry on the Study of Medicine; an Address delivered at St. Bartholomew's Hospital. By James Paget, F.R.C.S. Lond. 1846. 8vo. pp. 30.

22. The Potato Plant, its Uses and Properties; together with the cause of the present Malady. By A. Smee, F.R.S. London, 1846. 8vo, pp. 174.

23. Animal Chemistry, or Chemistry in its Applications to Physiology and Pathology. By Baron Liebig. Edited from the Author's MS., by W. Gregory, M.D. Third edition. Part I. Lond. 1846. 8vo, pp. 253.

24. The Stars and the Earth; or Thoughts upon Space, Time, and Eternity. London, 1846. 12mo, pp. 48.

25. On the Correlation of Physical Forces. By W. R. Grove, Esq., M.A. F.R.S. London, 1846. 8vo, pp. 52.

26. On Medical Education; a Lecture delivered at King's College. By W. A. Guy, M.B. London, 1846. 8vo, pp. 64.

27. Lectures and Observations on Clinical Surgery. By And. Ellis. Dublin, 1846. 8vo, pp. 275.

28. Lectures on Clinical Medicine. Diseases of the Heart. By P. M. Latham, M.D. Vol. II. London, 1846. 8vo.

29. Elements of Chemistry. By Thos. Graham, F.R.S. Second edition, entirely revised. Part I. London, Nov. 1846. 8vo, pp. 160.

30. The Pathological Anatomy of the Human Body. By Julius Vogel, M.D. Translated by G. E. Day, M.A. L.M. Cantab. London, 1847. 8vo, pp. 585. With ten plates.

30. An Essay on the Tongue, in Functional Derangement of the Stomach and Bowels, and on the appropriate Treatment. By E. Williams, M.D., Physician to the Essex and Colchester Hospital. Second edition, carefully rewritten, with much additional matter. London, 1846. 8vo, pp. 236.

31. Handbook of Human Anatomy, general, special, and topographical. Translated from the German of Dr. A. Von Behr, and adapted to the use of the English Student. By John Birkett, Surgeon, Demonstrator of Anatomy at Guy's Hospital. London, 1846. 8vo, pp. 457.

32. The Climate of Torquay. By E. Vivian, Esq. London, 1846. 8vo.

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34. Principles of Human Physiology. By W. B. Carpenter, M.D. F.R.S. Third edition. Lond. 1846. 8vo, pp. 776.

35. Knowledge; an Introductory Address delivered at the Bristol Athenæum, Oct. 5, 1846. By J. A. Symonds, M.D. Lond. 1846. 8vo, pp. 47.

36. A Manual of the Principles and Practice of Ophthalmic Medicine and Surgery. By T. Wharton Jones, F.R.S. London, 1847. 8vo, pp. 570.

37. Hygiea. Medicinsk och Pharmaceutisk

Mändas-Skrift. Redactörer: Dr. Carlson, Sonden-Berg, Huss, Retzius. Bandet 1844-5-6. Stockholm.

38. Svenska Läkare-Sällskapetts nya Handlingar. Bandet I, II, III, IV, V. Stockholm, 1837-8; 1843-4-5. 8vo.

39. Om Bright'ska Njursjukdomen. Akademisk Afhandling Af Pehr Henrik Malmsten, M.D. Stockholm, 1842. 8vo, pp. 120.

40. Ueber die Bright'sche Nierenkrankheit. Von Peter H. Malmsten. Uebersetzt von G. Von dem Busch, M.D. Bremen, 1846.

41. On Diseases of the Skin. By Erasmus Wilson, F.R.S. Second edition; with eight coloured plates London, 1847. 8vo, pp. 482.

42. Homöopathie, Allopathie, und die neue Schule. Von John Forbes, M.D. Bearbeilet Von Adolph Bawr, M.D. Wien, 1846. 8vo, pp. 70.

43. A Treatise on the Plague; more especially on the Police Management of that Disease. By A. White, M.D., Deputy Inspector-General of Military Hospitals. London, 1847. 8vo, pp. 342.

44. The India Journal of Medical and Physical Science, for 1845. Twelve numbers. Edited by C. Finch, M.D. Calcutta, 1845.

45. Clinical Facts and Reflections; also Remarks on the Impunity of Murder in some Cases of presumed Insanity. By Thomas Mayo, M.D. F.R.S. London, 1847. 8vo, pp. 217.

46. A descriptive Catalogue of the Anatomical Museum of St. Bartholomew's Hospital. Vol. I. Pathological Anatomy. Lond. 1846. 8vo, pp. 487.

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50. The Microscopic Anatomy of the Human Body. By A. H. Hassall. Part III. Oct. 1846.

51. On the Pathology and Treament of Scrofula; being the Fothergillian Prize Essay for 1846. By R. M. Glover, M.D., Lecturer in the Newcastle Medical School. London, 1844. 8vo, pp. 315.

52. Researches into the Physical History of Mankind. By J. C. Prichard, M.D. F.R.S., &c. Vol. 5, concluding the work. London, 1847. 8vo, pp. 570.

53. Lectures on the Comparative Anatomy and Physiology of the Vertebrate Animals. By Richard Owen, F.R.S., &c. Part I. London, 1846. 8vo, pp. 808.

54. Elements of Chemistry. By the late Edward Turner, M.D., &c. Eighth edition. Edited by Baron Liebig and W. Gregory, M.D., &c. Part I. Inorganic Chemistry. London, 1847. 8vo, pp. 676.

55. Practical Observations on some of the Diseases of the Stomach and Alimentary Canal. By James Alderson, M.D. F.R.S. London, 1847. 8vo, pp. 215.

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63. A Treatise on the Inhalation of the Vapour of Ether, for the prevention of Pain in Surgical Operations, &c. By J. Robinson, Surgeon-Dentist. London, 1847. 8vo.

64. The London and Provincial Medical Directory, 1847. London, 8vo, pp. 862.

65. Andeutungen über Ventilation. Eine Anrede an gebildete Nutaerzte. Von E. Sieveking, M.D. Homburg, 1846. 8vo, pp. 69.

66. Proceedings of the Lincoln Lunatic Asylum; and Communications with her Majesty's Commissioners in Lunacy. London, 1847. 8vo, pp. 97.

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69. On Cataract, Artificial Pupil, and Strabismus. By F. H. Brett, Esq. M.D. London, 1847 8vo, pp 89.

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73. A Treatise on Diet and Regimen. By W. H. Robertson, M.D. Part I.

74. Gray's Supplement to the Pharmacopœia. Rewritten and enlarged by Thomas Redwood. London, 1847. 8vo, pp. 1118.

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G. AND J. ADLARD, PRINTERS, BARTHOLOMEW CLOSE